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XLVIII. *Æolian Tones.*

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IN what has long been known as the Æolian Harp, a stretched string, such as a pianoforte wire or a violin string, is caused to vibrate in one of its possible modes by the impact of wind; and it was usually supposed that the action was analogous to that of a violin bow, so that the vibrations were executed in the plane containing the direction of the wind. A closer examination showed, however, that this opinion was erroneous and that in fact the vibrations are transverse to the wind†. It is not essential to the production of sound that the string should take part in the vibration, and the general phenomenon, exemplified in the whistling of wind among trees, has been investigated by Strouhal‡ under the name of *Reibungstöne*.

In Strouhal's experiments a vertical wire or rod attached to a suitable frame was caused to revolve with uniform velocity about a parallel axis. The pitch of the æolian tone generated by the relative motion of the wire and of the air was found to be independent of the length and of the tension of the wire, but to vary with the diameter (D) and with the speed (V) of the motion. Within certain limits the relation

* Communicated by the Author.

† Phil. Mag. vol. vii. p. 149 (1879); Scientific Papers, vol. i. p. 413.

‡ Wied. *Ann.* vol. v. p. 216 (1878).

between the frequency of vibration (N) and these data was expressible by

$$N = \cdot 185\, V/D, \quad \ldots \ldots \quad (1)^*$$

the centimetre and the second being units.

When the speed is such that the æolian tone coincides with one of the proper tones of the wire, supported so as to be capable of free independent vibration, the sound is greatly reinforced, and with this advantage Strouhal found it possible to extend the range of his observations. Under the more extreme conditions then practicable the observed pitch deviated considerably from the value given by (1). He further showed that with a given diameter and a given speed a rise of temperature was attended by a fall in pitch.

If, as appears probable, the compressibility of the fluid may be left out of account, we may regard N as a function of the relative velocity V, D, and ν the kinematic coefficient of viscosity. In this case N is necessarily of the form

$$N = V/D \,.\, f(\nu/VD), \quad \ldots \ldots \quad (2)$$

where f represents an arbitrary function; and there is dynamical similarity, if $\nu \propto VD$. In observations upon air at one temperature ν is constant; and if D vary inversely as V, ND/V should be constant, a result fairly in harmony with the observations of Strouhal. Again, if the temperature rises, ν increases, and in order to accord with observation, we must suppose that the function f diminishes with increasing argument.

"An examination of the actual values in Strouhal's experiments shows that ν/VD was always small; and we are thus led to represent f by a few terms of MacLaurin's series. If we take

$$f(x) = a + bx + cx^2,$$

we get

$$N = a\frac{V}{D} + b\frac{\nu}{D^2} + c\frac{\nu^2}{VD^3}. \quad \ldots \ldots \quad (3)$$

" If the third term in (3) may be neglected, the relation between N and V is linear. This law was formulated by Strouhal, and his diagrams show that the coefficient b is negative, as is also required to express the observed effect of a rise of temperature. Further,

$$D\frac{dN}{dV} = a - \frac{c\nu^2}{V^2D^2}, \quad \ldots \ldots \quad (4)$$

* In (1) V is the velocity of the wire relatively to the walls of the laboratory.

so that $D \, . \, dN/dV$ is very nearly constant, a result also given by Strouhal on the basis of his measurements.

"On the whole it would appear that the phenomena are satisfactorily represented by (2) or (3), but a dynamical theory has yet to be given. It would be of interest to extend the experiments to liquids."*

Before the above paragraphs were written I had commenced a systematic deduction of the form of f from Strouhal's observations by plotting ND/V against VD. Lately I have returned to the subject, and I find that nearly all his results are fairly well represented by two terms of (3). In C.G.S. measure

$$\frac{ND}{V} = \cdot 195\left(1 - \frac{3 \cdot 02}{VD}\right). \quad . \quad . \quad . \quad . \quad (5)$$

Although the agreement is fairly good, there are signs that a change of wire introduces greater discrepancies than a change in V—a circumstance which may possibly be attributed to alterations in the character of the surface. The simple form (2) assumes that the wires are smooth, or else that the roughnesses are in proportion to D, so as to secure geometrical similarity.

