

LXVI. *Observations on a Clock of Mr. John Shelton, made at St. Helena: In a Letter to the Right Honourable Lord Charles Cavendish, Vice-President of the Royal Society, from the Reverend Nevil Maskelyne, M. A. F. R. S.*

My Lord,

Read Jan. 21,
1762.

WHEN we reflect upon the great degree of perfection, to which the sciences are at present brought, and, at the same time, consider from what low beginnings in former times they have arisen to this height, we are apt to please ourselves with the idea of a certain kind of superiority, which we imagine we enjoy above the learned, who have gone before us. But, when we recollect by what slow degrees, and by what painful applications of ingenious men, in various ages, these improvements have been brought about, we shall, perhaps, be inclined to own, that our fancied superiority over our predecessors in science is chiefly that of being so fortunate, as, by coming after them, to enjoy the fruits of their labours, joined to our own. But, though we are obliged to give up part of the satisfaction, which we might receive, from assuming something to ourselves, from the present state of science; yet, in return, we shall be gratified with a pleasure not at all inferior, which the curious mind must receive, from this very consideration of the successive

cessive degrees, by which knowledge has been augmented to that pitch, in which we now behold it.

The antients were well acquainted with the roundness of the earth, and were satisfied, that heavy bodies, in every place, had a tendency to its center : but they had never any suspicion, that the force of their tendency to the center was greater in one country than another, or that, when dropped from any height, they fell faster in one latitude than another.

The great Huygens, who first set the doctrine of centrifugal forces in a clearer light, saw plainly, that the weight of bodies must naturally be less at the equator, than at the poles ; their great velocity there round the earth's axis taking off part of the weight, which they acquire by their gravitation towards the center of the earth. And, though he was not quite exact in settling the true proportion of the force of gravity in different latitudes ; yet we owe this obligation to him, of having made the first discovery of a thing, which hath since been the ground of so many theories and experiments.

Mr. Richer, when he went to the island of Cayenne, made the first experimental proof of the decrease of gravity, in approaching the equator, though he was not led thereto by Huygens's theory, which was then but lately published, and not so generally known ; but, from finding his clock, which he had brought with him, go considerably slower than it had gone in France.

But Sir Isaac Newton, first of all, shewed, how the variation of gravity in different latitudes depended not only upon the centrifugal force, but also upon the figure of the earth, which he likewise determined, as well

well as the proportion of the force of gravity in different latitudes, as far as theory alone could limit them.

The ingenious Mr. Colin Campbell made a very curious experiment, of the diminution of gravity from London to Jamaica, by means of an excellent clock, made by Mr. Graham, an account of which is given by Dr. Bradley, in N^o 432 of the Philosophical Transactions, to which he has added a table of his own, expressing the proportion of the force of gravity in different latitudes, and has subjoined the proportion of the equatorial to the polar diameter of the earth, which should follow from the experiment, according to Sir Isaac Newton's principles.

A like experiment was made by the learned French astronomers, who went to the polar circle, to measure the length of a degree of the meridian, by a clock made by the same excellent artist, expressly for the same purpose.

We are likewise obliged to the gentlemen of the Royal Academy of Sciences at Paris, for several experiments made by them, in order to determine the force of gravity in different places, by measuring the length of the second pendulum.

I could not fail of being desirous of improving the opportunity, which my voyage to St. Helena afforded me, of examining this curious point, among my other experiments: and your Lordship did me the honour, not only of approving of my design, but also of thinking, that it was proper the very best instruments yet known should be employed for this purpose; as did also the Royal Society, to whom I communicated my intention, who were pleased to

furnish me with an excellent clock, with a gridiron-pendulum adapted to it, executed by that diligent and ingenious artist Mr. John Shelton.

Dr. Bradley was pleased to take upon him the trouble of setting the clock up at the Royal Observatory at Greenwich, and there to examine its going, where he informed me, that it lost 11 seconds per day, upon sidereal time, the thermometer of Fahrenheit's construction, which was placed within of the clock-case, standing about 50 degrees, at a medium, during the time, in which the experiment was made. Though the contrivance of the gridiron-pendulum might be considered as a sufficient security against any variations in the going of the clock, which might arise from the changes of heat and cold, yet it was thought the experiment would be more satisfactory, if the temperature of the air was set down at the time.

