Great Experiments in Physics.
M. H. SHAMOS
Holt, Rinehart and Winston. N.Y. 1959.
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Henry Cavendish

1731-1810

The Law of Gravitation

MORE THAN a century elapsed between Newton's publication of his law of universal gravitation and its experimental proof. Not that physicists held serious doubts about the truth of his hypothesis; its agreement with astronomical data left little question of its validity. Nevertheless, as in all other new concepts in physics, *direct* experimental evidence was required: a measurement, in the laboratory, of the force of attraction between two masses. It is not surprising that the experiment was performed only after Newton's time; the technical difficulties were much too formidable. The force between any two masses of convenient size, such as could be employed in a laboratory investigation, is extremely minute, demanding great skill in its measurement. There was another reason why such an experiment was considered important: if the gravitational force between two known masses[1] were measured, the mass of the earth, and hence its density, would follow directly from such data.

The individual responsible for the first successful measurement of the gravitational force pioneered as well in other areas, notably chemistry and electricity, although he left unpublished most of his investigations in electricity. One of the wealthiest men of his day, he lived in virtual seclusion, devoting his entire life to science. Henry Cavendish was born on October 10, 1731, apparently at Nice (where his mother had gone for her health), the first son of Lord Charles Cavendish, himself an experimenter of some note. Details of his earliest education are lacking, but one would assume that he was tutored privately. When he was eleven he became a pupil of

[1] As determined by weighing.

the Rev. Dr. Newcombe, master of Hackney Seminary, and in 1749 entered Peterhouse College, Cambridge. At that time, Cambridge was not yet the center of scientific inquiry for which it later became so well known. In England science was, for the most part, privately supported, either by individuals or through the Royal Society. The great universities, Cambridge and Oxford, were noted for the liberal education they provide but lacked the tradition of scholarly research. Not until the middle of the nineteenth century did these universities become the focal points of scientific research in England. It is not surprising, therefore, to find science in the eighteenth century practiced in many cases by nonacademic persons, and even by those with little or no formal preparation.

Cavendish left Cambridge after three years without completing work for his degree and took up residence in London. His secluded life and the obscurity which surrounds his activities make it difficult to determine what led him into science. At any rate he appears to have taken a great interest in mathematics and experimental science. His earliest investigations were in chemistry and heat, but he published nothing until 1766, when he sent to the Royal Society a paper on *Factitious Airs.*[2] There followed many other papers on chemical investigations, climaxed in 1781 by his discovery that hydrogen (which he obtained by dissolving metals in dilute sulphuric acid) and oxygen, when burned together, formed water. He showed, furthermore, that the weight of water produced was equal to that of the gases that disappeared. Despite these advances, the combustion process was poorly understood; the *phlogiston theory*[3] was widely held, and even Cavendish referred to hydrogen as *phlogiston* or *inflammable air* and oxygen as *dephlogisticated air.* He is generally regarded also as the discoverer of nitric acid, produced in his combustion chamber.

His investigation of the composition of water led him into a dispute over prior discovery. Cavendish did not publish his *Experiments on Air* until 1783. In the meanwhile Joseph Priestly (1733–1804) had made a somewhat similar observation in 1781, unknown to Cavendish in as much as his retiring nature kept him out of touch with his scientific colleagues. James Watt (1736–1819), claiming ignorance of Priestly's experiments, proposed in 1783 that water was composed of dephlogisticated and inflammable air. For a time there was lively debate on the subject; the final conclusion was that Cavendish and Watt had conducted much the same investigations at about the same time, and came to similar conclusions. Had Cavendish been more communicative, probably the dispute would not have developed.

[2] Evidently he meant gases in the form of chemical compounds. These gases could be separated from alkaline substances by solution in acids.

[3] Based on the theory that air was the only gaseous element; *phlogiston* was that which escaped during combustion.

