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XXVII. *Acoustical Observations.* II.

By LORD RAYLEIGH, *F.R.S.**

Pure Tones from Sounding Flames.

THE best approximation to a pure tone is doubtless that given by a fork held over a suitably tuned air resonator; but unless the vibrations are maintained, the sound is of but short duration, and varies in intensity throughout. On the other hand the introduction of an electro-magnetic maintenance (as in Helmholtz's vowel experiments) somewhat complicates the apparatus. For many purposes extreme purity and constancy of pitch are not important; and thus an arrangement which shall be simple and easy to manage, even though less perfect in its operation than a tuning fork, is still a desideratum.

During the last year I have often used with good effect air resonators whose vibrations were maintained in a well-known manner by hydrogen-flames. In the common form of the experiment an open cylindrical tube is employed as resonator, and gives a sound, usually of a highly compound character. In order to obtain a pure tone, it is only necessary to replace the tube by a resonator of different form, such as a rather wide-mouthed bottle or jar; but a difficulty then arises from the progressive deterioration of the limited quantity of air included. A better result is obtained from a tube with a central expansion, such as a bulbous paraffin-lamp chimney, which allows of a through draught, and yet departs sufficiently

* Communicated by the Author.

from the cylindrical form to give a pure tone. For ready speech, it is sometimes necessary to restrict the lower aperture, *e. g.* by a bored disk of wood attached with wax. Another plan which answers very well is to block the middle of a cylindrical tube by a loosely fitting plug. The tubes that I used are of cast iron, and were plugged by rectangular pieces of wood provided with springs of brass wire to keep them in position. The length of the plug may be about two diameters of the tube; the length of the tube itself should be about twelve diameters. In all cases the best result requires that the tubes through which the hydrogen is supplied be of suitable length, and be provided with suitable burners. These may be made of glass, and are easily adjusted by trial.

For ordinary purposes a common hydrogen-bottle is sufficient; but the note is rather more steady when the hydrogen is supplied from a gas-holder. In this way I have obtained pure tones, giving with tuning forks pretty steady beats of more than two seconds' period. When the intensities are nearly equal, the phase of approximate silence is very well marked.

Points of silence near a wall from which a pure tone is reflected.

On this subject there are two papers by N. Savart *, who advances views very difficult of acceptance. A criticism of some of Savart's positions was published soon after by Seebeck; but the question does not appear to have been thoroughly cleared up.

One source of confusion is imperfect recognition of the fact that the positions of the silences depend upon the nature of the apparatus used for the investigation. In the case of the ear a silence requires that there be no variation of pressure at the open end of the ear-passage, whether it be in its natural state, or prolonged by a tube fitted into the external ear. The addition of a small cone or resonator will not affect the truth of this statement. Thus, if the influence of the head and body of the observer acting as simple obstacles be put out of account (as may fairly be done when a tube is used), the silences occur at distances from the wall which are *odd* multiples of the quarter wave-length†. On the other hand, if a membrane simply stretched over a hoop and held parallel to the wall be used as the indicator, the positions of zero disturbance are at distances from the wall equal to *even* multiples of the quarter wave-length.

In the theory of organ-pipes the places of zero velocity

* *Ann. d. Chim.* lxxi. 1839, xl. 1845.

† The waves are here supposed complete. Savart's "ondes" are only half as long.

and of maximum pressure-variation are usually called *nodes*; and the places of zero pressure-variation and of maximum velocity are called *loops*. If we retain this nomenclature, we may say that silences as investigated by the ear occur at loops, and that the maximum sound is found at nodes; but in Savart's papers the silences are identified with *nodes*. Moreover the difference is not one of words merely; for Savart considers that (apart from the effects of obstacles) the silences are to be found at distances from the wall which are *even* multiples of the quarter wave-length. A large part of his work is thus an endeavour to bring the facts into accordance with a mistaken theoretical view.