Soon after my arrival at St. Helena, I set up the clock in the valley near James's Fort, in a place elevated 85 feet above the level of the sea. Being sensible, how much the exactness of the going of a clock depends upon the firm manner of setting it up, I had large pieces of wood driven into a stone-wall, between the joining of the stones, to which I screwed the back of the clock-case, which was very solid and heavy, by four screws, the bottom resting upon a large flat stone. The pendulum had not been taken off from the clock, for carriage, but was secured to the clock-case, in order to prevent it from receiving any damage. A piece of wood was screwed to the back of the clock-case, having a round cavity in it before, just large enough to receive the bob of the pendulum

pendulum : another piece of wood, with such another cavity in it, likewise fitting the bob of the pendulum, was applied to it, on the fore-part, and screwed firmly to the other piece : so that the bob was embraced between the two pieces, and secured firmly to them, and to the back of the clock-case. Two little pieces of wood likewise kept the upper part of the pendulum in its place, from receiving any motion near the center of suspension.

When the pieces of wood were taken away, and the pendulum thereby disengaged, the clock was fit for use : only, to adjust the pendulum to the same exact length, as it was of at Greenwich, a mark had been made on the rod, where the top of the bob of the pendulum rose to ; and Dr. Bradley informed me of the number, which stood against the index, on the nut at the bottom of the pendulum, by screwing or unscrewing of which the bob is elevated or depressed.

I had a post fixed up on the top of a hill, at a distance due north of me, to which I always adjusted the transit instrument, before an observation, which then gave the true time of the transits of the Sun and stars across the meridian, within a very few seconds : for it was not necessary for my purpose, to be more precise in the fixing of my mark, which would have been attended with great trouble, on account of its situation, as the clock was to remain fixed up in the place, where it was, only for a few weeks, and as I always adjusted the instrument truly to the same vertical circle, which was at the same time very near to the meridian.

I shall

I shall now lay before your Lordship the transits, which I took of the Sun, contained in the following table; the first column of which, expresses the day of the month of the observation: the second, the time shewn by the clock, at the instant of the transit of the Sun's center, across the middle vertical wire of the instrument, on that day: the third column is made from the second, by applying the equation of time thereto. Therefore, the differences between the numbers in the second column, shew the rate at which the clock gets upon the Sun; and the differences between the numbers in the third column shew the rate, at which the clock gets upon mean time.

	Time by the clock at the Sun's transit.			Time by the clock, with the equation of time applied.		
	h	m	s	h	m	s
1761.						
April 25.	0	11	30	0	13	47
27.		15	10 $\frac{1}{2}$		17	48
29.		18	49		21	45,1
30.		20	40 $\frac{1}{2}$		23	44,8
May 3.		26	16 $\frac{1}{2}$		29	42,1
6.		31	56 $\frac{3}{4}$		35	39 $\frac{1}{2}$
8.		35	45 $\frac{1}{2}$		39	36,4

The thermometer, placed in the clock-case, taking a medium between the observations of morning, noon, and night, stood each day as follows:

		Degrees.
April	25.	73
	26.	73
	27.	73
	28.	73
	29.	73
	30.	72
May	1.	72
	2.	72
	3.	71
	4.	71
	5.	70
	6.	70
	7.	72
	8.	72

Your Lordship will perceive, that, from April 25 to April 27, the clock got $4^m 1^f$, upon mean time, in two days; which is at the rate of $2^m 0^f \frac{1}{2}$ per day. In like manner, from the differences between the following observations, the clock appeared to get per day $1^m 58^f \frac{1}{2}$, $1^m 59^f, 7$, $1^m 59^f, 1$, $1^m 59^f$, $1^m 58^f, 6$; the mean of all which is $1^m 59^f, 2$. The thermometer in the clock-case, at a medium, during this time, stood at 72.