Prior to his researches on the composition of water, Cavendish spent several years investigating electrical phenomena, but he published only two papers on the subject, in 1772 and 1776. Much later, when Maxwell edited a volume[4] of the unpublished papers of Cavendish, it appeared that the most significant of his results had not been put into print, but that he had anticipated many of the phenomena later discovered by Michael Faraday (1791–1867) and others.[5] Evidently Cavendish carried out these investigations mainly to satisfy his own curiousity and saw no compelling reason to publish his results.

Regarding his measurement of the density of the earth, there is nothing to indicate how Cavendish became interested in the problem, except that he had some interest in the torsion balance, which was the instrument employed in the experiment, and had discussed the problem with the Reverend Michell. He conducted a remarkably detailed series of measurements and obtained a result within one percent of that presently accepted.

Cavendish continued his investigations in various fields until his death in 1810, when he left behind a considerable fortune, allowed to accumulate during his lifetime, and a large stack of manuscripts which attested to his diverse interests and capabilities. The latter were suitably recognized when Cambridge University, late in the nineteenth century, named its new Cavendish Laboratory for him.

The following extract, published in the *Philosophical Transactions*, vol. 17 (1798), page 469, describes his measurement of the density of the earth.

[4] Maxwell, J. C., *The Electrical Researches of the Hon. Henry Cavendish* (Cambridge, Eng.: Cambridge University, 1879).

[5] He recognized that the inverse square law of electric force could be deduced from the fact that charge resides on the surface of a body, a principle later discovered by Coulomb.

Cavendish's Experiment

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

[■] The Royal Society.

for making experiments with it, in the manner he could wish, was so good as to give it to me.

The arm was constructed in the form of a truss.

The apparatus is very simple; it consists of a wooden arm, 6 feet 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.

As no more force is required to make this arm turn round on its center, than what is necessary to twist the suspending wire, it is plain, that if 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. The weights which Mr. Michell intended to use were 8 inches in diameter. One of these was to be placed on one side of the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and, when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and, consequently, half the difference of these positions would show how much the arm was drawn aside by the attraction of the weights.

That is, the torsion constant of the apparatus is required.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space.■ This Mr. Michell intended to do, by putting the arm in motion, and observing the time of its vibrations, from which it may easily be computed.*

Mr. Michell had prepared two wooden stands, on which the leaden weights were to be supported, and pushed forwards, till they came almost in contact with the case; but he seems to have intended to move them by hand.

