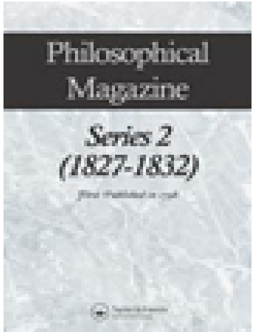

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I must apologize for having mentioned a subject which is not astronomical, on account of its intimate connexion with the preceding part of this paper.

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XXIX. *Comparison of a Formula representing the Velocity of Sound, with* Capt. Parry *and* Lieut. Foster's *Experiments on that Subject at Port Bowen; with some Remarks on the Ellipticity of the Earth. By* WM. GALBRAITH, *Esq. A.M.*

To the Editors of the Philosophical Magazine and Annals.

Gentlemen,

IN the first volume of the New Series of the Phil. Mag., p. 337, I have given two formulæ to determine the velocity of sound. It would be more convenient, however, to adapt that for the temperature by Fahrenheit's thermometer to zero of that scale, and it becomes

$$V = (102{\cdot}4225 + 0{\cdot}1103t)\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)(10{\cdot}2739 - 0{\cdot}0138\cos 2\lambda) + \omega\cos\phi.$$

This gives the velocity in English feet, when the English barometer and Fahrenheit's thermometer are used.

By a comparison of this formula adapted to the centigrade thermometer, I found an almost perfect accordance with Dr. Moll's experiments. I also found that the effect of wind on the 27th of June 1823 was about 19 feet,—half the difference between the velocities, as determined from each extremity of the base. Indeed there can be little doubt that the velocity of sound is affected by that of the wind at the time. Dr. Gregory of Woolwich, in a series of experiments on sound detailed in the first volume of the Transactions of the Cambridge Philosophical Society*, expressly states that the wind increases or diminishes the velocity of sound according as it blows in the same or in an opposite direction;—a conclusion which might almost *à priori* have been anticipated. The only difficulty is, to adapt the formula to the actual state of the atmosphere with regard to moisture. The expansion of the *dry air* with which Messrs. Dulong and Petit operated, was 0·375 from the freezing point to the boiling point of water. It is a little greater, however, in *moist air*, such as exists in an ordinary state of the atmosphere. Laplace in that case assumed 0·4, and from a mean of a great number of experiments on air, sound, &c. I found 0·4112, that adopted in the above formula.

* See Phil. Mag. vol. lxiii. p. 401.—EDIT.

Now

Now this is very nearly true in the usual state of the atmosphere, but in extreme cases of dryness and moisture it must vary a little from this, so that I have not been able to discover the exact quantity of variation. From such comparisons as I have been able to make, it seems, however, in its present state, to be pretty accurate. As I have already, in the volume referred to, shown its agreement with Professor Moll's experiments, I shall now compare it with those made by Captains Parry and Foster at Port Bowen; and as they had no anemometer to determine the velocity of the wind, I shall make a probable estimation of its effects, from Smeaton's table in the 51st volume of the Philosophical Transactions, as nearly as I can, from the account of the weather given along with the observations, and the angle between the direction of the wind and sound estimated to the nearest point, that being the degree of accuracy attainable only from the *data*, page 86, Appendix to the Third Voyage.

Experiments made at Port Bowen, in Latitude 73° 14′ *N. The extent of the measured Base was* 12892·89 *feet, and the bearings of the Gun S.* 71° 48′ *E.*

1824.	Bar. in Inches	Temp.	Wind.		Weather.	No. of Guns fired.	Interval in Seconds between Flash and Report.			Exp. Velocity per sec.
							P.	F.	Mean.	
Nov. 24	29·841	− 7°	E.S.E.	light	overcast	5	12s·3525	12s·4300	12s·3912	1040·49
Dec. 8	29·561	− 9	N.N.E.	squally	very clear	6	12 ·3310	12 ·5266	12 ·4288	1037·34
1825.										
Jan. 10	30·268	−37	E.S.E.	light	clear	4	12 ·5889	12 ·4700	12 ·5290	1029·04
Feb. 7	29·647	−24·5	N.E.	light	very clear	6	12 ·6390	12 ·6167	12 ·6278	1020·99
17	29·598	−18	calm	...	overcast	6	12 ·3720	12 ·4400	12 ·4060	1039·25
21	29·735	−37·5	calm	...	overcast	6	12 ·8167	12 ·7067	12 ·7617	1010·28
Mar. 2	30·398	−38·5	easterly	light	a little overcast	6	12 ·6400	12 ·7800	12 ·7100	1014·39
22	30·258	−21·5	westerly	light	very clear and fine	6	12 ·4000	12 ·7167	12 ·5583	1026·64
June 3	30·118	+33·5	easterly	light	very clear	6	11 ·7333	11 ·7440	11 ·7387	1098·32
4	30·102	+35	S.E.	strong, squally	clear	6	11 ·5889	11 ·4733	11 ·5311	1118·10

Now, by applying the above formula, in which t is the temperature by Fahrenheit's thermometer, f the elastic force of aqueous vapour, p the barometric pressure, λ the latitude, w the velocity of the wind, and ϕ the angle between the wind and sound. Above 0° Fahr. I have taken f according to the temperature marked, which cannot cause any great error.

