

# The Michell-Cavendish experiment

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In 1960 I set up a torsion balance experiment to measure gravitational attraction, based on the principle of the well-known 'Cavendish experiment' ('Travelling Hopefully Towards  $G$ ', *S.S.R.*, 1962, **44**, 58). I have found the subject an interesting one for research, both scientific and historical. In this article I have outlined the known facts about the original experiment.

Cavendish published his work in the *Philosophical Transactions of the Royal Society*, 1798, **88**, 469. This paper begins: 'Many years ago, the late Rev. John Michell, of this Society, contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death, the apparatus came to the Rev. Francis John Hyde Wollaston, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me.' Cavendish added: '. . . as there were some parts of it which I thought not so convenient as could be wished, I chose to make the greatest part of it afresh.'

Michell, after distinguishing himself as a mathematician and scientist at Cambridge, became the Rector of Thornhill, near Dewsbury in Yorkshire, in 1767, and remained there until his death in 1793. Thornhill was thus the birthplace of the experiment. Cavendish made clear his debt to Michell, whose name deserves a place in any reference to the work. It seems that we should credit him, together with Coulomb, with the discovery of the torsion balance principle. Cavendish said: 'Mr. Michell informed me of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. Coulomb's experiments.' Sir Edward Thorpe, who edited Cavendish's papers in 1920, stated that the experiment '. . . was based upon a principle which he (Michell) had suggested and used as far back as 1768.'

Michell's body lies in an unmarked grave in the chancel of Thornhill Church. A memorial tablet high up in the tower refers to his Christian virtues, but not to his scientific achievements, which are, however, noted in the Church guide.

Practically the only source of information about the experiment is Cavendish's *Phil. Trans.* paper. I have examined the Cavendish manuscripts in the possession of the Duke of Devonshire at Chatsworth, but they contain no reference to the work. At the request of the Royal Society, the experiment was repeated in 1840 by Francis Baily, the astronomer, whose work is described in great detail taking up the whole of Vol. 14 of the *Memoirs of the Royal Astronomical Society* for 1843. This account yields a few more pieces of information, such as some structural details not provided by Cavendish. Baily spent a great deal of time on his experiment, but he did not, in fact, produce a more dependable result.

The original apparatus has not survived. Baily said: 'Cavendish's apparatus belongs to the Royal Institution, of whom I borrowed it for the purpose of having copies of its several parts made for my experiments.' But a search of records at the Royal Institution has yielded no reference to the apparatus.

The experiment was performed at Cavendish's house on Clapham Common. The house stood at the south-west corner of the junction of the present Cavendish Road with Clapham Common South Side. There were extensive grounds stretching alongside Cavendish Road (then called Dragmore, or Dragmire, Lane).

Cavendish did not give the exact location of the experiment. He simply said: '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.'

Baily said: 'Cavendish chose an out-house in his garden at Clapham Common, and having constructed his apparatus within the building, he moved the masses by means of ropes passing through a hole in the wall and observed the motions of the torsion rod by means of a telescope fixed in an ante-room on the outside.' Poynting, in his book *The Mean Density of the Earth*, published in 1895, said: 'He selected for the experiment, according to Baily, an out-house in his garden, and within this he appears to have constructed an inner chamber to contain the apparatus.' As Cavendish wished to obtain good thermal insulation it seems likely that he would enclose the apparatus in an inner chamber within a building, rather than simply setting it up in an 'out-house'. Cavendish's diagram shows a chamber about 10 ft wide and 8 ft high. He referred to a door, but gave no indication of the other dimension.

I have found an old map, 'Batten's Plan of Clapham' dated 1827 (ten years after Cavendish's death), which shows in the grounds of his house a building about 56 ft by 28 ft with its length nearly east-west, which is at an angle to the general lines of the house and grounds (see Plate). This is significant, because Cavendish said of his apparatus, when discussing a possible magnetic effect: 'Now, as it happened, the case in which the arm was inclosed was placed nearly parallel to the magnetic east and west.' It seems unlikely that the large chamber containing the apparatus would be placed at an angle to the building

enclosing it, so that it is highly probable that the building on Batten's Plan was the actual site of the experiment. I cannot find any earlier map to confirm the existence of this building in Cavendish's time.