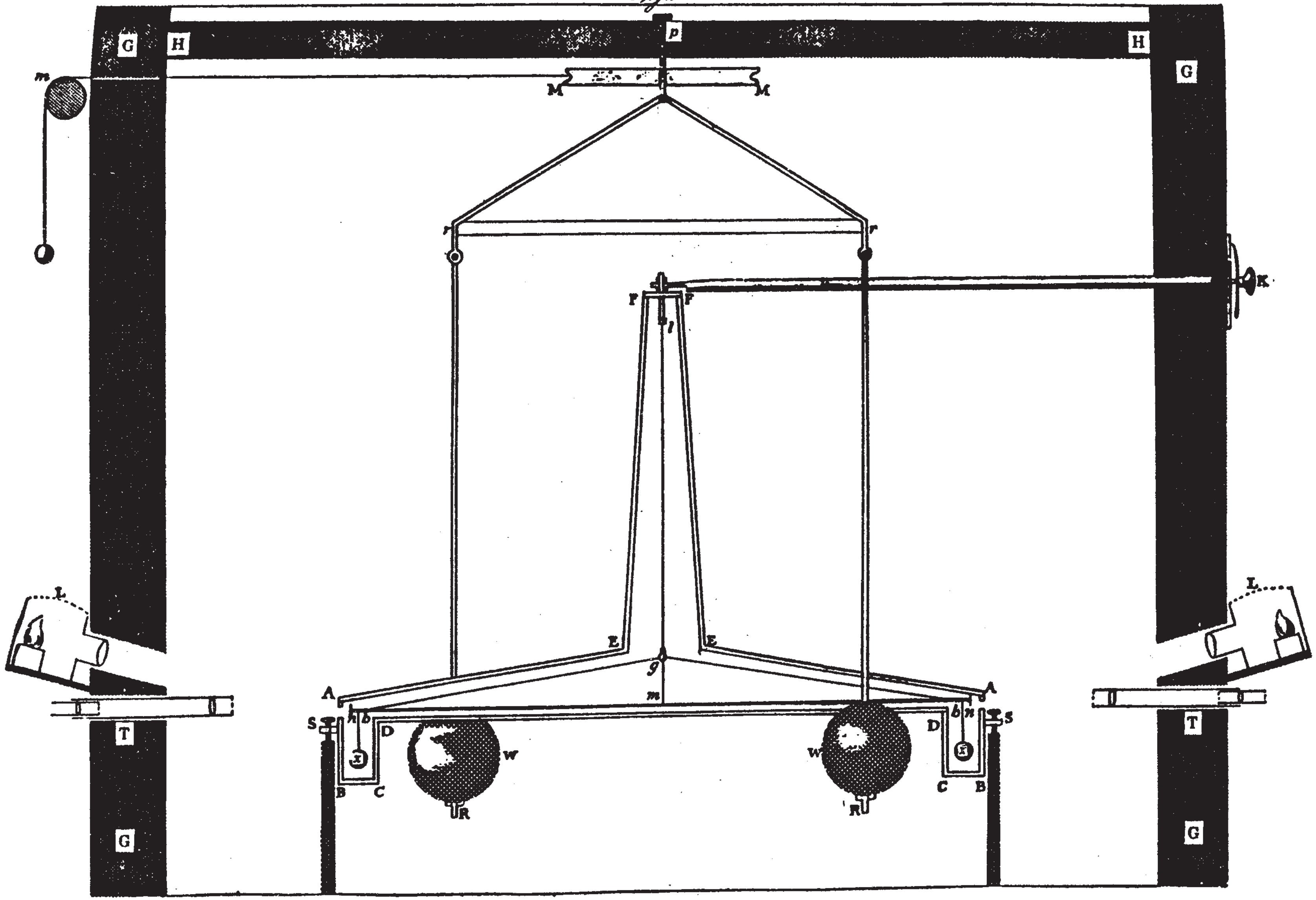
Note the absence of scientific notation, which did not come into common usage until much later.

As the force with which the balls are attracted by these weights is excessively minute, not more than $\frac{1}{50,000,000}$■ 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

In his experiments on the laws of electric and magnetic force.

* Mr. Coulomb has, in a variety of cases, used a contrivance of this kind for trying small attractions;■ but 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.

Fig. 1



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, 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 manner, that I could move them without entering into the room. This difference in the manner of observing, rendered it necessary to make some alteration in Mr. Michell's apparatus; and, 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.

Figure 1 is a longitudinal 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 *bx* from the arm *gbmb*, which is itself suspended by the slender wire *gl*. This arm consists of a slender deal■ rod *bmb*, strengthened by a silver wire *bgb*; by which means it is made strong enough to support the balls, though very light.*

Deal: fir or pine.

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*; the two others are not represented, to avoid confusion. *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 wooden bar *rr*, from the center pin *Pp*. This pin passes through a hole in the beam *HH*, perpendicularly over the center 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,

† M. Cassini, in observing the variation compass placed by him in the observatory (which was constructed so as to make very minute changes of position visible, and in which the needle was suspended by a silk thread), found that standing near the box, in order to observe, drew the needle sensibly aside; which I have no doubt was caused by this current of air. It must be observed, that his compass box was of metal, which transmits heat faster than wood, and also was many inches deep; both which causes served to increase the current of air. To diminish the effect of this current, it is by all means advisable to make the box, in which the needle plays, not much deeper than is necessary to prevent the needle from striking against the top and bottom.

* Mr. Michell's rod was entirely of wood, and was much stronger and stiffer than this, though not much heavier; but, as it had warped when it came to me, I chose to make another, and preferred this form, partly as being easier to construct and meeting with less resistance from the air, and partly because, from its being of a less complicated form, I could more easily compute how much it was attracted by the weights.