When the median plane is parallel to the wall, the obstruction presented by the head displaces considerably the positions of the silences. In his first paper Savart proposes to add 27 mm. to the measured distances between the external ear nearer to the wall and the wall itself, in order to take account of the interval between the external ear and the sentient apparatus. In the case of the ear further from the wall a similar distance is to be subtracted. I am at a loss to understand how the situation of the sentient apparatus can be supposed to be an element in the question at all. Every thing must surely depend upon whether there is or is not a variation of pressure at the outer end of the ear-passage. In the second paper Savart takes (as it appears to me) a further step in the wrong direction. He states that the positions of the silences are the same, whether they be observed with the ear nearer to the wall, or with the ear further from it, and draws the conclusion that the part of the head with which we have to deal is that situated in the median plane midway between the ears. Having already added 27 mm. to his measurements (in the case of the ear nearer to the wall), to take account of the distance between the external ear and the labyrinth, he now adds 50 mm. more. By this artificial treatment the distances of the silences from the wall are made to agree with the series of even multiples of the quarter wave-length, though considerable anomalies remain unexplained.

There can be no doubt, I imagine, that Savart's theoretical views are quite erroneous, and that what has to be explained by the action of the head as an obstacle is the displacement of the silences from the loops, and not from the nodes. An exact theoretical investigation of this subject is of course out of the question; but some information bearing upon it may be obtained from a calculation given in my 'Theory of Sound,' § 328, relating to the character of the obstruction to sound presented by rigid spheres. It appears that if a source of sound

be situated at the surface of a sphere whose circumference is moderate in comparison with the wave-length, the *phase* (which is the element on which the phenomena under consideration principally depend) at a distance is approximately the same as if the source were moved outward from the surface through a distance equal to half the radius, and the sphere were removed altogether. By the theorem of reciprocity, § 294, it follows that in the case of reflection of plane waves there is a silence at the point on the surface of the sphere nearest the wall when, not this point itself, but another further from the centre by half the radius, is distant from the wall by an odd multiple of the quarter wave-length, provided that the distance between the sphere and wall be not too small a multiple of the radius. Instead therefore of adding with Savart 27 mm., or 77 mm., to the observed distances in the expectation of so arriving at even multiples of the quarter wave-length, we ought rather to subtract some such distance as 50 mm. in the expectation of arriving at odd multiples of the same quantity.

The following are some of Savart's results given in the first paper :—

Designations des divers points.	Distances des points à la paroi. mètres.
Paroi.	0·000
1^{r} ventre.	·148
1^{r} nœud.	·373
2e ventre.	·716
2e nœud.	1·000
3e ventre.	1·358
3e nœud.	1·615
4e ventre.	1·997
4e nœud.	2·275

If we subtract $27+50$ $(=77)$ from Savart's numbers for nodes we get

·296, ·923, 1·538, 2·198,

corresponding to

$\frac{1}{2}$ (·619), $\frac{3}{2}$ (·619,) $\frac{5}{2}$ (·619), $\frac{7}{2}$ (619),

or to

·309, ·927, 1·546, 2·163 ;

·619 being the value of the half-wave (onde). The "ventre" between the wall and the first node does not belong to the regular system at all.

From a theoretical point of view, it appeared to me highly improbable that the silences for the two ears should occur in the same position of the head, except perhaps in the case of a

particular wave-length equal to about three diameters; and laboratory experiments with steadily maintained tones had made me familiar with the phenomenon of sounds apparently transferring themselves from one ear to the other when the head is moved ; but I thought it desirable to try a few experiments in the open air especially directed to the examination of this point.

The source of sound was a lamp-chimney and hydrogen-flame, as described above, of pitch e' flat, so that the quarter wave-length was about eleven inches. The apparatus was placed at distances varying from about 18 to 50 feet in front of a tolerably flat wall; and the observer, with one ear stopped, investigated the positions of the silences, holding the middle plane of his head parallel to the wall. Although the positions of the silences were not very well defined, presumably in consequence of unequal amplitudes of the superposed vibrations, the most inattentive observer could not fail to notice that it was necessary to move the head considerably in order to pass from a place where the sound was a minimum for one ear to a place where it was a minimum for the other ear. We may therefore conclude that Savart's statement is not generally true, and that the views which he has founded upon it have no sufficient claim upon our acceptance.