Cavendish performed experiments with the apparatus on 17 days in August and September 1797 and April and May 1798. These dates suggest the use of an unheated building. This is also suggested by Cavendish's reference to the weight of the deal torsion arm when dry and '... when very damp, as it commonly was during the experiments.' A contemporary book, *Records of the*

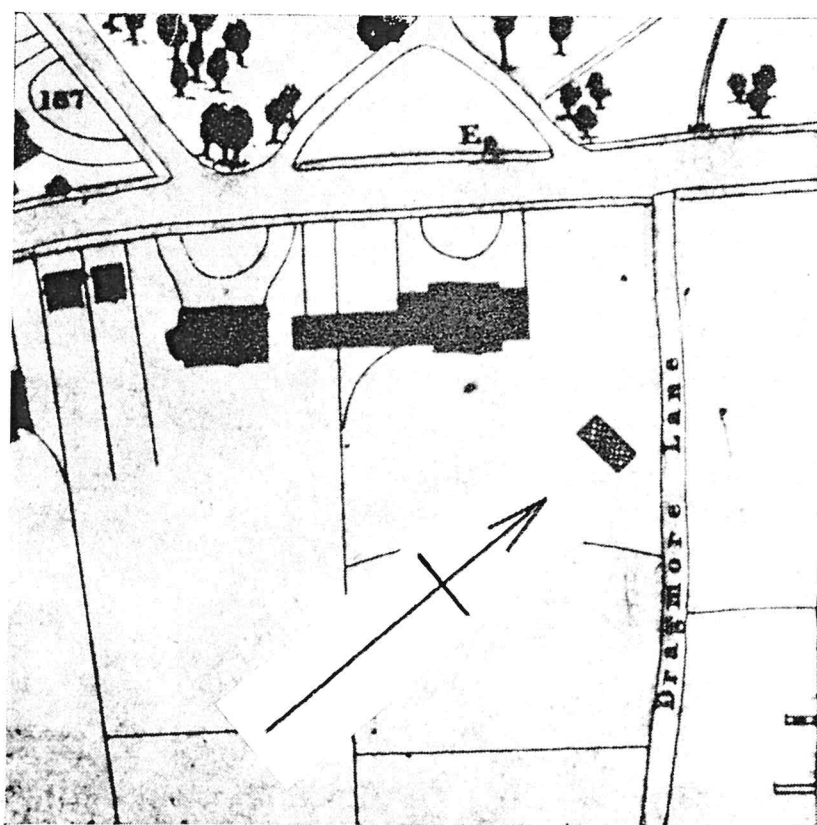


Plate 1. Portion of Batten's Plan of Clapham (1827). In the middle is Cavendish House and its grounds. The arrow points due north and towards the building which was probably the site of the experiment

*Seasons*, remarked: 'The summer of 1797 was in every respect the most melancholy we ever recollect. We had almost constant rain from April to Christmas.' The figures given by Cavendish show an increase of 3.5 per cent. in weight due to the 'very damp' conditions. I have found that a specimen of deal left for a long time in saturated water vapour at room temperature increased in weight by 17 per cent.

After the reference to Michell's part in the work, Cavendish's paper continues: 'The apparatus is very simple; it consists of a wooden arm, 6 ft long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position, by a slender wire 40 inches long, and to each extremity is hung a leaden ball, about 2 inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.'

Fig. 1 shows some details of the suspension, the middle section of the

torsion arm and the screening box. Particulars of the support *HR* were not given by Cavendish: these details have been taken from diagrams of Baily's apparatus which he stated to be, in this respect, a copy of the original.