The completion of (5) from the theoretical point of view requires the introduction of ν. The temperature for the experiments in which ν would enter most was about 20° C., and for this temperature

$$\nu = \frac{\mu}{\rho} = \frac{1806 \times 10^{-7}}{\cdot 00120} = \cdot 1505 \text{ C.G.S.}$$

The generalized form of (5) is accordingly

$$\frac{ND}{V} = \cdot 195\left(1 - \frac{20 \cdot 1\, \nu}{VD}\right), \quad . \quad . \quad . \quad . \quad (6)$$

applicable now to any fluid when the appropriate value of ν is introduced. For water at 15° C., $\nu = \cdot 0115$, much *less* than for air.

Strouhal's observations have recently been discussed by Krüger and Lauth†, who appear not to be acquainted with my theory. Although they do not introduce viscosity, they recognize that there is probably some cause for the observed deviations from the simplest formula (1), other than the complication arising from the circulation of the air set in

* 'Theory of Sound,' 2nd ed. vol. ii. § 372 (1896).

† "Theorie der Hiebtöne," *Ann. d. Physik,* vol. xliv. p. 801 (1914).

motion by the revolving parts of the apparatus. Undoubtedly this circulation marks a weak place in the method, and it is one not easy to deal with. On this account the numerical quantities in (6) may probably require some correction in order to express the true formula when V denotes the velocity of the wire through otherwise undisturbed fluid.

We may find confirmation of the view that viscosity enters into the question, much as in (6), from some observations of Strouhal on the effect of *temperature*. Changes in ν will tell most when VD is small, and therefore I take Strouhal's table XX., where D=·0179 cm. In this there appears

$$t_1=11^\circ,\quad V_1=385,\quad N_1/V_1=6{\cdot}70,\quad \nu_1,$$
$$t_2=31^\circ,\quad V_2=381,\quad N_2/V_2=6{\cdot}48,\quad \nu_2.$$

Introducing these into (6), we get

$$6{\cdot}70-6{\cdot}48=\frac{\cdot195}{D}\left(1-\frac{20{\cdot}1\,\nu_1}{V_1 D}\right)-\frac{\cdot195}{D}\left(1-\frac{20{\cdot}1\,\nu_2}{V_2 D}\right),$$

or with sufficient approximation

$$\nu_2-\nu_1=\frac{\cdot52\,D^2V}{\cdot195\times20{\cdot}1}=\cdot016 \text{ C.G.S.}$$

We may now compare this with the known values of ν for the temperatures in question. We have

$$\mu_{31}=1853\times10^{-7},\quad \rho_{31}=\cdot001161,$$
$$\mu_{11}=1765\times10^{-7},\quad \rho_{11}=\cdot001243;$$

so that

$$\nu_2=\cdot1596,\qquad \nu_1=\cdot1420,$$

and

$$\nu_2-\nu_1=\cdot018.$$

The difference in the values of ν at the two temperatures thus accounts in (6) for the change of frequency both in sign and in order of magnitude.

As regards dynamical explanation it was evident all along that the origin of vibration was connected with the instability of the vortex sheets which tend to form on the two sides of the obstacle, and that, at any rate when a wire is maintained in transverse vibration, the phenomenon must be unsymmetrical. The alternate formation in water of detached vortices on the two sides is clearly described by H. Bénard *.

* *C. R.* t. 147, p. 839 (1908).

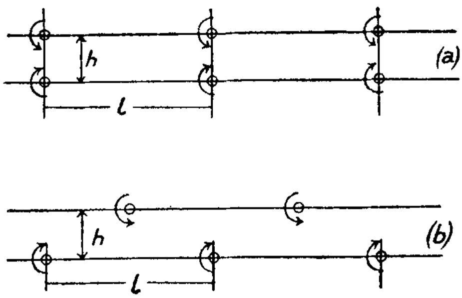
"Pour une vitesse suffisante, au-dessous de laquelle il n'y a pas de tourbillons (cette vitesse limite croît avec la viscosité et decroît quand l'épaisseur transversale des obstacles augmente), *les tourbillons produits périodiquement se detachent alternativement à droite et à gauche du remous d'arrière qui suit le solide; ils gagnent presque immédiatement leur emplacement définitif, de sorte qu'à l'arrière de l'obstacle se forme une double rangée alternée d'entonnoirs stationnaires, ceux de droite dextrogyres, ceux de gauche lévogyres, séparés par des intervalles égaux.*"

The symmetrical and unsymmetrical processions of vortices were also figured by Mallock* from direct observation.