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.

Fig. 2

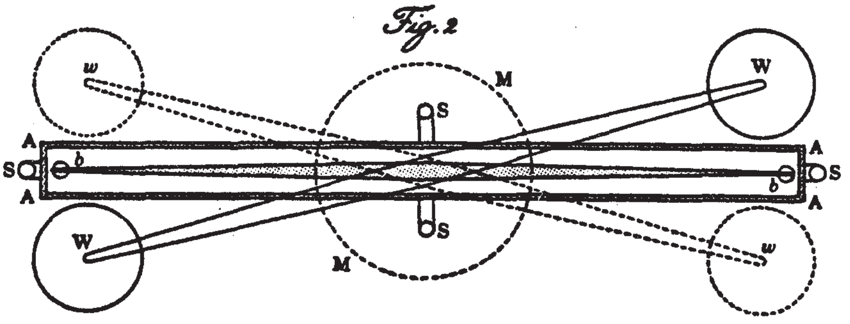


Figure 2 is a plan of the instrument. *AAAA* is the case. *SSSS*, the four screws for supporting it; *bb*, the arm and balls. *W* and *W*, the weights. *MM*, the pulley for moving them. When the weights are in this position, both conspire in drawing the arm in the direction *bW*; but, when they are removed to the situation *w* and *w*, represented by the dotted lines, both conspire in drawing the arm in the contrary direction *bw*. These weights are prevented from striking the instrument, by pieces of wood, which stop them as soon as they come within 1/5 of an inch of the case. The pieces of wood are fastened to the wall of the building; and I find that the weights may strike them with considerable force, without sensibly shaking the instrument.

In order to determine the situation of the arm, slips of ivory are placed within the case, as near to each end of the arm as can be done without danger of touching it, and are divided to 20ths of an inch. Another small slip of ivory is placed at each end of the arm, serving as a vernier, and subdividing these divisions into five parts; so that the position of the arm may be observed with ease to 100ths of an inch, and may be estimated to less. These divisions are viewed, by means of the short telescopes *T* and *T* (Fig. 1.) through slits cut in the end of the case, and stopped with glass; they are enlightened by the lamps *L* and *L*,■ with convex glasses, placed so as to throw the light on the divisions; no other light being admitted into the room.

■ Which burned lamp oil. The diagram shows the tops of the lamp housings to be ventilated.

The divisions on the slips of ivory run in the direction *Ww* (Fig. 2.) so that, when the weights are placed in the positions *w* and *w*, represented by the dotted circles, the arm is drawn aside, in such direction as to make the index point to a higher number on the slips of ivory; for which reason, I call this the positive position of the weights.

FK (Fig. 1.) is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *gl* is fastened, and thereby enables the observer to turn round the wire, till the arm settles in the middle of the case, without danger of touching either side. The wire *gl* is fastened to its support at top and to the center of the arm at bottom, by brass clips, in which it is pinched by screws.

In these two figures, the different parts are drawn nearly in the proper proportion to each other, and on a scale of one to thirteen.

Before I proceed to the account of the experiments, it will be proper to say something of the manner of observing. Suppose the arm to be at rest, and its position to be observed, let the weights be then moved, the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is 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.■ To do this, I observe three successive extreme points of a vibration, and take the mean between the first and third of these points, as the extreme point of vibration in one direction, and then assume the mean between this and the second extreme, as the point of rest; for, as the vibrations are continually diminishing, it is evident, that the mean between two extreme points will not give the true point of rest.

In the same way that the resting point of a chemical balance, for example, is determined by the *method of swings*.

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.

Apparently what was meant here is that the disturbing forces would also influence the motion of the arm.

The next thing to be determined is the time of vibration, which I find in this manner;■ I observe the two extreme points of a vibration, and also the times at which the arm arrives at two given divisions between these extremes, taking care, as well as I can guess, that these divisions shall be on different sides of the middle point, and not very far from it. I then compute the middle point of the vibration, and, by proportion, find the time at which the arm comes to this middle point. I then, after a number of vibrations, repeat this operation, and divide the interval of time, between the coming of the arm to these two middle points, by the number of vibrations, which gives the time of one vibration. The following example will explain what is here said more clearly.