Fig. 2 shows the end view of the torsion arm. The small masses hung in downward projections of the screening box, and their movement was observed through windows in the ends of the screening box by telescopes (see Fig. 3).

Fig. 3 is a general view which has been slightly adapted from Cavendish's

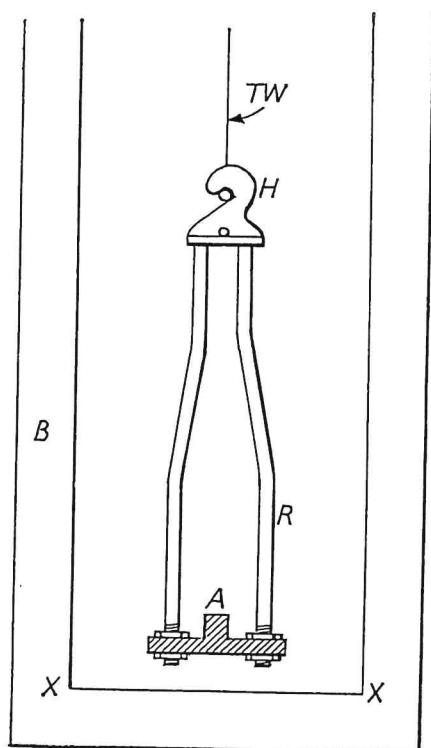


Fig. 1. Section through middle of torsion arm

Scale: XX = 3.6 in

- B: Mahogany screening box 0.75 in thick.  
 TW: Torsion wire.  
 H: Double hook which fitted over a rod clamped to the end of the torsion wire.  
 R: Brass rods which carried the torsion arm.  
 A: Torsion arm middle section of area 0.33 in<sup>2</sup>: this figure was given by Cavendish; he showed the shape of the section but did not give the separate dimensions. The wider portion tapered steadily towards each end to give the rectangular section shown in Fig. 2.  
 XX: Level of screening box surface shown in Fig. 2.

original diagram. *FF* is the bottom line of the original. This would have made the position of the telescopes inconvenient for the lengthy observations required. I have shown what I consider to have been the actual floor level, with a suitably placed table and chair for the observer. The original showed a lamp pointing downwards above each telescope. But there was not room for the observer's head below the lamp, and I think it probable that the lamps were to one side of the telescopes.

Of the torsion wire, Cavendish said: 'In my first experiments, the wire by which the arm was suspended was  $39\frac{1}{4}$  in. long, and was of copper silvered, one foot of which weighed 2.4 grains: its stiffness was such as to make the

arm perform a vibration in about 15 min. I immediately found, indeed, that it was not stiff enough, as the attraction of the weights drew the balls so much aside, as to make them touch the sides of the case; I, however, chose to make some experiments with it, before I changed it.' These figures give a diameter for the first wire of about 0.027 cm. The stiffer wire, with which most of the experiments were made, gave a period of about half that of the first wire. The length and weight were not given, but, if the length is assumed to have been