In a remarkable theoretical investigation† Kármán has examined the question of the stability of such processions. The fluid is supposed to be incompressible, to be devoid of viscosity, and to move in two dimensions. The vortices are concentrated in points and are disposed at equal intervals (l) along two parallel lines distant h. Numerically the vortices are all equal, but those on different lines have opposite signs.

Apart from stability, steady motion is possible in two arrangements (a) and (b), fig. 1, of which (a) is symmetrical.

Fig. 1.



Kármán shows that (a) is always unstable, whatever may be the ratio of h to l; and further that (b) is usually unstable also. The single exception occurs when $\cosh(\pi h/l)=\sqrt{2}$, or $h/l=0{\cdot}283$. With this ratio of h/l, (b) is stable for every kind of displacement except one, for which there is neutrality.

* Proc. Roy. Soc. vol. lxxxiv. A. p. 490 (1910).

† *Göttingen Nachrichten,* 1912, Heft 5, S. 547; Kármán and Rubach, *Physik. Zeitschrift,* 1912, p. 49. I have verified the more important results.

The only procession which can possess a practical permanence is thus defined.

The corresponding motion is expressed by the complex potential (ϕ potential, ψ stream-function)

$$\phi + i\psi = \frac{i\zeta}{2\pi} \log \frac{\sin\{\pi(z_0 - z)/l\}}{\sin\{\pi(z_0 + z)/l\}}, \quad . \quad . \quad . \quad (7)$$

in which ζ denotes the strength of a vortex, $z = x + iy$, $z_0 = \frac{1}{4}l + ih$. The x-axis is drawn midway between the two lines of vortices and the y-axis halves the distance between neighbouring vortices with opposite rotation. Kármán gives a drawing of the stream-lines thus defined.

The constant velocity of the processions is given by

$$u = \frac{\zeta}{2l} \tanh \frac{\pi h}{l} = \frac{\zeta}{l\sqrt{8}} \cdot \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This velocity is relative to the fluid at a distance.

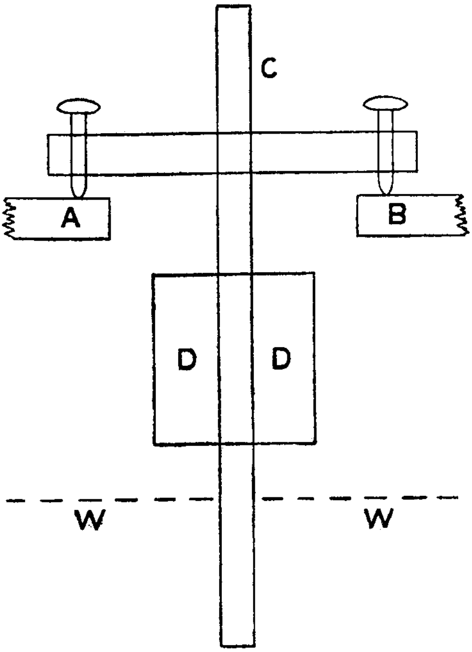
The observers who have experimented upon water seem all to have used obstacles not susceptible of vibration. For many years I have had it in mind to repeat the æolian harp effect with water*, but only recently have brought the matter to a test. The water was contained in a basin, about 36 cm. in diameter, which stood upon a sort of turn-table. The upper part, however, was not properly a table, but was formed of two horizontal beams crossing one another at right angles, so that the whole apparatus resembled rather a turn-*stile*, with four spokes. It had been intended to drive from a small water-engine, but ultimately it was found that all that was needed could more conveniently be done by hand after a little practice. A metronome beat approximate half seconds, and the spokes (which projected beyond the basin) were pushed gently by one or both hands until the rotation was uniform with passage of one or two spokes in correspondence with an assigned number of beats. It was necessary to allow several minutes in order to make sure that the water had attained its ultimate velocity. The axis of rotation was indicated by a pointer affixed to a small