When the median plane of the head is perpendicular to the wall, the silences are observed at distances which are *odd* multiples of the quarter wave-length, agreeably with theory.

Sensitive Flames.

The beautiful phenomenon of sensitive flames is now familiar to students of acoustics; but its rationale is by no means understood. An important contribution to the facts, from which some day a theory will doubtless arise, is contained in the observation of Prof. Tyndall as to the "seat of sensitiveness." My present purpose is to bring forward another fact which also will probably be found important. It may be thus stated. Under the action of stationary sonorous waves a flame is excited at *loops* and *not* at *nodes*.

The source of sound was a little contrivance on the principle of the bird-call, blown from a well-regulated bellows. Probably a very high organ-pipe or whistle might be employed; but it is necessary to use a nearly pure tone; and the pitch must be high, or the flame will be not affected sufficiently. At a distance of a few feet the sound was reflected perpendicularly from a large board. The flame itself was that called by Tyndall the vowel flame, issuing from a pin-hole steatite burner fed from a gas-holder with gas at high pressure (9 or 10 inches).

The observations were made by moving the burner to and fro in front of the board, until the positions were found in which the flame was least disturbed by the sound; and it will be seen from the results that these positions were very well defined. The distances between the board and the orifice of the burner was then taken with a pair of dividers, and measured on a scale of millimetres. Two observers, J and E, adjusted the flame independently of one another. The following are the results obtained:—

First position	J. $16\frac{1}{2}$, $16\frac{1}{2}$ ..... mean $16\frac{1}{2}$ E. 16, 16 .. .. „ 16	$16\frac{1}{4}$	16·25
Second position.....	J. 31, $31\frac{1}{2}$ ..... „ $31\frac{1}{2}$ E. $32\frac{1}{2}$, 31, 32 ..... „ $31\frac{3}{4}$	$31\frac{1}{2}$	15·7
Third position	J. 47, $47\frac{1}{2}$ ..... „ $47\frac{1}{4}$ E. $46\frac{1}{2}$, 47, $45\frac{1}{2}$ ... „ $46\frac{3}{4}$	$46\frac{3}{4}$	15·6
Fourth position ...	J. 62, $62\frac{1}{2}$ ..... „ $62\frac{1}{4}$ E. 64, $60\frac{1}{2}$, $62\frac{1}{2}$ ... „ $62\frac{1}{4}$	$62\frac{1}{4}$	15·6
Fifth position	J. $78\frac{1}{2}$, $78\frac{1}{2}$ ..... „ $78\frac{1}{2}$	$78\frac{1}{2}$	15·5

The second column contains the individual measurements by the two observers; the third shows the mean of all the results for the same position. The numbers in the fourth column are the results of dividing those of the third column by 1, 2, 3, 4, 5, respectively, and agree very well together, proving that the positions coincide with nodes. If the positions had coincided with loops, the numbers of the third column would have been in the ratios 1 : 3 : 5 : 7 : 9. The wave-length of the sound was thus 31·2 mm., corresponding to pitch $f\sharp^{vi}$.

A few observations were made at the same time on the positions of the silences, as estimated by the ear listening through a tube. As was to be expected, they coincided with the loops, bisecting the intervals given by the flame. When the flame was in a position of minimum effect, and the free end of the tube was held close to the burner at an equal distance from the reflecting wall, the sound heard was a maximum, and diminished when the end of the tube was displaced a little in either direction. It may therefore be taken as established that the flame is affected where the ear would not be affected, and *vice versâ*.

Aerial Vibrations of very Low Pitch maintained by Flames.