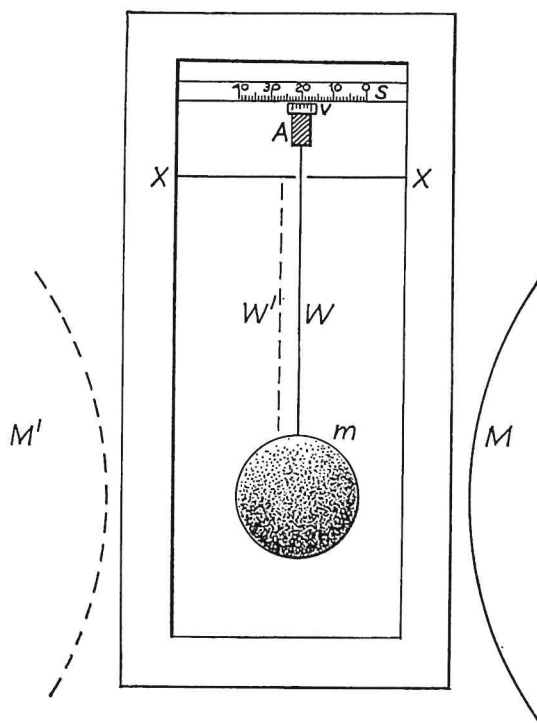


Fig. 2. End view of torsion arm

Scale: XX = 3.6 in

- m: Lead sphere: diameter about 2 in: mass = 11,262 grains (729.8 g).  
M: Lead sphere: diameter about 12 in: mass =  $2.439 \times 10^6$  grains ( $1.581 \times 10^5$  g).  
W: Wire supporting m.  
S: Fixed scale: 1 division = 0.05 in.  
V: Vernier on torsion arm: reading observed by telescope was direct to 0.2 scale division, and could be estimated to 0.05 scale division.  
A: End of torsion arm: area = 0.146 in<sup>2</sup>.  
XX: Screening box surface below torsion arm: see Fig. 1.  
WW': Displacement when large masses were reversed to position M': about 0.3 in. The centres of the large masses were 8.85 in from the middle line of the apparatus when in the position M or M'.

the same, calculation shows that the diameter must have been about 0.038 cm (which is about 28 s.w.g.).

The 'times of vibration' given by Cavendish for the two torsion wires were just under 15 min and just over 7 min, but examination of his calculations shows that these figures are half of what we now mean by the 'period of vibration'. The meaning of 'one vibration' appears to have changed with the years.

The times at which the torsion arm reached certain scale readings were given to the nearest second. The method of timing was not stated: presumably it was by counting the ticks of a pendulum clock while watching the arm.

Readings of torsion arm position do not appear to have been taken at both ends simultaneously, so, for this purpose, a single telescope and scale would have sufficed, as the controls, *CM* and *CA*, could have been at the same end. Cavendish did not mention any assistant, except that he noted before the last