* From an old note-book. "Bath, Jan. 1884. I find in the baths here that if the spread fingers be drawn pretty quickly through the water (palm foremost was best), they are thrown into transverse vibration and strike one another. This seems like æolian string..... The blade of a flesh-brush about $1\frac{1}{2}$ inch broad seemed to vibrate transversely in its own plane when moved through water broadways forward. It is pretty certain that with proper apparatus these vibrations might be developed and observed."

stand resting on the bottom of the basin and rising slightly above the level of the water.

The pendulum (fig. 2), of which the lower part was immersed, was supported on two points (A, B) so that the

Fig. 2.



possible vibrations were limited to one vertical plane. In the usual arrangement the vibrations of the rod would be radial, *i. e.* transverse to the motion of the water, but it was easy to turn the pendulum round when it was desired to test whether a circumferential vibration could be maintained. The rod C itself was of brass tube $8\frac{1}{2}$ mm. in diameter, and to it was clamped a hollow cylinder of lead D. The time of complete vibration (τ) was about half a second. When it was desired to change the diameter of the immersed part, the rod C was drawn up higher and prolonged below by an additional piece—a change which did not much affect the period τ. In all cases the length of the part immersed was about 6 cm.

Preliminary observations showed that in no case were vibrations generated when the pendulum was so mounted that the motion of the rod would be circumferential, viz. in the direction of the stream, agreeably to what had been found for the æolian harp. In what follows the vibrations, if any, are radial, that is transverse to the stream.

In conducting a set of observations it was found convenient to begin with the highest speed, passing after a sufficient time to the next lower, and so on, with the minimum of intermission. I will take an example relating to the main rod, whose diameter (D) is $8\frac{1}{2}$ mm., $\tau=60/106$ sec., beats of metronome 62 in 30 sec. The speed is recorded by the number of beats corresponding to the passage of *two* spokes, and the vibration of the pendulum (after the lapse of a sufficient time) is described as small, fair, good, and so on. Thus on Dec. 21, 1914:

2 spokes to 4 beats gave fair vibration,
. . . . 5 . . . . good . . . .
. . . . 6 . . . . . rather more . . . .
. . . . 7 . . . . good . . . .
. . . . 8 . . . . fair . . . .

from which we may conclude that the maximum effect corresponds to 6 beats, or to a time (T) of revolution of the turn-table equal to $2\times 6\times 30/62$ sec. The distance (r) of the rod from the axis of rotation was 116 mm., and the speed of the water, supposed to move with the basin, is $2\pi r/\mathrm{T}$. The result of the observations may intelligibly be expressed by the ratio of the distance travelled by the water during one complete vibration of the pendulum to the diameter of the latter, viz.

$$\frac{\tau\,.\,2\pi r/\mathrm{T}}{\mathrm{D}}=\frac{2\pi\times 116\times 62}{8{\cdot}5\times 6\times 106}=8{\cdot}36.$$

Concordant numbers were obtained on other occasions.

In the above calculation the speed of the water is taken as if it were rigidly connected with the basin, and must be an over estimate. When the pendulum is away, the water may be observed to move as a solid body after the rotation has been continued for two or three minutes. For this purpose the otherwise clean surface may be lightly dusted over with sulphur. But when the pendulum is immersed, the rotation is evidently hindered, and that not merely in the neighbourhood of the pendulum itself. The difficulty thence arising has already been referred to in connexion with Strouhal's experiments and it cannot easily be met in its entirety. It may be mitigated by increasing r, or by diminishing D. The latter remedy is easily applied up to a certain point, and I have experimented with rods 5 mm. and $3\frac{1}{2}$ mm. in diameter. With a 2 mm. rod no vibration could be observed. The final results were thus tabulated:—

Diameter ...	8·5 mm.	5·0 mm.	3·5 mm.
Ratio	8·35	7·5	7·8

from which it would appear that the disturbance is not very serious. The difference between the ratios for the 5·0 mm. and 3·5 mm. rods is hardly outside the limits of error; and the prospect of reducing the ratio much below 7 seemed remote.