Instruments for the measurement of time were by then rather highly developed. Not only the pendulum clock, but also the lever escapement were used routinely.

Extreme points	*Division*	*Time*			*Point of rest*	*Time of middle of vibration*		
27.2		h.	′	″		h.	′	″
	25	10	23	4	—	10	23	23
	24			57				
22.1	—	—	—		24.6			
27	—	—	—		24.7			
22.6	—	—	—		24.75			
26.8	—	—	—		24.8			
23	—	—	—		24.85			
26.6	—	—	—		24.9			
	25	11	5	22	—	11	5	22
	24		6	48				
23.4								

h. ′ ″ denote hours, minutes, and seconds.

The first column contains the extreme points of the vibrations. The second, the intermediate divisions. The third, the time at which the arm came to these divisions; and the fourth, the point of rest, which is thus found: the mean between the first and third extreme points is 27.1, and the mean between this and the second extreme point is 24.6, which is the point of rest, as found by the three first extremes. In like manner, the point of rest found by the second, third, and fourth extremes is 24.7, and so on. The fifth column is the time at which the arm came to the middle point of the vibration, which is thus found: the mean between 27.2 and 22.1 is 24.65, and is the middle point of the first vibration; and, as the arm came to 25 at 10ʰ 23′ 4″, and to 24 at 10ʰ 23′ 57″, we find, by proportion, that it came to 24.65 at 10ʰ 23′ 23″. In like manner, the arm came to the middle of the seventh vibration at 11ʰ 5′ 22″; and, therefore, six vibrations were performed in 41′ 59″, or one vibration in 7′ 0″.■

Note the long period, 7 minutes, the result of the large moment of inertia of the arm.

To judge of the propriety of this method, we must consider in what manner the vibration is affected by the resistance of the air, and by the motion of the point of rest.

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An analysis of the errors contributed by these sources is omitted. Cavendish concluded that they were negligible.

ACCOUNT OF THE EXPERIMENTS

In my first experiments, the wire by which the arm was suspended was 39¼ inches long, and was of copper silvered, one foot of which weighed 2⁴⁄₁₀ grains: its stiffness was such, as to make the arm perform a vibration in about 15 minutes. 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.

In this trial, the rods by which the leaden weights were suspended were of iron; for, as I had taken care that there should be nothing magnetical in the arm, it seemed of no signification whether the rods were magnetical or not; but, for greater security, I took off the leaden weights, and tried what effect the rods would have by themselves. Now I find, by computation, that the attraction of gravity of these rods on the balls, is to that of the weights, nearly as 17 to 2500;[a] so that, as the attraction of the weights appeared, by the foregoing trial, to be sufficient to draw the arm aside by about 15 divisions, the attraction of the rods alone should draw it aside about 1/10 of a division; and, therefore, the motion of the rods from one near position to the other, should move it about 1/5 of a division.

Simply by the ratio of their masses, and taking into account the inverse square law.

The result of the experiment was, that for the first 15 minutes after the rods were removed from one near position to the other, very little motion was produced in the arm, and hardly more than ought to be produced by the action of gravity; but the motion then increased, so that, in about a quarter or half an hour more, it was found to have moved 1/2 or 1 1/2 division, in the same direction that it ought to have done by the action of gravity. On returning the irons back to their former position, the arm moved backward, in the same manner that it before moved forward.

It must be observed, that the motion of the arm, in these experiments, was hardly more than would sometimes take place without any apparent cause; but yet, as in three experiments which were made with these rods, the motion was constantly of the same kind, though differing in quantity from 1/2 to 1 1/2 division, there seems great reason to think that it was produced by the rods.

As this effect seemed to me to be owing to magnetism, though it was not such as I should have expected from that cause, I changed the iron rods for copper, and tried them as before; the result was, 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 experiment.