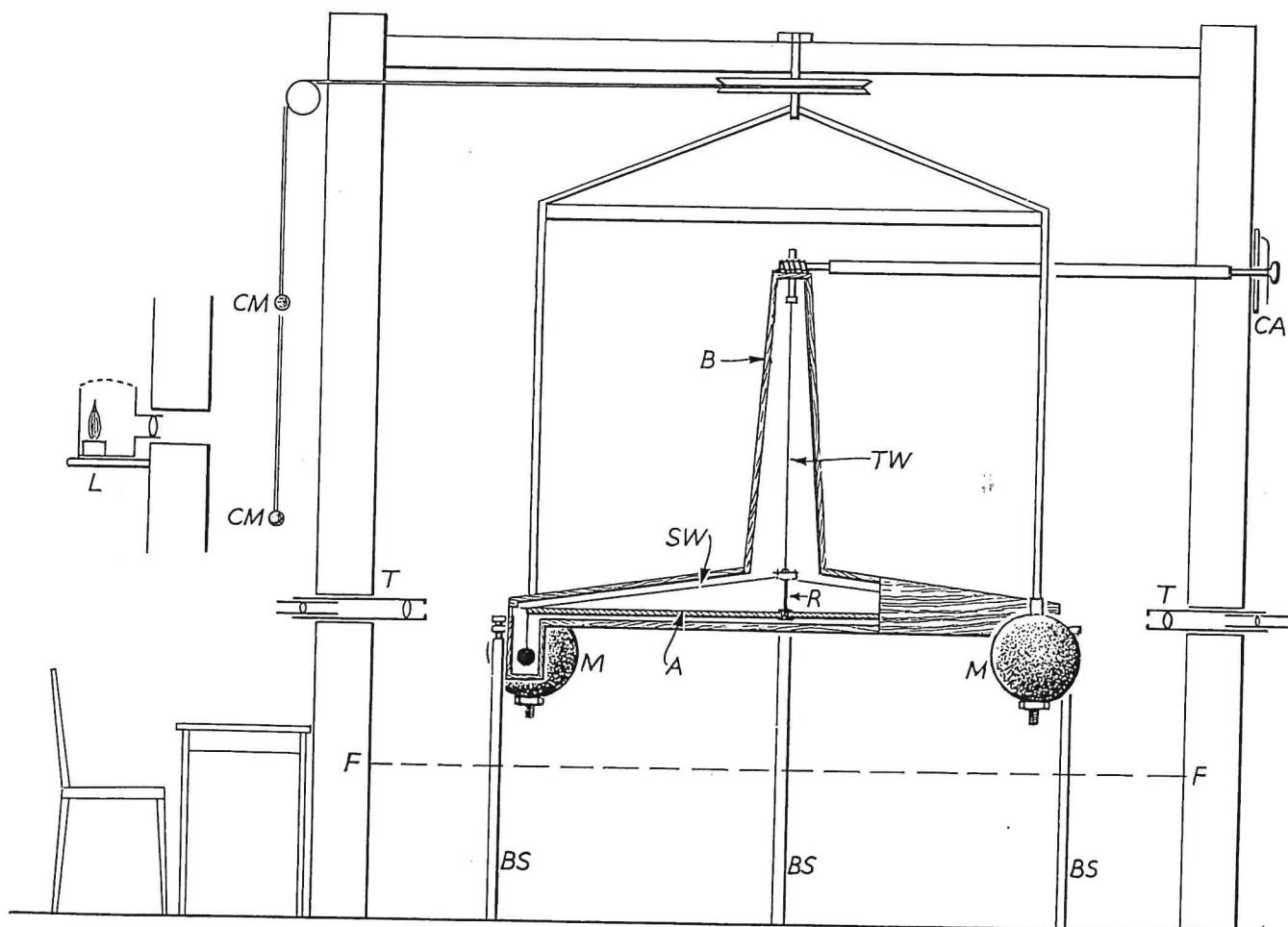


Fig. 3. General view

Scale: FF = 9 ft 9 in

- R: Brass rods; see Fig. 1.  
 A: Deal torsion arm: length 73.3 in: mass = 2400 grains (155.5 g). From each end projected a vernier: mass = 45 grains (2.9 g): centre of gravity 1.35 in from the end.  
 SW: Supporting wire to strengthen A: silver: total mass = 170 grains (11.0 g).  
 TW: Torsion wire.  
 CA: Control to rotate torsion arm.  
 B: Screening box completely enclosing torsion wire, torsion arm and small masses.  
 BS: Four screening box supports (two in the middle).  
 M: Large masses hanging by copper rods from a framework which could be rotated by controls CM. These masses were brought to rest 0.2 in from the screening box by stops (not shown) attached to the walls of the room. The distance between the centres of the large masses was 73.3 in, the same as between the centres of the small masses, so that the attracting forces were not normal to the torsion arm.  
 T: Telescopes to read the verniers. The thickness of the wall, 9 in, is that of two ordinary bricks.  
 L: Lamp throwing a beam of light through the wall near each telescope to illuminate the vernier.

two experiments: 'The two following experiments were made by Mr. Gilpin, who was so good as to assist me on the occasion.' (Mr. Gilpin was the Secretary of the Royal Society.)

The determination of the gravitational constant requires the measurement of the masses and apparatus dimensions given above, the period of vibration

of the torsion arm and the displacement of the arm when the attracting masses are reversed. (In addition, to find the mean density of the earth, which was the form in which Cavendish gave his result, the diameter of the earth is required.) The most difficult measurement is that of the torsion arm displacement. When the attracting masses are reversed the torsion arm starts to swing, and will continue to oscillate about the displaced position for some hours. If a static measurement is to be made of the displacement it cannot be made until at least twelve hours have elapsed. Such a delay could not have mattered to Cavendish, an eccentric recluse, but he chose to calculate the rest position from observations made as soon as possible after moving the large masses. He explained: '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.'