The instinct of an experimenter is to try to get rid of a disturbance, even though only partially; but it is often equally instructive to increase it. The observations of Dec. 21 were made with this object in view; besides those already given they included others in which the disturbance due to the vibrating pendulum was augmented by the addition of a similar rod ($8\frac{1}{2}$ mm.) immersed to the same depth and situated symmetrically on the same diameter of the basin. The anomalous effect would thus be doubled. The record was as follows:—

2 spokes to 3 beats gave little or no vibration,
.... 4 .... fair ......
.... 5 .... large ......
.... 6 .... less ......
.... 7 .... little or no ......

As the result of this and another day's similar observations it was concluded that the 5 beats with additional obstruction corresponded with 6 beats without it. An approximate correction for the disturbance due to improper action of the pendulum may thus be arrived at by decreasing the calculated ratio in the proportion of 6 : 5; thus

$$\tfrac{5}{6}(8{\cdot}35)=7{\cdot}0$$

is the ratio to be expected in a uniform stream. It would seem that this cannot be far from the mark, as representing the travel at a distance from the pendulum in an otherwise uniform stream during the time of one complete vibration of the latter. Since the correction for the other diameters will be decidedly less, the above number may be considered to apply to all three diameters experimented on.

In order to compare with results obtained from air, we must know the value of ν/VD. For water at 15° C. $\nu=\mu=\cdot0115$ C.G.S.; and for the 8·5 mm. pendulum $\nu/\mathrm{VD}=\cdot0011$. Thus from (6) it appears that ND/V should have nearly the full value, say ·190. The reciprocal of this, or 5·3, should agree with the ratio found above as 7·0; and the discrepancy is larger than it should be.

An experiment to try whether a change of viscosity had appreciable influence may be briefly mentioned. Observations were made upon water heated to about 60° C. and at 12° C. No difference of behaviour was detected. At 60° C. $\mu = \cdot 0049$, and at 12° C. $\mu = \cdot 0124$.

I have described the simple pendulum apparatus in some detail, as apart from any question of measurements it demonstrates easily the general principle that the vibrations are transverse to the stream, and when in good action it exhibits very well the double row of vortices as witnessed by dimples upon the surface of the water.

The discrepancy found between the number from water (7·0) and that derived from Strouhal's experiments on air (5·3) raises the question whether the latter can be in error. So far as I know, Strouhal's work has not been repeated; but the error most to be feared, that arising from the circulation of the air, acts in the wrong direction. In the hope of further light I have remounted my apparatus of 1879. The draught is obtained from a chimney. A structure of wood and paper is fitted to the fireplace, which may prevent all access of air to the chimney except through an elongated horizontal aperture in the front (vertical) wall. The length of the aperture is 26 inches (66 cm.), and the width 4 inches (10·2 cm.); and along its middle a gut string is stretched over bridges.

The draught is regulated mainly by the amount of fire. It is well to have a margin, as it is easy to shunt a part through an aperture at the top of the enclosure, which can be closed partially or almost wholly by a superposed card. An adjustment can sometimes be got by opening a door or window. A piece of paper thrown on the fire increases the draught considerably for about half a minute.

The string employed had a diameter of ·95 mm., and it could readily be made to vibrate (in 3 segments) in unison with a fork of pitch 256. The octave, not difficult to mistake, was verified by a resonator brought up close to the string. That the vibration is transverse to the wind is confirmed by the behaviour of the resonator, which goes out of action when held symmetrically. The sound, as heard in the open without assistance, was usually feeble, but became loud when the ear was held close to the wooden frame. The difficulty of the experiment is to determine the velocity of the wind, where it acts upon the string. I have attempted to do this by a pendulum arrangement designed to determine the wind by its action upon an elongated piece of mirror (10·1 cm. × 1·6 cm.) held perpendicularly and just in front of the string. The pendulum is supported on two points—in this respect like the one used for the water experiments; the mirror is above, and there is a counter-weight below. An arm projects horizontally forward on which a rider can be placed. In commencing observations the wind is cut off by a large card inserted across the aperture and just behind the string. The pendulum then assumes a sighted position, determined in the usual way by reflexion. When the wind operates the mirror is carried with it, but is brought back to the sighted position by use of a rider of mass equal to ·485 gm.

