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JUNE 4, 1883.

PROF. W. GRYLLS ADAMS,

IN THE CHAIR.

ON THE VELOCITY OF SOUND IN AIR.

By D. J. BLAKLEY, Esq.

THE importance of this subject, whatever the value of the work of any individual observer may be, must be manifest to all, for upon a right understanding of it depends the power of designing buildings of good acoustical proportions and wind instruments of all kinds, and the massing together to the greatest advantage (or perhaps we should say the least disadvantage) of large numbers of singers and instrumentalists. In the highest regions of physical science, also, the determination of the velocity of sound has had its place, not only for its own sake, but as a means to an end of great importance. However, it will not be necessary for our purpose to do more than touch upon this point.

For a moment let us look broadly at the question, accepting the velocity as being about 1,120 feet per second at ordinary temperature. This is a great velocity but not quite so great as that of a shot from a rifled gun. Probably many present have been at times troubled by the loss of clearness in rapid passages of music through echos in a large hall, and at such times have been tempted to wish that sound in air had no velocity at all, or, if it must have a measurable velocity, a very high one, say, like that of light. Consider what this means; with such a velocity the smallest pipe in an organ would be about equal in length to the proposed Channel tunnel, and an ordinary flute would be a construction beyond the power of human labour: in short, the possibility of the possession and use of wind instruments as a complete class depends upon the velocity of sound being what it is; if it were much greater, say, even two or three times, we should lose the bass wind, and if as much less, the treble. For instance, the bass trombone is an unwieldy enough instrument already, and would be utterly unmanageable if twice or three times its present size, and at the other end of the scale the piccolo flute and oboe could not be fingered if they

were much smaller than they now are. Again, an illustration may be drawn from the action of hydrogen on the voice. As is well known, the velocity of sound in this gas is much greater than in air, and when it is inhaled the voice not only loses power, but its quality is much changed. The loss of power is due to the extreme tenuity of the gas, but the explanation of the change of quality is probably to be found in the fact that the proportionate power of resonance of the vocal cavities to the different partials of any given compound tone would not be the same for hydrogen as for air. There is thus a relationship between the velocity of sound and our own physical faculties which it is interesting to observe.

We may now consider more closely the actual velocity, and take notice of some of the difficulties which have been met with in arriving at determinations. Newton, from a calculation based upon the weight and elastic force of air, established a theoretical velocity of 916 feet at 32° Fah., which theoretical velocity has since been corrected and is given by Regnault as 918.49 feet, or 279.95 m. at 0° cent. This calculated velocity, however, differs greatly from all observed velocities, a summary of which is here given :

TABLE I.

VELOCITY OF SOUND IN FREE AIR IN METRES PER SECOND
AT 0° CENT.

From Mr. Bosanquet's Summary in Phil. Mag. for April 1877—

				Metres.
Academie des Sciences	(1738)	...	...	332'
Bensenberg	(1811)	...	...	{ 333'7
				{ 332'3
Goldingham	(1821)	...	...	331'1
Bureau des Longitudes	(1822)	...	...	330'6
Moll and Van Beek	...	...	...	332'2
Stampfer and Myrbach...	...	...	...	332'4
Bravais and Martin	(1844)	...	...	332'4
Wertheim	...	...	...	331'6
Stone	(1871)	...	...	332'4
Le Roux (tube m. .07 diam.)	...	...	...	330'7
Regnault	...	...	...	330'7

Newton's conjecture was that sound required time only for its transmission from particle to particle of air, and that it moved instantaneously through the particles themselves. Laplace, however, gave the true explanation of the difference, which is due to the elasticity of the air as it enters into Newton's formula being disturbed by the heat and cold developed in the alternate condensation and rarefaction of the wave. The correction is $\sqrt{\frac{C_p}{C_v}} = \sqrt{1.42}$ C_p being the specific heat at constant pressure, and C_v the specific heat at constant volume. Newton's formula $V = \sqrt{\frac{p}{\rho}}$ Laplace's

formula $V = \sqrt{\frac{g(1+\frac{1}{2})}{\rho}}$ Thus by Laplace was the first great step taken towards reconciling theory with direct observation. The different values of observations taken under different temperatures need not detain us, for the question is simply that of the varying value of d in the formulæ, and it has not been a source of any difficulty to observers; the only point of special interest to musicians in connection with it is that this variation with temperature amounts to one-twentieth part in about 55° Fah., or in a wooden organ pipe, to a raising of pitch of exactly an equal tempered semitone in 60·4° Fah., say from 32° to 92·4°: in metal pipes, and brass and ebonite instruments, a little less, owing to the slight counteracting influence of the elongation of the material. For the sake of comparing observations, they are reduced to a common standard of temperature, and a correction must also be made for the presence of vapour in the air, which correction amounts to a reduction of the observed velocity of about from 2 to 5 feet in our climate.