Many textbooks suggest that the torsion arm was always in a state of oscillation, owing to convection currents, so that the rest position could never be determined directly but could only be deduced. The results show that this is untrue, as in a number of cases there was no appreciable movement of the torsion arm for half an hour before moving the large masses. In two cases Cavendish deliberately set the arm oscillating before starting the experiment, though he did not explain why. He did not quote any figures for the static torsion arm position over a long period, though I know how instructive such observations can be. But it was the slow change in torsion arm position over many hours, and from one day to the next, which convinced him of the need to complete a displacement determination as rapidly as possible.

The new torsion arm rest position can be found by observing its position at the ends of three successive swings. Suppose that the scale reading of the torsion arm when initially at rest is  $a$ . When the attracting masses are moved the arm will commence a damped oscillation about the new rest position. The first swing carries the arm to  $b$  on the scale, then back to  $c$ , then over to  $d$  etc. If these swings have a uniform decrement, then

$$\frac{a - b}{c - b} = \frac{c - b}{c - d} = \dots = \delta$$

Cavendish did not use  $a$  in calculating the new rest position as an appreciable time is taken in moving the attracting masses, so that the initial swing is not true S.H.M. He found the rest position by calculating the mean of  $b$  and  $d$  and the mean of this figure and  $c$ , i.e. he took the rest position to be at a scale reading of  $\frac{1}{2}[c + \frac{1}{2}(b + d)]$ . The arm displacement is thus

$$= a - \frac{1}{2}[c + \frac{1}{2}(b + d)].$$

It can be shown that this is an approximation with an error of  $\frac{(\delta - 1)^2}{4\delta^3}$  in 1.

In the experiments with the thicker wire the average value of  $\delta$  was 1.075,

giving an error of 1 in 1,000 which was less than the smallest observable change in position, so that the approximation was justified. In the first three experiments in which a thinner wire was used  $\delta$  was about 1.20, giving an error of 6 in 1,000. The smallest observable change was 3.3 in 1,000 of the displacement, so in this case the approximation introduced an appreciable error.

The simple theory of the experiment equates the torsional couple in the wire to that produced by the attractions of a large mass on a small mass at each end of the torsion arm. The torsional couple per radian twist of the wire is related to the period of oscillation and the moment of inertia of the suspended system. Cavendish considered a number of corrections to this simple theory.

(1) Attraction of the more distant small mass by each large mass decreased the couple acting by the factor 0.9983.

(2) Attraction by the copper rods which supported the large masses increased the couple by the factor 1.0077.

(3) Attraction of the torsion arm by the large masses increased the couple by the factor 1.0139.

(4) The effect of attraction by the shielding box was shown to be negligible.

The combined effect of these corrections was that the couple acting was increased by the factor 1.0199.

Cavendish did not use the term 'moment of inertia', but calculated what mass placed in the position of the small mass would have the same 'resistance to motion' as the various parts of the suspended system. His mathematical argument proceeded by a consideration of ratios, instead of by the use of the algebraic equations with which we are more familiar.

Most of the experiments gave more than one determination, as the large masses (or 'weights') were moved more than once. The positions of the large masses are denoted by  $m$  for 'midway' (i.e. in a position equidistant from the small masses), + giving an increase of scale reading and - giving a decrease.

Experiments 1, 2 and 3 were made with the thinner wire. To prevent the torsion arm striking the screening box the swing of the torsion arm had to be restricted by moving the large masses three times in making a change.

The mean density of the earth is given without units, i.e. it is the density relative to water. The result is calculated from the expression derived by Cavendish when all corrections had been applied, which was  $N^2/10844 B$ .

