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ON THE MEAN DENSITY OF THE EARTH

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From the Author

ON THE MEAN DENSITY OF THE EARTH.

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ALTHOUGH the determination of the mean density of the whole terraqueous globe of our planet, is admitted to be a problem of the utmost importance to several branches of philosophy, particularly to physical astronomy, and the figure and constitution of the earth; it would seem, from the discordancy of the declared opinions of some eminent philosophers, that the problem is still in an uncertain state. Since the first notice of this subject by NEWTON, in his admirable Principia, it has often been incidentally alluded to, without receiving a precise determination; with the exception of two instances only, in which it has been stated to be, certainly or approximately, determined by experiment; namely, in the case of the Schehallien experiment, by Dr. MASKELYNE and myself, and by the Honorable HENRY CAVENDISH, by a method invented by Mr. MICHELL.

The former of these experiments was made by Dr. MASKELYNE, in the years 1774, 1775, and 1776, by means of that large mountain in Scotland, in measuring its dimensions, and in comparing its attraction on a plummet, with that of the whole earth on the same; the calculations on it having been made by myself, and first published in the Philosophical Transactions of the year 1778; and since more correctly in the second volume of my Mathematical Tracts.

The other experiment, by Mr. CAVENDISH, was by observing the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same.

By some strange mistake, or perversion, for many years, it was customary among certain persons, to withhold the mention of my name, with regard to the great share that I had in the experiment on Schehallien. But from certain complaints which I have made, some little justice has lately been awarded to me on that head; though still, it would seem, with reluctance, as the opinion is promptly assumed that the latter small experiment is susceptible of the greater accuracy, and the numbers in its result gratuitously adopted as nearer the truth than that of the former. As this is an opinion which I have never been able to bring my mind to acknowledge, and as it is a matter of great importance in the present state of physics, I have been desirous to draw the attention of philosophers to a closer consideration of the subject, with a view to a more deliberate and impartial decision of this point.

From the closest and most scrupulous attention I can employ on this question, the preference, in point of accuracy, appears to be decidedly in favour of the large or mountain experiment, over that of the small balls. It is indeed true, that though the large mass of the mountain must yield an immensely greater force than a small ball, yet it may be said that this advantage must be balanced, either wholly or in great part, on the score of distance, as the plummet is acted on at a great distance from the centre of the mountain, while the balls are approached very near together; so that the visible effects may thus be nearly equal, by the reciprocal balancing between magnitude and distance. Hence the visible effect of the mountain, is that of the small angle of eleven or twelve seconds, by which the plummet is drawn aside from the perpendicular; thereby showing that the attraction of the earth, on the plummet, is to that of the mountain on it, as radius is to the tangent of those seconds; while, in the other experiment, the small pendulous balls are drawn aside by the large ones the space of between one-seventh and two-thirds of an inch; the distance of each ball from the middle of their connecting rod, being a little more than thirty-six inches. The first or immediate small results of the two experiments, thus appearing so far to be about equally favourable, it will be necessary to examine the circumstances of each of them separately, that we may be able to judge more particularly of their merits; and, first, of the Schehallien experiment.

This experiment, it is well known, was conducted by the late Astronomer Royal, Dr. MASKELYNE, than whom a more correct, faithful, and experienced individual probably never existed. The account of his measures and observations, taken in conducting it, is minutely detailed in the Philosophical Transactions of the year 1775, or in my edition of the Transactions, vol. xiii, page 702; where all the instruments and operations are particularly described, in the most plain and satisfactory manner. The principal instrument was the ten foot zenith sector; with which the meridian zenith distances of forty-three stars by three-hundred and thirty-seven observations, were carefully taken, both on the north and south sides of the mountain. The medium of all these, with other necessary measures, gave a final result of 11.6 seconds, for the sum of the deviations of the plumb line, on both sides of the mountain; and that, in all probability, within much less than half a second of the truth. Other instruments used, were the Royal Society's transit instrument made by Mr. BIRD, and an astronomical clock by SHELTON, which had both been provided on occasion of the observations on the transit of Venus, in 1761 or 1769. Besides these and several other instruments, one of RAMSDEN's best theodolites was used, in measuring the figure and dimensions of the mountain, which was performed in the most correct manner by skilful surveyors; so as that thence an exact model of it might be made, or all its dimensions accurately taken, for computing the attraction.