It must be observed, that the effect which seemed to be produced by moving the iron rods from one near position to the other, was, at a medium, not more than one division; whereas the effect produced by moving the weight from the midway to the near position, was about 15 divisions; so that, if I had continued to use the iron rods, the error in the result caused thereby, could hardly have exceeded 1/80 of the whole.

It must be observed, that in this experiment the attraction of the weights drew the arm from 11.5 to 25.8, so that, if no contrivance had

Experiment I. Aug. 5. Weights in midway position

Extreme points	*Division*	*Time*			*Point of rest*	*Time of middle vibration*			*Difference*	
		h.	′	″		h.	′	″	′	″
	11.4	9	42	0						
	11.5		55	0						
	11.5	10	5	0	11.5					
At 10ʰ 5′, weights moved to positive position.										
23.4										
27.6	—	—	—		25.82					
24.7	—	—	—		26.07					
27.3	—	—	—		26.1					
25.1	—	—	—							
At 11ʰ 6′, weights returned back to midway position.										
5.										
	11	0	0	48	—	0	1	13		
	12		1	30						
18.2	—	—	—		12		—	—	14	56
	12		16	29	—		16	9		
	11		17	20						
6.6	—	—	—		11.92		—	—	14	36
	11		30	24	—		30	45		
	12		31	11						
16.3	—	—	—		11.72		—	—	15	13
	12		45	58	—		45	58		
	11		47	4						
7.7										

Motion on moving from midway to pos. = 14.32
pos. to midway = 14.1
Time of one vibration = 14′ 55″

been used to prevent it, the momentum acquired thereby would have carried it to near 40, and would, therefore, have made the balls to strike against the case. To prevent this, after the arm had moved near 15 divisions, I returned the weights to the midway position, and let them remain there, till the arm came nearly to the extent of its vibration, and then again moved them to the positive position, whereby the vibrations were so much diminished, that the balls did not touch the sides; and it was this which prevented my observing the first extremity of the vibration. A like method was used, when the weights were returned to the midway position, and in the two following experiments.

The vibrations, in moving the weights from the midway to the positive position, were so small,■ that it was thought not worth while to observe the time of the vibration. When the weights were returned to the midway position, I determined the time of the arm's coming to the middle point of each vibration, in order to see how nearly the times of the different vibrations agreed together. In great part of the following

Because of the technique employed, which had the effect of damping the vibrations.

experiments, I contented my self with observing the time of its coming to the middle point of only the first and last vibration.

Omitted are the data from two additional experiments, which yielded substantially the same results.

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These experiments are sufficient to show, that the attraction of the weights on the balls is very sensible, and are also sufficiently regular to determine the quantity of this attraction pretty nearly, as the extreme results do not differ from each other by more than $\frac{1}{10}$ part. 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.

The first cause which occurred to me was, that possibly there might be a want of elasticity, either in the suspending wire, or something it was fastened to, which might make it yield more to a given pressure, after a long continuance of that pressure, than it did at first.

To put this to the trial, I moved the index so much, that the arm, if not prevented by the sides of the case, would have stood at about 50 divisions, so that, as it could not move farther than to 35 divisions, it was kept in a position 15 divisions distant from that which it would naturally have assumed from the stiffness of the wire; or, in other words, the wire was twisted 15 divisions. After having remained two or three hours in this position, the index was moved back, so as to leave the arm at liberty to assume its natural position.

It must be 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 two or three hours, 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 twice repeated, I could not perceive any such effect.

That is, exceeds the elastic limit.

A discussion of the results obtained with a stiffer suspension (having a period of about 7 minutes) is omitted. The same long-term drift was found.

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My next trials were, to see whether this effect was owing to magnetism. Now, as it happened, the case in which the arm was inclosed, was placed nearly parallel to the magnetic east and west, and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth;■ and the weights also, after having remained some time, either in the positive or negative position, would

By induced magnetism.

acquire polarity in the same direction, and would attract the balls; but, when the weights were moved to the contrary position, that pole which before pointed to the north, would point to the south, and would repel the ball it was approached to; but yet, as repelling one ball towards the south has the same effect on the arm as attracting the other towards the north, this would have no effect on the position of the arm. After some time, however, the poles of the weight would be reversed, and would begin to attract the balls, and would therefore produce the same kind of effect as was actually observed.