Observations have been taken on several occasions, but it will suffice to record one set whose result is about equal to the average. The (horizontal) distance of the rider from the axis of rotation was 62 mm., and the vertical distance of the centre line of the mirror from the same axis is 77 mm. The force of the wind upon the mirror was thus $62 \times \cdot485 \div 77$ gms. weight. The mean pressure P is

$$\frac{62 \times \cdot485 \times 981}{77 \times 16\cdot2} = 23\cdot7 \frac{\text{dynes}}{\text{cm.}^2}.$$

The formula connecting the velocity of the wind V with the pressure P may be written

$$P = C\rho V^2,$$

where ρ is the density; but there is some uncertainty as to the constancy of C. It appears that for large plates $C = \cdot62$, but for a plate 2 inches square Stanton found $C = \cdot52$. Taking the latter value*, we have

$$V^2 = \frac{23\cdot7}{\cdot52\rho} = \frac{23\cdot7}{\cdot52 \times \cdot00123},$$

on introduction of the value of ρ appropriate to the circumstances of the experiment. Accordingly

$$V = 192 \text{ cm./sec.}$$

The frequency of vibration (τ^{-1}) was nearly enough 256; so that

$$\frac{V\tau}{D} = \frac{192}{256 \times \cdot095} = 7\cdot9.$$

In comparing this with Strouhal, we must introduce the

* But I confess that I feel doubts as to the diminution of C with the linear dimension.

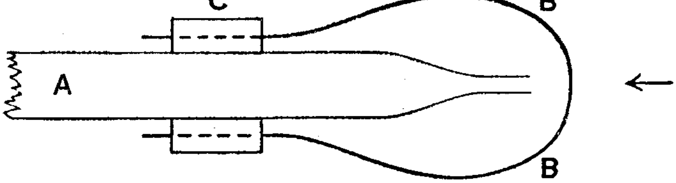
appropriate value of VD, that is 19, into (5). Thus

$$\frac{V}{ND}=\frac{V\tau}{D}=6\cdot 1.$$

Whether judged from the experiments with water or from those just detailed upon air, this (Strouhal's) number would seem to be too low; but the uncertainty in the value of C above referred to precludes any very confident conclusion. It is highly desirable that Strouhal's number should be further checked by some method justifying complete confidence.

When a wire or string exposed to wind does not itself enter into vibration, the sound produced is uncertain and difficult to estimate. No doubt the wind is often different at different parts of the string, and even at the same part it may fluctuate rapidly. A remedy for the first named cause of unsteadiness is to listen through a tube, whose open end is brought pretty close to the obstacle. This method is specially advantageous if we take advantage of our knowledge respecting the mode of action, by using a tube drawn out to a narrow bore (say 1 or 2 mm.) and placed so as to face the processions of vortices behind the wire. In connexion with the fire-place arrangement the drawn out glass tube is conveniently bent round through 180° and continued to the ear by a rubber prolongation. In the wake of the obstacle the sound is well heard, even at some distance (50 mm.) behind; but little or nothing reaches the ear when the aperture is in front or at the side, even though quite close up, unless the wire is itself vibrating, But the special arrangement for a draught, where the observer is on the high pressure side, is not necessary; in a few minutes any one may prepare a little apparatus competent to show the effect. Fig. 3 almost explains itself. A is the drawn out glass tube;

Fig. 3.



B the loop of iron or brass wire (say 1 mm. in diameter), attached to the tube with the aid of a cork C. The rubber prolongation is not shown. Held in the crack of a slightly opened door or window, the arrangement yields a sound which is often pure and fairly steady.