By only reading over the accounts of these operations, (in the places before mentioned) made by means of such instruments, and in such hands, every person must be convinced of the impossibility that any error could have been committed, capable of causing any sensible inaccuracy in the conclusion of the work.

It remains now to describe the share which I bore in this important business; which consisted in taking all the measurements as above described, and from those data, calculating what must have been the exact magnitude of the mountain; what its attraction on the plummet, relatively to that of the globe of the earth on the same; and what must be the mean density of the earth. These computations, which employed my daily and assiduous labours during the greater part of two years, are recorded in the Philosophical Transactions of the year 1778, and also in the second volume of my Mathematical Tracts. It may there be seen, that after computing, trigonometrically, the bearing and distance of every point in the numerous sections of the mountain, from the two observatories, I conceived it to be divided into nearly one thousand vertical columns, of given bases and altitudes. I then computed the quantity of the attraction of all these columns, on the plummet, in the direction of the meridian, when placed at the two observatories, on both sides of the hill, where the whole effect had been observed, which attraction was thus found to be expressed by the number $8811\frac{2}{3}$. I then computed, from the magnitude of the earth, what must be its attraction on the same plummet, and found it expressed by the number 87522720.

Consequently, the whole attraction of the earth, is to the sum of the two contrary attractions of the mountain, as the number 87522720 to $8811\frac{2}{3}$; that is, as 9933 to 1 very nearly; on supposition that the density of the matter in the hill, is equal to the mean density of that in the earth.

But Dr. Maskelyne found by his observations, that the sum of the deviations of the plumb line, produced by the two contrary attractions, was 11.6 seconds. Hence then it is inferred, that the attraction of the earth, is actually to the sum of the attractions of the hill, nearly as radius to the tangent of 11.6 seconds, that is, as 1 to .000056239, or as 17781 to 1; or as 17804 to 1 nearly, after allowing for the centrifugal force arising from the rotation of the earth about its axis.

Having now obtained the two results, namely, that which arises from the actual observations, and that due to the computation on the supposition of an equal density in the two bodies, the two ratios compared, must give the ratio of their densities, and which is therefore that of 17804 to 9933, or 1434 to 800 nearly, or almost as 9 to 5; and so much does the mean density of the earth exceed that of the hill. Consequently, if we know the density of the latter, we shall thence obtain that of the former.

At the time when this computation was first printed, in the year 1778, the real density of the hill was unknown. It was only known that it consisted chiefly of very hard and dense rocks, much heavier than common stone, which is allowed to be $2\frac{1}{2}$ times the density of water. I then, by way of example in applying the density, multiplied $\frac{9}{5}$ by $2\frac{1}{2}$, which produced $\frac{9}{2}$ or $4\frac{1}{2}$ for the density of the earth, on the smallest assumption; till such time as we should come to know more nearly what the real density of those rocks is: and therefore I must feel reason to complain, that this number ($4\frac{1}{2}$) has often been stated, rather unfairly, as my final conclusion for the earth's mean density; instead of being only the very lowest limit that might be used, till we could better learn something on that point with more certainty. But a lithological survey of the mountain being afterwards accurately made, at my earnest request, by that excellent philosopher and geologist, Mr. PLAYFAIR, the result of which was published in the Philosophical Transactions for the year 1811; I then applied his mean statement of the rocks to my own calculations, which gave me the number 5 for the density of the earth; as I published in the fourteenth volume of my edition of the Philosophical Transactions, and in the second volume of my Tracts.

In Mr. PLAYFAIR's account of the mountain, are given the names and nature of the several rocks that compose it, with tables or lists of their densities or specific gravities. In one table is a list of thirteen specimens of densities, contained between the numbers 2.6109 and 2.6656, the medium of the whole being 2.639876. In another table, of fifteen specimens, the densities are limited between 2.71845 and 3.0642, the medium of all which is 2.81039. And the mean between these two means, gives 2.725 for the medium density of the whole mountain, admitting it to be quite solid, or without vacuities, as it appears to be on the exterior surface at least. But in the calculation in my Tracts I went even a little higher, using the number 2.75 or $2\frac{3}{4}$, thus $\frac{9}{5} \times 2\frac{3}{4}$, which gives $\frac{99}{20}$ or 4.95 for the mean density of the earth. Or, if we assume the density of the mountain still higher, as 2.8 instead of 2.75, we then obtain $\frac{9}{5} \times 2.8 = 5.05$, a little more than 5 for the earth's density; which last number 5, I therefore fix upon, in conclusion, as probably the nearest to the truth; or at least it is sufficiently large, as it is grounded on several assumptions that are most favourable for the highest result; namely, 2.777714, or $2\frac{7}{9}$ for the density of the mountain; also $\frac{9}{5}$ as rather above the calculated ratio of the densities of the earth and mountain; and lastly, the assumption of the mountain being quite solid; though it is probable that there may be cavities in most mountains, as they are generally the production either of volcanoes, or of earthquakes.