To try whether this was the case, I detached the weights from the upper part of the copper rods by which they were suspended, but still retained the lower joint, namely, that which passed through them; I then fixed them in their positive position, in such manner, that they could turn round on this joint, as a vertical axis. I also made an apparatus, by which I could turn them half way round, on these vertical axes, without opening the door of the room.

Having suffered the apparatus to remain in this manner for a day, I next morning observed the arm, and, having found it to be stationary, turned the weights half way round on their axes, but could not perceive any motion in the arm. Having suffered the weights to remain in this position for about an hour, I turned them back into their former position, but without its having any effect on the arm. This experiment was repeated on two other days, with the same result.

We may be sure, therefore, that the effect in question could not be produced by magnetism in the weights; for, if it was, turning them half round on their axes, would immediately have changed their magnetic attraction into repulsion, and have produced a motion in the arm.

As a further proof of this, I took off the leaden weights, and in their room placed two 10-inch magnets;■ the apparatus for turning them round being left as it was, and the magnets being placed horizontal, and pointing to the balls, and with their north poles turned to the north; but I could not find that any alteration was produced in the place of the arm, by turning them half round; which not only confirms the deduction drawn from the former experiment, but also seems to show, that in the experiments with the iron rods, the effect produced could not be owing to magnetism.

The sequence of tests described below mark Cavendish a most careful and thorough experimenter.

The next thing which suggested itself to me was that possibly the effect might be owing to a difference of temperature between the weights and the case; for it is evident, that if the weights were much warmer than the case, they would warm that side which was next to them, and produce a current of air, which would make the balls approach nearer to the weights. Though I thought it not likely that there should be sufficient difference, between the heat of the weights and case, to have any sensible effect, and though it seemed improbable that, in all foregoing experiments, the weights should happen to be warmer than the case, I resolved to examine into it, and for this purpose removed the apparatus used in the last experiments, and supported the weights by the copper rods, as before; and, having placed them in the midway position, I put a lamp under each, and placed a thermometer with its ball close to the outside of the case, near that part which one of the weights approached to in its positive position, and in such manner that I could distinguish the divisions by the telescope. Having done this, I shut the door, and some time after moved the weights to the positive position. At first, the arm was drawn aside only in its usual manner; but, in half an hour, the effect was so much increased that the arm was drawn 14 divisions aside, instead of about three, as it would otherwise have been, and the thermometer was raised near $1\frac{1}{2}°$; namely, from 61° to $62\frac{1}{2}°$.■ On opening the door, the weights were found to be no more heated, than just to prevent their feeling cool to my fingers.

Judging by the temperature, the thermometers carried Fahrenheit scales.

As the effect of a difference of temperature appeared to be so great, I bored a small hole in one of the weights, about three quarters of an inch deep, and inserted the ball of a small thermometer, and then covered up the opening with cement. Another small thermometer was placed with its ball close to the case, and as near to that part to which the weight was approached as could be done with safety; the thermometers being so placed, that when the weights were in the negative position, both could be seen through one of the telescopes, by means of light reflected from a concave mirror.

In these three experiments, the effect of the weight appeared to increase from two to five tenths of a division, on standing an hour; and the thermometers showed, that the weights were three or five tenths of a degree warmer than the air close to the case. In the two last experiments, I put a lamp into the room, overnight, in hopes of making the air warmer than the weights, but without effect, as the heat of the weights exceeded that of the air more in these two experiments than in the former.