$N$  sec was the time of vibration of the torsion arm in the absence of the attracting forces (marked  $m$  in the table).  $B$  scale divisions was the displacement (or 'motion') of the torsion arm provided that the small masses were on the middle line of the apparatus when the attracting forces were acting. In general this was not so, and the required value of  $B$  ('motion of arm corrected') had to be calculated from the observed value. Cavendish showed that the same formula would apply, to the degree of accuracy required, if  $N$  sec was the time of vibration in the presence of the attracting forces (marked + or - in the table) and  $B$  scale divisions was the observed value ('motion of arm').

His method of observing can be understood by considering one experiment in detail.

Experiment 14. May 26th, 1798

(1)	<i>Weights in negative position</i>						
	<i>Extreme points</i>	<i>Divisions</i>	<i>Time</i>			<i>Point of rest</i>	<i>Time of middle of vibration</i>
			<i>h</i>	<i>m</i>	<i>s</i>		<i>h m s</i>
		16·1	9	18	0		
		16·1		24	0		
(2)		16·1		46	0		
		16·1		49	0	16·1	
	<i>Weights moved to positive position</i>						
(3)	27·7						
(4)	{	23	10	0	46	22·37	10 1 1
(5)		22		1	16		
(6)	{	22		7	58	22·50	8 5
(7)		23		8	27		
	27·2	23		15	2		15 9
		22		15	32		
	18·3					22·65	
	26·8					22·75	
	19·1					22·85	
	26·4					22·97	
		23		43	40		43 32
		22		44	22		
(8)	20·0	22		49	53	23·15	50 41
	{	23		50	37		
		26·2					

The weights were then returned to the negative position and a similar set of readings was taken.

#### Notes

- (1) The negative position of the weights (i.e. the large masses) was that of  $M$  in Fig. 2.
- (2) The arm had been at rest for 31 min.
- (3) For reasons given previously the point of rest was not calculated here: it would have been  $\frac{1}{2}[27·7 + \frac{1}{2}(16·1 + 17·3)] = 22·20$ .
- (4) The 'middle of vibration' was at  $\frac{1}{2}(27·7 + 17·3) = 22·5$ . This was half way between the observations at 23 and 22, so its time was half way between those observed, i.e. at 10h 1m 1s. The 'middle of vibration' (which was not the same as the 'point of rest') had to be judged from the previous swing so that points for observation could be taken on either side.
- (5) First point of rest calculated at  $\frac{1}{2}[17·3 + \frac{1}{2}(27·2 + 27·7)] = 22·37(5)$ . The second decimal place was not fully justified by the accuracy of observation, which was correct to 0·05. The displacement of the arm on moving the weights was  $22·37 - 16·10 = 6·27$ .
- (6) The 'middle of vibration' was at 22·25, which was 0·25 of 29 sec = 7·25 sec away from 7 min 58 sec, giving 8 min 5 sec. Cavendish gave a detailed justification of this method

of timing for the accuracy required. It gave in each case a time a little earlier than that of the 'point of rest': the difference of two such times had only a very small error.

- (7) The 'point of rest' was moving and continued to do so.
- (8) The 'middle of vibration' was at 23.1, which was outside the range of observation, but its time was found as 50 min 37 sec + 0.1 of 44 sec = 50 min 41 sec. The time of seven vibrations was thus 50 min 41 sec - 1 min 1 sec = 49 min 40 sec, giving an average time of vibration of 7 min 6 sec. This is what we should now call a period of vibration of 14 min 12 sec.

The results of all the experiments are given in the following table, taken from the original article.