The observations, as your Lordship will perceive, agree so near together, in giving the same rate of the going of the clock, that there is scarce room for preferring one to another. But, from the circumstances of the observations, I should rather chuse to deduce the rate, at which the clock gets per day, from the transits of April 30 and May 8; which is exactly

1^m 59^f; the thermometer, at a medium, during this time, standing at 71.

Allowing, therefore, the clock to get 1^m 59^f in a mean solar day, upon mean solar time, in 23^h 56^m 4^f of mean solar time, or in a sidereal day, it will get only 1^m 58^f $\frac{2}{3}$ upon mean solar time: but a clock adjusted to mean solar time loses 3^m 56^f by the stars, in a sidereal day, therefore the clock loses 1^m 57^f $\frac{1}{3}$ of sidereal time, in one revolution of the stars; which agrees exactly with what I find by the transits ϵ , ν , α , and ζ Leonis. For, April 30, ϵ Leonis passed at 7^h 32^m 30^f by the clock; ν Leonis at 7^h 39^m 24^f; α Leonis at 7^h 55^m 52^f; and ζ Leonis at 8^h 3^m 36^f. And May 8, ϵ Leonis passed at 7^h 5^m 10^f; ν Leonis at 7^h 12^m 3^f; α Leonis at 7^h 28^m 31^f; and ζ Leonis at 7^h 36^m 15^f. Therefore, the clock loses 27^m 20^f by ϵ Leonis, in the space of 14 days, and 27^m 21^f in the same time, by the other three stars: the medium is 27^m 20^f $\frac{3}{4}$; whence the clock loses upon the stars, in one revolution, 1^m 57^f $\frac{1}{3}$; which agrees entirely with what is deduced above from the transits of the Sun, which was 1^m 57^f $\frac{1}{3}$.

The same clock at Greenwich, with the pendulum adjusted to the same length, lost 11^f upon the stars, in a sidereal day. Therefore, the force of gravity at Greenwich is to the force of gravity at St. Helena, as the square of 23^h 59^m 49^f to the square of 23^h 58^m 2^f $\frac{2}{3}$:: 10000000 : 9975405. The extent of the vibrations of the pendulum here, as well as in England, is exactly 1° 45' on each side of the perpendicular, according to the divided arch, which is at the bottom of the pendulum.

I flatter myself, your Lordship will excuse me, if I do not attempt to deduce any consequences, at present, from the above observations, either with respect to the law, which the force of gravity observes in its changes in different latitudes, or with respect to the figure of the Earth, which it has been supposed might be determined from experiments of this kind alone, independently of any others, the great Sir Isaac Newton having himself set us the example. If the body of the Earth was homogeneous throughout, not only the figure of the Earth, but also the law of the variations of gravity in different latitudes would be given, and would be the same as Sir Isaac Newton has described them. But if the Earth be not homogeneous, and there seems great reason, from late experiments, to doubt if it be so, we can form no certain conclusions concerning the figure of the Earth, from knowing the force of gravity in different latitudes; as this force must depend not only on the external figure, but also in the internal constitution and density of the Earth.

Your Lordship will, perhaps, think, many more experiments, not only of the kind, which I have the honour to give an account of to your Lordship, but also of other different kinds, to be necessary, before we shall be able to infer any thing with certainty, concerning the internal constitution of the Earth, or even to determine its external figure. But every experiment is useful, which tends to throw a light over this intricate subject, and to shew the perfect agreement of the laws of nature, with the actual constitution of things.

In hopes the experiment here recited may be taken in this view, as tending, in conjunction with a variety of others, to illustrate an important point, I beg leave to present it to your Lordship; and am

My Lord,

Your Lordship's

most obedient

and devoted

humble servant,

St. Helena,
July 30, 1761.

Nevil Maskelyne.

LXVII. *Observations upon some Gems similar to the Tourmalin; by Mr. Benjamin Wilson, F. R. S.*

Read Jan. 28, 1762. **T**HE honour I received from the Royal Society, in consequence of my late papers on electricity, and which principally respected the Tourmalin, is a pleasing motive for embracing the first opportunity to communicate farther enquiries, that tend to throw more light upon this curious subject.

In September last, I met with several gems of different sizes and colours, that resemble the Tourmalin, in regard to electric experiments. The most beautiful
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