On the evening of October 17, the weights being placed in the midway position, lamps were put under them, in order to warm them; the door was then shut, and the lamps suffered to burn out. The next morning it was found, on moving the weights to the negative position, that they were $7\frac{1}{2}°$ warmer than the air near the case. After they had continued an hour in that position, they were found to have cooled $1\frac{1}{2}°$, so

EXPERIMENT VI. SEPT. 6. Weights in midway position

Extreme points	*Divisions*	*Time* h	′	*Point of rest*	*Thermometer in air*	*in weight*
	18.9	9	43	—	55.5	
	18.85	10	3	18.85		
			Weights moved to negative position			
13.1	—	10	12	—	55.5	55.8
18.4	—		18	15.82		
13.4	—		25			
missed						
13.6	—		39	—	55.5	55.8
17.6	—		46	15.65		
13.8	—		53	15.65		
17.4	—	11	0	15.65		
14.0	—		7	15.65		
17.2	—		14	—	55.5	
			Weights moved to positive position			
25.8	—		23			
17.5	—		30	21.55		
25.4	—		37	21.6		
18.1	—		44	21.65		
25.0	—		51			
missed						
24.7	—	0	5			
19.	—		12	21.77		
24.4	—		19			

Motion of arm on moving weights from midway to − = 3.03
− to + = 5.9

as to be only 6° warmer than the air. They were then moved to the positive position; and in both positions the arm was drawn aside about four divisions more, after the weights had remained an hour in that position, than it was at first.

May 22, 1798. The experiment was repeated in the same manner, except that the lamps were made so as to burn only a short time, and only two hours were suffered to elapse before the weights were moved. The weights were now found to be scarcely 2° warmer than the case; and the arm was drawn aside about two divisions more, after the weights had remained an hour in the position they were moved to, than it was at first.

On May 23, the experiment was tried in the same manner, except that the weights were cooled by laying ice on them; the ice being confined in its place by tin plates, which, on moving the weights, fell to the ground, so as not to be in the way. On moving the weights to the negative position, they were found to be about 8° colder than the air, and their effect on the arm seemed now to diminish on standing, instead of increasing, as it did before; as the arm was drawn aside about 2½ divisions less, at the end of an hour after the motion of the weights, than it was at first.

It seems sufficiently proved, therefore, that the effect in question is produced, as above explained, by the difference of temperature between the weights and case; for, in the sixth, eighth, and ninth experiments, in which the weights were not much warmer than the case, their effect increased but little on standing; whereas, it increased much, when they were much warmer than the case, and decreased much, when they were much cooler.

It must be observed, that in this apparatus, the box in which the balls play is pretty deep, and the balls hang near the bottom of it, 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.

Omitted are the results of several more measurements, and a discussion of the method employed to calculate the density of the earth, together with the corrections applied.

■...

This table summarizes all the results. The second and third columns give the motions of the weight and arm respectively. The fourth gives the motion of the arm corrected for the change in attraction caused by its motion (Cavendish concluded that this was the only significant correction). The next two columns give the period, as observed and corrected, again for the same reason that the motion of the arm required correction. The last column contains the values of density computed from these data.

CONCLUSION. The following table contains the result of the experiments.

Exper.	*Mot. weight*	*Mot. arm*	*Do. corr.*	*Time vib.*		*Do. corr.*	*Dens.*
				'	"		
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		6.57	5.29
	− to +	6.1	5.9	by mean			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

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 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 can 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.

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. 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 difference of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems very 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 very unlikely that the density of the earth should differ from 5.48 by so much as 1/14 of the whole.

■ Compare this result with that found by C. V. Boys (*Phil. Trans.*, 1895) using a similar technique, 5.5270. The agreement is within 1 percent.

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, I made the ninth, tenth, eleventh, and fifteenth 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

By measuring the deflection of a plumb bob suspended near the hill. But to determine the density of the earth from this observation, one needs to know the mass of the mountain accurately.

An appendix, containing a calculation of the effect of the mahogany case on the balls (found to be negligible), has been omitted.

that circumstance, and when the balls are placed in any other part of the case.

According to the experiments made by Dr. Maskelyne, on the attraction of the hill Schehallien,■ the density of the earth is 4½ times that of water; which differs rather more from the preceding determination than I should have expected. 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 quantity I cannot measure.

■......................................

SUPPLEMENTARY READING

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