For all these reasons, then, it is highly probable, that the earth's mean density is very near five times the density of water; but not higher. If any person should still hesitate to adopt this conclusion, his hesitation must arise from doubts either on the data obtained by the measurements, or on the accuracy of the computations made from them. But if any such person attentively read over Dr. MASKELYNE's account of the measurements, in the Philosophical Transactions of 1775, his doubts must be soon removed, as to the data supplied by the survey of the hill, or by the astronomical observations. And as to the accuracy of my own computations, made from those data, they are fully and fairly before the public, in the works before mentioned; and let any person, who doubts, look over and repeat the calculations there stated, and try if he can find any inaccuracy in them. The only possible ground of doubt in the measured data, must be in the observed deviation in the plumb line, taken by Dr. MASKELYNE; but when we consider the accuracy of the observer, and of the instruments, and read the account of the use of them, it must be then very difficult to doubt of their accuracy. On this point it is commonly acknowledged that a good observer, with the best instruments, can observe angles to a small fraction of a second. Dr. MASKELYNE's observations give 11.6 seconds for the sum of the deviations of the plumb line, from a medium of between 300 and 400 observations. Now let us suppose it possible to have committed an error of four tenths of a second in this number, and that the true number should have been 12 seconds, instead of 11.6, being an error of the twenty-ninth part of the whole. This then would cause an error of the 29th part of the result; which would reduce the density 5 to about 4.8; showing that the number 11.6 is not too small, but may be the contrary. Next, let us assume 11 seconds only, omitting the six-tenths, being almost the twentieth part of the whole, and which therefore would give nearly 5.25 for the earth's density, being still far below the number 5.48, as deduced from Mr. CAVENDISH's experiment. Hence it appears, that our result cannot be made to agree with that of Mr. CAVENDISH, unless our 11.6 seconds be diminished to about 10.5 or 10.4, on the supposition of an error of more than a whole second in excess, in the number 11.6 seconds; which cannot be admitted, without doing great violence to the observations.

Having thus failed in our endeavour to discover any error, or even suspicion of error, in the conduct or result of the Schehallien experiment, let us now turn our attention to the other experiment, as performed by Mr. CAVENDISH. And here I must at once disclaim all expectation of meeting any failing with regard to the operator himself, whom I well knew to be a most excellent philosopher and mathematician, as well as a patient, accurate, and acute experimenter. The failure then, if any, must be expected from the nature of the machine, and of the calculations.

From the perusal of Mr. CAVENDISH's account of the machine he employed (in the Philosophical Transactions of 1778, or vol. xviii. of my edition), and the nature of the arithmetical calculations, they at once appear to be formidable and discouraging in the highest degree. The machine is small, comparatively with those in the former, or mountain experiment. It is not easily to be understood, without actually seeing it, though assisted with the view of the drawing of the whole, on account of the intricacy and perplexity of the construction. In the first place, at each end of a light wooden rod, of near two yards in length, is attached a small leaden ball of two inches diameter; the middle of the rod being fixed to and suspended by a long and very slender copper wire; by any small movement of these balls and the connecting rod, in a horizontal direction, by the torsion or twisting of the wire, a very minute and slow vibratory motion is commenced. To produce this small motion in the two little balls, and their connecting rod, two other large balls of ten inches diameter, are connected together by certain machinery, at like distance as the former, and capable of being moved to different distances on the horizontal level with the small balls. By so setting the large balls near the small ones, these are attracted by the former, producing a very small motion in them, and in consequence a very slow vibration. So minute are these motions, that the extent of the vibrations is but a small fraction of an inch, and the duration of each vibration is not performed but in the time of several minutes, from three or four to near fifteen minutes. So minute are these motions, that telescopes and other means are necessary to view and to estimate their quantity and durations. To produce these minute motions, very complex machinery are necessarily employed, while the delicate movements are watched for many hours together, during many days, and recorded with regard to the extent and time of each vibration. Then, from these spaces and times, the density of the earth is to be calculated, by peculiar theorems, as compared with the vibrations of common pendulums that are produced by the attractions of the earth.