In a lecture "On the Explanation of certain Acoustical Phenomena," * I showed the production of a pure tone of about 95 vibrations per second from a glass resonator and a

* Proceedings of the Royal Institution, March 15, 1875. Nature, vol. xviii. p. 319.

hydrogen-flame. With a larger resonator of the same kind —a globe with a short neck, intended for showing the combustion of phosphorus in oxygen, the pitch is 64 vibrations per second. I have lately made some further experiments, with the view of finding whether there is any obstacle to the maintenance by flames of vibrations of still lower frequency. The resonator, whose natural pitch is 64, was fitted with a paste-board tube 2 inches in diameter and 14 inches long. In this condition its calculated frequency * is about 25 ; and it was found that vibrations could be maintained by a hydrogen-flame, or even by a flame of common gas. The supply-tube should be of considerable length ; and the orifice must not be much contracted. Although the intensity of vibration was such as to make it a matter of difficulty to keep the flame alight, scarcely any thing could be heard. I saw no reason to doubt that still slower vibrations might be maintained by flames.

In illustration of the mechanics of this subject, an apparatus was contrived, in which by the aid of electricity a periodic communication of heat to a limited mass of air could be effected. By means of a perforated cork one leg of a U-tube containing mercury was fitted air-tight to the neck of an inverted bottle of about 200 cub. centims. capacity. The diameter of the column of mercury was about 1 centim., and the length of the column about 25 centims. The combination constituted a resonator, differing from an ordinary air resonator by the substitution of mercury for air in the channel joining the interior of the vessel with the external atmosphere. Inside the bottle was a spiral of fine platinum wire, at one end in communication through the cork with one pole of a battery of two or three small Grove cells. The other end of the platinum spiral was connected with a copper wire, which terminated in the U-tube near the equilibrium-position of the mercury-surface. The second pole of the battery was in permanent connexion with the outer extremity of the mercury column. As the mercury vibrates, the circuit is periodically completed and broken. The current passes, and the platinum wire glows, when the mercury rises in the leg connected with the bottle. Thus the communication of heat occurs when the air in the interior is *condensed* by the vibration, which is the necessary condition for maintenance, as is explained in the lecture referred to.

Rijke's Notes on a large scale.

The production of sound in tubes by heated gauze was

* 'Theory of Sound,' vol. ii. § 307 (8).

discovered by Rijke*, and is perhaps the most interesting of all the cases in which vibrations are maintained by heat. The probable explanation is given in the Royal-Institution lecture. It is surprising that the phenomenon is not more generally known, as on a large scale the effect is extremely striking. I have employed a cast-iron pipe 5 feet long and $4\frac{3}{4}$ inches in diameter, hung over a table from a beam in the the roof of my laboratory. The gauze (iron wire) is of about 32 meshes to the linear inch, and may advantageously be used in two thicknesses. It should be moulded with a hammer on a circular wooden block of somewhat smaller diameter than that of the pipe, and will then retain its position in the pipe by friction. When it is desired to produce the sound, the gauze caps are pushed up the pipe to a distance of about a foot, and a gas-flame from a large rose-burner is adjusted underneath, at such a level as to heat the gauze to a bright red heat. For this purpose the vertical tube of the lamp should be prolonged, if necessary, by an additional length of brass tubing. In making the adjustment a more convenient view of the interior of the pipe is obtained with the aid of a small piece of looking-glass held obliquely underneath. Sometimes a sound is excited by the flame itself independently of the gauze. This should be avoided if possible, as it impedes the due heating of the gauze. When a good red heat is attained the flame is suddenly removed, either by withdrawing the lamp or by stopping the supply of gas. In about a second the sound begins, and presently rises to such intensity as to shake the room, after which it gradually dies away. The whole duration of the sound may be about 10 seconds.

Mutual Influence of Organ-Pipes nearly in unison.

The easiest way of approaching the consideration of this subject is to take the case of an open or stopped pipe, divided into two similar parts by a rigid barrier along its middle plane. In the absence of the barrier, the vibrations of the two halves under the action of the wind are in the same phase; and at first sight there appears to be no reason why this state of things should be disturbed by the barrier. Nevertheless it is well known to physicists that the two halves do in fact take opposite vibrations, with the result that the sound in the external air at a distance from the compound pipe is a small fraction only of that due to either half acting alone. In the pipe itself the vibration is *more*, and not less, intense on account of the barrier. It is true that at the very beginning of the sound,

* *Pogg. Ann.* cvii. 339, 1859.

when the wind first comes on, the vibrations in the two halves are similar, as is evidenced by the greater loudness; but the opposition of phase is rapidly established, usually in a fraction of a second of time. As a system with two degrees of freedom, the compound pipe is capable of two distinct modes of vibration, in one of which the vibrations of the component pipes are in the same phase, and in the other in opposite phases. Why the action of the wind should maintain the latter mode of vibration to the exclusion of the former has not hitherto been explained; but the fact remains that that mode of vibration, which depends for its possibility upon the barrier, is chosen in preference to the other mode, which is not dependent upon the barrier, and in the absence of the barrier is the one necessarily adopted.