1824.

1824.	Exp. Velocity in feet.	Contained Angle.	Calculated Velocity.	Estimated Effect of Wind.	Final Velocity.	Difference.
Nov. 24	1040·49	4° 18′	1045·51	+ 4·0	1049·5	+ 9·0
Dec. 8	1037·34	85 42	1043·24	+ 4·0	1047·2	+ 9·9
1825.						
Jan. 10	1029·04	4 18	1011·48	+ 4·0	1015·5	−13·5
Feb. 7	1020·99	63 12	1025·66	+ 2·0	1027·7	+ 6·7
17	1039·25	......	1033·03	......	1033·0	− 5·7
21	1010·28	......	1010·91	......	1010·9	+ 0·6
Mar. 2	1014·39	18 12	1009·78	+ 4·0	1013·8	− 0·6
22	1026·64	161 48	1029·06	− 4·0	1025·1	− 1·5
June 3	1098·32	18 12	1092·82	+ 4·0	1096·8	− 1·5
4	1118·10	16 48	1094·60	+25·0	1119·6	+ 1·5
			Mean error of the whole . . .			+ 4·9
			Of a single set			+ 0·5

In most of the above experiments, the experimental and calculated velocities approximate very closely. There is, no doubt, some uncertainty in the estimated effect of the wind, though it is believed it cannot be great. Perhaps it is a little too great in the first two experiments. I cannot reconcile the third very well by any probable supposition. The only one on which the effect of the wind is considerable, is the last, when it was *strong and squally, and blowing nearly in the direction of the sound.* Upon the whole, the comparison appears satisfactory, though it would have been less objectionable had the velocity of the wind been ascertained by experiment, and its direction more accurately observed.

I may add, that since my last communication on experiments by the pendulum, I have reconsidered the whole; and upon rejecting those evidently affected with *some cause not well explained*, I have found the following formula:

$$P = 39{\cdot}01326 + 0{\cdot}20686 \sin^2 (\lambda - \theta) \;\ldots\ldots\; (A)$$

In which P is the length of the pendulum, λ the observed latitude, and θ the reduction of the latitude.

Also $\epsilon = 0{\cdot}00330 = \frac{1}{303}$ very nearly;

And P at London by computation from formula (A) is 39·13937, while I have found it from Captain Kater's experiments to be 39·13938, almost exactly the same. P at Paris, by the same formula is 39·12982, or 0·00053 greater than by experiment. And these two instances show the great accuracy of the formula.

The

The most probable ellipticity by the pendulum-experiments appears to be, from my calculations, ...	0·00330
The same, from my comparison of degrees ...	0·00322
Mr. Ivory's investigations give from arcs	0·00324
Laplace adopted	0·00326
Mean of the whole	0·00327

It is probable that Mr. Ivory's ellipticity, or 0·00324, is the most accurate of the whole, and may safely be adopted as that to which it will ultimately converge, since it satisfies all the most accurate arcs hitherto measured with extreme precision.

But the most extraordinary circumstance attending all these comparisons is their discrepancy from those of Mr. Professor Airy, of Cambridge, who finds from Captain Sabine's pendulum-experiments 0·003474; and still more so the result of his comparison of arcs, which is 0·003589! *these arcs being the very same as those which Mr. Ivory and I have employed.* To what cause then must this discordance be attributed? Can it be supposed that the Professor has committed an error either in his investigations, or in his calculations, or in both? In such an important investigation it would be most desirable to see the whole scrutinized with great care, and this scrutiny would come with a better grace from the Professor himself than from any other individual. I am, Gentlemen, yours, &c.

Edinburgh, June 18, 1828. WILLIAM GALBRAITH.

XXX. *On the Reduction of Circummeridian Altitudes of the Sun**.

PROFESSOR GAUSS's ingenious method of effecting the usual reduction of circummeridian altitudes of the sun not having yet been noticed in any English work, the following deduction of the same will perhaps deserve a place in the Philosophical Magazine. Let

$\varphi =$ the latitude of the place of observation.

$\delta =$ the sun's declination at noon.

$\Delta\delta =$ the change of the sun's declination in 24 hours at noon expressed in seconds.

$-t =$ the number of seconds any observation was taken before noon.

$+t =$ the number of seconds any observation was taken after noon.

$O =$ the observed altitude of the sun for the time t.

$M =$ the meridian altitude of the sun.

* Communicated by the Author.

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