All these effects were so minute, and produced by machinery so complex, and the results calculated by theorems derived from intricate mathematical investigations, that it is impossible, at first, for ordinary readers to conceive how any accurate results can be deduced from them; and even for the more judicious reader to place confidence in them, except chiefly on account of the high character of the experimenter himself. From the nature of the machinery, I could therefore derive no confidence in the results, nor compare them with the mountain experiment, without repeating the whole of the calculations. But, after a long life spent in almost daily abstruse investigations, from the tenth year of my age, and now being at eighty-four, and oppressed with distressing illness, I thought I might be excused from such a task. But, after urging more than one mathematical friend, without being able to interest them sufficiently to engage in so severe an operation, my anxiety to accomplish the business induced me to make an exertion to effect it myself; especially as the learned experimenter informs us, that he availed himself of the assistance of the then clerk of the Society, who he says made some of the experiments, and who doubtless made most of the arithmetical computations: operations, of both kinds, in which I remember he was much employed by Sir Charles Blagden, and other gentlemen, in preparing their papers for the Royal Society. I have therefore recomputed all the experiments, and have traced the investigations of all the theorems; and have found that my labour has not been in vain; but, on the contrary, has been rewarded with the following copious list of errata, some of which are large or important.

In the following instances it is to be noted, that the references are made to Mr. Cavendish's paper, as printed in my edition of the Philosophical Transactions, as I am not now possessed of a set of the original edition; but with which, however, I have had my own set compared and verified.

Some of the errata in Mr. CAVENDISH'S *paper.*

In page 399, line 10 from the bottom, for 8739000, read 8740000.

In page 399, line 6 b, or from the bottom, for 8739000, read 8740000.

The same also in line 5 b.

In page 399, line 4 b, for 10683 read 10685.

The same also in line 1 b.

In page 403, lines 12 and 13, for 8739000, read 8740000.

———— line 13, for 10844, read 10847.

———————for 10683, read 10685.

In page 404, line 11, for 185, read 186.5.

————lines 15, 16, 22, 25, for 185, read 186.5.

It is to be noted, that after the experiments have been all made, and the motion of the arm carrying the small balls, and expressed in twentieths of an inch, observed and denoted by the letter B; also the time of one vibration, expressed in seconds, denoted by the letter N; and both of these being corrected according to certain rules there given, then the mean density of the earth D, in each experiment, is to be computed by this theorem,

$$viz.\ D = \frac{N^2}{10683\,B},\ \text{or when corrected,}\ D = \frac{N^2}{10685\,B}.$$

And by this theorem were calculated the following twenty-nine experiments, as they stand recorded in the original.

Table of the results of the experiments.

Experiments.	Motion of the arm.	The same corrected.	Time of vibration.	Ditto corrected.	The Density.
	20ths. Inc.	20ths. Inc.	M. S.		
1	14.32	13.42			5.50
2	14. 1	13.17	14 55		5.61
3	15.87	14.69			4.88
4	15.45	14.14	14 42		5.07
5	15.22	13.56	14 39		5.26
6	14. 5	13.28	14 54		5.55
7	3. 1	2.95		6 54	5.36
8	6.18	.	7 1		5.29
9	5.92	.	7 3		5.58
10	5. 9	.	7 5		5.65
11	5.98	.	7 5		5.57
12	3.03	2.9			5.53
13	5. 9	5.71	7 4		5.62
14	3.15	3.03	By	6 57	5.29
15	6. 1	5.9			5.44
16	3.13	3.00	mean.		5.34
17	5.72	5.54			5.79
18	6.32	.	6 58		5.10
19	6.15	.	6 59		5.27
20	6.07	.	7 1		5.39
21	6.09	.	7 3		5.42
22	6.12	.	7 6		5.47
23	5.97	.	7 7		5.63
24	6.27	.	7 6		5.34
25	6.13	.	7 6		5.46
26	6.34	.	7 7		5.30
27	6. 1	.	7 16		5.75
28	5.78	.	7 2		5.68
29	5.64	.	7 3		5.85