The two possible modes of vibration have, as in almost all such cases, two distinct periods of vibration, the difference depending upon the behaviour of the air just outside the open ends. In consequence of the inertia of the external air at an open end, the effective length of a pipe exceeds its actual length by about six tenths of the radius. The increment of effective length is therefore greater in the case of the compound column of air when its parts vibrate in the same phase, than it would be for either of the parts if removed from the influence of the other. On the other hand, when the vibrations are in opposite phases, the increment must be much less, one component pipe absorbing the air discharged from the other. Accordingly one note of the compound pipe is graver, and the other, which is the one actually sounded, is more acute, than the natural notes of the component pipes when supposed to act independently of one another.

In order to show this effect it is not necessary that the two pipes be similar, or even of exactly the same pitch. If two pipes in approximate unison be placed so that their open ends are contiguous, a mutual influence is exerted, which is usually sufficient to prevent the production of beats. The examples about to be given will show that the unison need not be exact; but the greater the deviation from unison, the more intense is the residual sound. Beyond the limit of the admissible departure from unison, beats ensue; but at first they are irregular, and liable to be disturbed by very slight causes, such as draughts of air. According to theory, the frequency of the beats ought to be a little greater than the difference of the frequencies of the notes given by the pipes independently; but I have not been able to detect the difference experimentally. It would therefore seem that over most of the range for which the mutual influence is sensible and

regular, it is sufficiently powerful to prevent more than one note being sounded.

In the experiments that I have tried, the pipes were blown from a bellows provided with a special regulator, and the pitches of the various notes were determined by counting the beats for 20 seconds between them and a somewhat sharper note on a harmonium. Sometimes the blown ends of the pipes were near together, and sometimes (in the case of open pipes) the unblown ends ; but during the course of an experiment the positions of the pipes were not altered. In order to prevent a pipe speaking, I placed some cotton wool over the wind-way, and sometimes inserted a stopper; so that the pitch of the pipe as a resonator was entirely altered. The following are the details of some of the observations :—

I. Sept. 23. Open metal pipes about 2 feet long, one of them provided with an adjustable paper slider for modifying the pitch. Blown ends near one another; unblown ends distant.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
4·5, 4·5	5·0, 4·8	3·2, 3·1 ;

So that the note given by both pipes together is decidedly sharper than those of the separate pipes.

II. Sept. 23. Same pipes as in I. Unblown ends near one another ; blown ends distant.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
4·8	5·1	3·8

III. Sept. 25. Same pipes placed parallel to one another at a distance of about 14 inches.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
5·15, 5·20	5·30, 5·45	5·00, 5·15

The note of both pipes together is somewhat higher than the notes of the single pipes.

IV. Sept. 26. Same pipes. Unblown ends near ; blown ends distant.

Beats per second.

One pipe alone.	Other pipe alone.	Both pipes together.
5·80, 5·85	7·15, 7·50, 7·45	5·35, 5·50, 5·45

V. Sept. 26. Two bottles, tuned with water to about g, were blown by wind issuing from flattened tubes connected

with the bellows by lengths of india-rubber tubing. When the bottles were sufficiently removed from one another, the mutual influence was very small, being insufficient to prevent the formation of slow and pretty steady beats of about four seconds' period. This experiment shows that the mutual influence depends upon the proximity of the open ends of the pipes, and not upon any effects propagated through the supply-pipes leading from a common bellows.