Date	Expt. No.	Movement of weights	Motion of arm	Motion of arm corrected	Time of vibration	Time of vibration corrected	Density of the earth
					m s	m s	
1797							
Aug. 5	1	m to + + to m	14.32 14.10	13.42 13.17	14 55(m)		5.50 5.61
Aug. 6	2	m to + + to m	15.87 15.45	14.69 14.14	14 42(m)		4.88 5.07
Aug. 7	3	+ to m m to +	15.22 14.50	13.56 13.28	14 39(m) 14 54(+)		5.26 5.55
Aug. 12	4	m to + + to - - to +	3.10 6.18 5.92	2.95	7 1(-) 7 3(+)	6 54	5.36 5.29 5.58
Aug. 20	5	+ to - - to +	5.90 5.98		7 5(-) 7 5(+)		5.65 5.57
Sept. 6	6	m to - - to +	3.03 5.90	2.90 5.71			5.53 5.62
Sept. 18	7	m to - - to +	3.15 6.10	3.03 5.90	7 4 (by mean)	6 57	5.29 5.44
Sept. 23	8	m to - - to +	3.13 5.72	3.00 5.54			5.34 5.79
1798							
April 29	9	+ to -	6.32		6 58(-)		5.10
May 5	10	+ to -	6.15		6 59(-)		5.27
May 6	11	+ to -	6.07		7 1(-)		5.39
May 9	12	- to +	6.09		7 3(+)		5.42
May 25	13	- to + + to -	6.12 5.97		7 6(+) 7 7(-)		5.47 5.63
May 26	14	- to + + to -	6.27 6.13		7 6(+) 7 6(-)		5.34 5.46
May 27	15	- to +	6.34		7 7(+)		5.30
May 28	16	- to +	6.10		7 16(+)		5.75
May 30	17	- to + + to -	5.78 5.64		7 2(+) 7 3(-)		5.68 5.85
							Av.: 5.48

The results of Experiments 1 and 2, and the first determination of 3, have been found using the observed 'midway' time of vibration and the corrected arm motion. In the second determination of Experiment 3 there appears to be an error, as the time of vibration of 14 min 54 sec in the + position has been combined with the corrected arm motion of 13.28 to give the density of 5.55. The arm motion used should have been the observed value of 14.50, which gives a density of 5.08.

In Experiment 4 the corrected time of vibration of 6 min 54 sec has been obtained from the observed time of 7 min 1 sec in the - position, and this has been combined with the corrected arm motion to give the density 5.36. For the rest of Experiment 4, and Experiments 5 and 9 to 17, the result has been obtained from the observed time of vibration and the observed arm motion.

In Experiments 6, 7 and 8 the time of vibration was not observed: Cavendish's figure of 7 min 4 sec ('by mean') was taken from the average results of Experiments 4 and 5. Both observed and corrected values are given: the results for the density can be obtained from either.

There is another error in the table: Baily pointed out that the average density is 5.448 and not 5.48. If the corrected value for the second part of Experiment 3 is used (5.08 instead of 5.55) the overall average is reduced from 5.448 to 5.432, or, as 3 decimal places cannot be justified, from 5.45 to 5.43. The value obtained by Boys with his much more accurate torsion balance was 5.53. Cavendish's own estimate of the accuracy of his experiment was cautious. He suggested that the error would not be more than  $\frac{1}{14}$  of the mean result: this would put his result between 5.04 and 5.82. He evidently did not regard the work as completed, as he referred to: '. . . a defect which I intend to rectify in some future experiments.' There is no evidence that he ever did any more work on the subject. Baily commented: 'Cavendish's object in drawing up the Memoir appears to have been more for the purpose of exhibiting a specimen of what he considered to be an excellent method, than of deducing a result which should lay claim to the full confidence of the scientific world.'

Finally I shall refer to a feature of Cavendish's results which I have found to be an interesting subject of research. After some comments on his first three experiments, he wrote: 'But there is a circumstance in them, the reason of which does not readily appear, namely, that the effect of the attraction seems to increase, for half an hour, or an hour, after the motion of the weights; as it may be observed, that in all three experiments, the mean position kept increasing for that time, after moving the weights to the positive position; and kept decreasing after moving them from the positive to the midway position.' This effect appeared in every case, and has been noted in the results of Experiment 14. Cavendish examined the effect in detail and concluded that it was due to small temperature differences. But he did not attempt to estimate the possible error of his results due to this cause: this I hope to do in another paper.