The last column shows the numbers for the required density, resulting from the calculation by the foregoing theorem, being all a little above 5, excepting the third number, which is a little below 5. And immediately after, is the following remark, showing the author's doubt of their accuracy; *viz.* "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 cur-"rent 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." It then proceeds: "By a mean of the experiments made with the wire first "used," (*viz.* the first six numbers or experiments) "the "density of the earth comes out 5.48 times greater than that "of water; and by the mean of those made with the latter "wire, it comes out the same; &c."

Now, though the former list of errata were but small in quantity, yet here is one of considerable magnitude, *viz.* in the medium of the first six experiments, said to be 5.48, which is very erroneous, the true medium being only 5.31; and it is rather curious that that medium 5.48 has been obtained, by taking the third experiment as 5.88 instead of 4.88, through mere oversight or carelessness. If this were the only error, it might perhaps be excused as a single accident; but the whole will make a very different appearance, when we have shown that many small errors exist in almost all the numbers in the last column of the table, as resulting from erroneous calculations, in the use of the general theorem before mentioned, and evinced by a comparison of the numbers in the foregoing table, with those of the following one, derived by our calculation from the same data, and by the same theorem.

The corrected table of the experiment results.

Experiments	Motion of arm corrected.	Time of vibr corrected.	Ditto in seconds.	Densities corrected.
	20ths Inch.	Min. Sec.	Seconds.	
1	13.46	14 55	895	5.49
2	13.21	14 55	895	5.59
3	15.17	14 55	895	4.86
4	14.68	14 42	882	4.89
5	14.46	14 39	879	4.93
6	13.63	14 54	894	5.41
7	2.92	6 54	414	5.41
8	3.09	7 1	421	5.29
9	2.96	7 3	423	5.57
10	2.95	7 5	425	5.64
11	2.99	7 5	425	5.57
12	2.85	6 57	417	5.62
13	2.86	6 57	417	5.61
14	2.97	6 57	417	5.40
15	2.95	6 57	417	5.43
16	2.97	6 57	417	5.40
17	2.77	6 57	417	5.79
18	3.16	6 58	418	5.10
19	3.08	6 59	419	5.26
20	3.03	7 1	421	5.38
21	3.05	7 3	423	5.42
22	3.06	7 6	426	5.47
23	2.99	7 7	427	5.64
24	3.14	7 6	426	5.34
25	3.07	7 6	426	5.46
26	3.17	7 7	427	5.30
27	3.05	7 16	422	5.38
28	2.89	7 2	422	5.68
29	2.82	7 3	423	5.85

Here the medium of the first six of these experiments is 5.19; of the other twenty-three experiments it is 5.43; and the mean of both these means is 5.31, instead of 5.48, as stated in the former table, being the error arising from the sum of the numerical calculations. The remaining difference, 0.31, about the 17th part of the whole, must therefore be ascribed to the inaccuracy of making and reading off experiments, with such intricate and inadequate machinery.

I cannot conclude this paper of enquiry, without expressing a hearty wish for the repetition of the large or mountain experiment, in some other favourable situation, and with improved means, if possible. For this purpose, I shall venture just to mention an idea which has sometimes occurred to my mind, namely, that one of the large pyramids in Egypt might profitably be employed, instead of a mountain, for this experiment. Such a body offers several advantages for the purpose. In the first place, the mass is sufficiently large, standing on a base of about the size of the whole space of Lincoln's Inn Fields, and of a height almost double of that of St. Paul's steeple; then the station for the plummet, or zenith sector, could be taken much nearer the centre of the mass, than on a mountain, which would give a larger quantity of deviation of the plummet; then the regular figure and the known composition of the mass would yield great facilities in the calculation of its attraction; lastly, the deviation of the plummet might be observed on all the four sides. Should such a project take place, it will be best to take the stations at about one fourth of its altitude above the base, that being the place where the deviation of the plummet would be the greatest. Finally, so favourable for such an experiment do those circumstances appear, and so anxious are my wishes for its completion and success, that, were it not for my great age and little health, I should be glad to make one in any party to undertake such an expedition.

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