Some further remarks on this subject will be found in a paper read before the Musical Association, Dec. 2, 1878. Reference may also be made to some allied experiments by Gripon*, with which I have only lately become acquainted. They appear scarcely to extend to the case with which I have principally occupied myself, namely that in which both pipes are blown. M. Gripon had, however, anticipated me in the experimental determination of the effect of a flange in modifying the correction for an open end† of a pipe.

Kettledrums.

The theory of the vibrations of uniform and uniformly stretched flexible circular membranes, vibrating *in vacuo*, has been known for many years‡. In practice deviations from such theoretical results are to be expected, if only in consequence of the reaction of the air, which must operate with considerable force on a vibrating body exposing so large a surface in proportion to its mass. In the case of kettledrums, the problem is further complicated by the action of the shell, which limits the motion of the air on one side of the membrane.

From the fact § that kettledrums are struck, not in the centre, but at a point about midway between the centre and edge, we may infer that the vibrations which it is desired to excite are not of the symmetrical class. I find, indeed, that the sound undergoes little, if any, change when the central point is touched by the finger. Putting therefore the symmetrical vibrations out of account, we have to consider the parts played by vibrations of the following modes :—(1) that with one nodal diameter and no nodal circle ; (2) that with two nodal diameters and no nodal circle; (3) that with three nodal diameters and no nodal circle ; (4) that with one nodal diameter and one nodal circle, &c. The investigation proved to be of greater difficulty than I had expected, partly in consequence of the short duration of the sounds. Better ears than mine

* *Ann. d. Chim.* iii. p. 371, 1874.
† Phil. Mag. June 1877.
‡ 'Theory of Sound,' ch. ix.
§ De Pontigny. Proceedings of the Musical Association, Feb. 1876.

are liable to be puzzled in attempting to analyze compound sounds of such complication and irregularity. The following results, however, are believed to be trustworthy.

The principal tone corresponds to mode (1); the tone corresponding to (2) is about a *fifth* higher; that of mode (3) is about a major seventh above the principal tone; the tone of mode (4) is a little higher again, forming an imperfect octave with the principal tone. For the corresponding modes of a uniform membrane vibrating *in vacuo*, the theoretical intervals are those represented by the ratios 1·34, 1·66, 1·83, or about a fourth, a major sixth, and an interval nearly midway between a major and a minor seventh, respectively.

In experimenting on this subject it is important to bear in mind that the system of tones is really *double*, and that its components coincide only on the supposition of perfect symmetry. In practice the requirement of symmetry is difficult to attain even approximately; and thus it is that beats are generally heard, arising from the superposition of vibrations of nearly equal frequency. For the purpose of identifying the various modes, the want of symmetry is rather advantageous than otherwise. In the case of the gravest mode, I fastened with cement a small load (a halfpenny) to a point of the membrane situated about halfway between its centre and edge. In this way the two gravest tones fell asunder to about a semitone, one of them (the graver) being excited alone by a blow anywhere along the diameter through the load, the other alone by a blow anywhere along the perpendicular diameter. With the aid of a resonator tuned to the pitch of the *subordinate* tone, the nodal diameters of the two modes (1) may be fixed with great precision by the absence of beats. With a resonator turned to a pitch midway between those of the two tones, the beats are most distinct when the blow is delivered at a point near the middle of one of the four quadrants formed by the two nodal diameters; but the position for the most distinct beats necessary varies with the pitch of the resonator, and also with the situation of the observer. It may be remarked that, provided the deviation from symmetry be moderate, the same vibrations (except as to phase) are excited, whether a blow be delivered at any point, or at the other point on the same diameter equally distant from the centre; and vibrations excited by striking one point are damped by touching the other. The other modes with nodal diameters only were identified in a similar way. The mode (4) with a nodal circle is known by the cessation of sound at a particular point when various places along a radius are tried; on either side of this point the sound revives.

The drum that I examined is of about 25 inches diameter; and

the form of the shell is nearly hemispherical. During the experiments the pitch of the principal tone was about 120 vibrations per second. The vibrations were excited by a small wooden hammer, such as is used for harmonicons, the head being covered with cotton-wool tied on with string. For the graver tones the thickness of the cotton wool may with advantage be greater than for higher tones.

