



XXXVI. Notice relating to the seconds pendulum at Port Bowen

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in combining with matter never changes its nature; it is never annihilated; it passes from free heat to latent heat, and the contrary, according to circumstances. The only question is about the generality of the fact; whether it be true that heat which expands bodies is always concealed from the thermometer. We have proved that it is true in elastic fluids; and analogy, aided by the discoveries of Dr. Black, affords a strong argument that it holds without exception.

The theory we have been explaining is nowise inconsistent with the doctrine of specific heat and capacity. We have here compared the quantities of heat which unite with bodies when their temperature is raised, with the dilatation which they produce. But we may likewise compare with one another the quantities of heat requisite to cause a given rise of temperature in different bodies; and, in this view, they are called specific heats, and the bodies themselves are said to have different capacities for heat. These two ways of considering the manner in which heat combines with bodies, are clearly distinguished. The one by no means supersedes the other. On the contrary, we may deduce from the property of latent heat we have endeavoured to establish, the condition which causes the capacity of a body to be constant, or to vary. Whenever equal additions of latent heat produce equal increments of volume, the capacity must be constant; otherwise it must vary. This will readily appear, if it be considered that it is the latent heat which causes the expansion, and that we employ the expansion to measure the free heat, or the temperature. The specific heat of bodies is, therefore, plainly regulated by the latent heat. But in other respects the doctrine of capacity leads to considerations of which we have had no occasion to speak.

The observations I have been led to make have carried me far beyond my original intention, and I must reserve what further remains on this subject for a future occasion.

Feb. 5, 1827.

J. IVORY.

XXXVI. *Notice relating to the Seconds Pendulum at Port Bowen.* By J. IVORY, Esq. M.A. F.R.S.*

THE 4th part of the Philosophical Transactions just published, contains an experimental determination of the seconds pendulum at Port Bowen, a station in Prince Regent's Inlet, by Lieutenant Henry Foster, R.N. F.R.S. The result

* Communicated by the Author.

of this experiment and the comparison of it with my formula, Phil. Mag. for Oct. 1826, are as follows:

Latitude.	Observed pendulum.	Computed pendulum.	Excess of calculation.
73° 13' 39".4	39.20347	39.20265	—00082

The error is not great: and this is the 29th experiment represented by my formula with small discrepancies.

The latitude of Port Bowen being little more than a degree short of that of Captain Sabine's station at Greenland, we may compare the two experiments.

	Latitude.	Observed pendulum.
Sabine	74° 32' 19"	39.20335
Foster	73 13 39	39.20347

Here the pendulum has shortened for an increase of latitude equal to 1° 18' 40". But it ought to have lengthened at least .00250. Thus there is a discrepancy between the experiments of the two observers, greater than between my formula and Captain Sabine's result.

Port Bowen is in the middle of Captain Sabine's northern stations. We may therefore employ Mr. Foster's experiment to compute the pendulums at those stations in different hypotheses of ellipticity, in order to compare them with the experimental determinations of Captain Sabine. Put l and λ for the length of the pendulum and the latitude, at Port Bowen; and let l' and λ' denote the same things for any of Captain Sabine's stations; then,

$$l' = l - f(\sin^2 \lambda - \sin^2 \lambda').$$

According to my formula, $f = 0.20835$; and according to Captain Sabine's calculations, $f = 0.20227$; and these values may be considered as nearly the greatest and least that can be assigned with any probability. Calculating, now, with these data, we get:

Station.	Observed pendulum.	Computed pendulum $f = 0.20835$	Computed pendulum $f = 0.20227$
Drontheim . .	39.17456	39.17920	39.17990
Hammerfest .	39.19519	39.19804	39.19820
Greenland . .	39.20335	39.20605	39.20597
Spitzbergen .	39.21469	39.21436	39.21404

The computed quantities are very consistent with my formula; but they do not agree well with the observed pendulums. In particular the discrepancy at Drontheim, computed from Mr. Foster's experiment on one side, is nearly equal to

what it was before found to be by calculating from Unst and Stockholm on the other side*.

I confine myself to these observations which must stand as long as any trust can be put in the rules of arithmetic. To venture upon any discussion concerning the cause of the singular discordance between Captain Sabine's experiments and those made by other observers, might possibly stir up an altercation of no pleasant kind.

Feb. 5, 1827.

J. IVORY.

XXXVII. *An Account of M. Longchamp's Theory of Nitrification; with an Extension of it.* By THOMAS GRAHAM, M.A.

To the Editors of the Philosophical Magazine and Annals of Philosophy.

Gentlemen,

M. LONGCHAMP, in a memoir read some time ago before the Academy of Sciences, and published lately in the *Annales de Chimie et de Physique*, (t. xxxiii. p. 1.) has developed a theory of the natural production of nitre in various soils, and superficially upon certain rocks. This theory, in its full detail, is, perhaps, not altogether new; for several of the opinions of which it consists have been advocated, or at least broached, by preceding chemists. But M. Longchamp has certainly the merit of confidently displaying these opinions in their full force, and of methodizing them into a consistent system. Of this theory we propose to give an account, as nearly as possible in the words of the author, and to subjoin certain speculations, with the view of supplying a material deficiency in the theory of M. Longchamp.

It may be premised that M. Longchamp confines himself to the production of the acid of the native nitrous salts, and very properly avoids any supposition of the production of their base, previously existing as fact and reason point out that it must be, and, unlike the nitric acid of these salts, incapable of a synthetic formation.

There is reason to doubt the original proposition of Glauber, and which as far as regards the nitric acid has been the prevailing theory to the present day, that "saltpetre is formed by the decomposition of animal and vegetable substances;" for nitrates form and are found in materials and in places which contain no vegetable or animal matter, and which have never been exposed to the emanations of animals.

* Phil. Mag. Oct. 1826, p. 251.