I am not in a position to decide the question as to the function of the shell; but I think it at least doubtful whether it introduces any really advantageous modification into the relations of the component tones. It is possible that its advantage lies rather in obstructing the flow that would otherwise take place round the edge of the membrane. It must be remembered that the sounds due to the various parts of a vibrating membrane interfere greatly. In the case of a membrane simply stretched upon a hoop, and vibrating away from all obstacles, no sound at all would be heard at points in the prolongation of its plane. And even when there is a shell, no sound would be heard at points on the axis of symmetry, at least if the symmetrical vibrations may be left out of account.

The Æolian Harp.

So far as I am aware, it has always been assumed by writers who refer to this subject that the vibrations of the string are in the plane parallel to the direction of the wind; and, indeed, the action of the wind in maintaining the motion is usually explained as the result of friction, and as analogous to the action of a violin-bow. It is more than a year since I made some experiments with the view of testing a suspicion of the incorrectness of this view; and I then arrived at the conclusion that the vibrations are in fact executed in the plane perpendicular to the direction of the wind. I suppose for simplicity that the length of the string is perpendicular to the direction of the wind, as is usually the case in practice. Recently I have repeated these experiments in an improved form, and with confirmatory results.

The best draught is that obtained from a chimney. In my later experiments a fireplace was fitted with a structure of wood and paper, which could prevent all access of air to the chimney, except through an elongated horizontal aperture in the front (vertical) wall. The length of the aperture was 26 inches, and the width 4 inches; and along its middle a gut string was stretched over bridges. The strength of the draught could be regulated by slightly withdrawing the framework from the fireplace, so as to allow the passage of air to the chimney otherwise than through the slit.

A fine point of light was obtained from a fragment of a silvered bead attached to the string with wax, and illuminated by a suitably placed candle, and was observed in the direction of the length of the string through an extemporized telescope. In this way there could be no mistake as to the actual plane of vibration, or uncertainty as to the direction of the wind over the string. The path of the point of light was seen to be nearly rectilinear and *vertical*, showing that the vibration is *across* the wind. Sometimes the path was sensibly elliptic with the major axis vertical.

When a string is stretched across the slit at the bottom of a slightly open window, there is usually some difficulty in determining the actual direction of the wind where it plays upon the string. On a still night, and with a regular fire, the sound is sometimes steady for a long time, but it is wonderfully sensitive to the slightest changes in the draught. On one occasion it was found impossible to open a distant door so slightly as not to stop the sound, which would revive in a few seconds after the door was closed again. A piece of paper no larger than the hand thrown upon the fire (which was burning without flame) altered the draught sufficiently to stop the sound until the heated air due to its combustion had passed up the chimney. It is the irregularity, and not, as has been asserted, the insufficient intensity, of the wind which prevents the satisfactory performance of the harp in the open air.

Terling Place, Witham,
Feb. 8, 1879.

XXVIII. *On High Electrical Resistances.* *By* J. Hopkinson, *F.R.S.*, *D.Sc.**

In the Philosophical Magazine of July 1870 Mr. Phillips describes a method of readily constructing very high electrical resistances. A pencil-line is ruled on glass; the ends of the line are provided with the means of making electrical connexion; and the whole is varnished: by this means a resistance of two million ohms was obtained; and it was found to be constant under varying potential. This method of constructing resistances is alluded to in Maxwell's 'Electricity' (p. 392); but I do not know that it has received the examination it deserves, or that it has come into general use. Having need of resistances of over 100 million ohms, I have made a few on Mr. Phillips's plan, ranging from 26,000 ohms to 96,000,000 ohms (which are fairly satisfactory), and one or two

* Communicated by the Author.

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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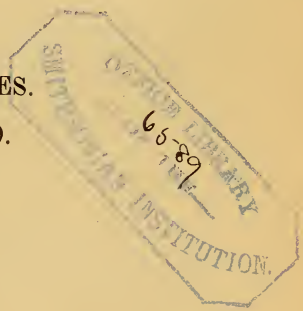

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat
Cur mare turgescat, pelago cur tantus amaror
Cur caput obscura Phœbus ferrugine condat
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.



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