These corrections for temperature and moisture being made, there remained at the time of Regnault's great series of experiments* considerable differences in the results arrived at by different observers, partly due, doubtless, to errors of observation and partly to an assumption of the absolute correctness of Laplace's formula, the theory in the application of which is that the excess of pressure in the wave above the barometric pressure of the air is infinitely small.

Now, experiments on the velocity of sound may be classed as open air experiments and laboratory experiments, and I venture to think that the latter offer advantages which cannot be enjoyed in open air work. The usual method in the open air has been for an observer at a distance from a gun to note the time which elapses between the flash and the hearing of the report, but even when the actual record of the time is aided by electrical or other apparatus, some difficulties and sources of error remain; the rapidity of perception in the two faculties of sight and hearing may not be the same (Sir G. Airy estimates the perception of sound to be one-fifth of a second slower than that of light), and the accurate registration of temperature and moisture is difficult, especially when the sound-wave passes at various heights above the earth's surface, as is the case when the experiment is carried out on two hills separated by a valley.

Midway in character between open air and laboratory experiments stand those of Regnault, carried out in gas and water mains, one of his reasons for choosing this method being the facility afforded by these tubes for the accurate observation of temperature and moisture. Passing by his work for the moment, we may note the laboratory method employed by

* *Memoires de l'Academie des Sciences*, Tome 37.

Kundt, which is pretty well known, as it is well suited to the lecture-room and gives beautiful results. It consists in its best form in the use of two glass tubes connected by a smaller tube or rod of glass, wood, or metal; this connecting rod being clamped in certain positions to established modes, and its free vibrating ends being fitted with pistons working in the large tubes. The waves are excited by friction in the vibrating rod and transmitted therefrom to the air or other gas in the tubes, and the successive half-wave lengths are registered by the positions assumed by lycopodium dust during the vibration. By using tubes of different diameters he obtained the results shown in table 2; but appears to have experienced some difficulty in the determination of pitch, owing to the evanescent character of the sound. The intensity also was not registered. The method is beautifully adapted for comparative rather than for absolute results.

TABLE II.

VELOCITY OF SOUND IN TUBES IN METRES PER SECOND AT
0° CENT.

KUNDT.		
Diameter of Tube. M.M.	Velocity. M.	Difference. M.
55	332.80	...
26	332.73	
13	329.47	
6.5	323.00	
3.5	305.42	

TABLE III.

VELOCITY OF SOUND IN TUBES IN METRES PER SECOND AT
0° CENT.

REGNAULT.		
Diameter of Tube. M.M.	Velocity. (Mean.) M.	Velocity. (Limiting.) M.
1,100	330.5	330.3
300	330.3	329.25
108	327.52	324.25
Free air.	330.71	330.60

We may now turn to Regnault's experiments, carried out in a most thorough manner, and the complete record of which is a mine of valuable information.

The one point which appears to me to be somewhat open to question (see table 3) is whether the rate of diminution of velocity is so great as his work appears to prove; for if this rate of diminution is extended until we reach tubes of the size used in musical instruments, we should have a velocity much less than experiments show to exist in such tubes (*Compare with Kundt*). Possibly the want of perfect smoothness in Regnault's tubes may account for the difference, but I feel that the question is still an open one.*

It has long appeared to me that useful results might be obtained by making use of tubes giving musical notes, as the pitch of a steadily sounding note can be readily determined within a remarkable degree of accuracy, and there should be no difficulty in determining the temperature within a half degree Fah., which is equivalent to six inches in the velocity. (Experiment—Resonance of closed tube to fork of 512 vibrations, the length of the tube being modified during the experiment to show both the maximum and constrained or imperfect resonance.) Modifying this experiment by sounding the tube with an organ-pipe mouth, the disturbing influence of the contraction of area at the lip comes into play in addition to the correction for the open end, so that although the value of the latter is pretty accurately known no measurement of velocity based merely upon the length of a pipe is at all reliable. By adding, however, a half-wave or wave length to a stopped pipe, maintaining the original pitch, and measuring this added length, I hoped to be able to get reliable results. The observations though agreeing very well indeed, so long as the pressure remained constant, did not agree in their different series when taken under very slight differences of pressure, much less than would cause sensible variation through change of intensity, according to Regnault's determinations.

To properly understand this point, it will be necessary to examine some of the results of imperfect or constrained resonance. If the proper tones of a closed organ-pipe agreed exactly with the odd numbers of the harmonic series, or, in other words, if we were sure that the pitch of the speaking pipe was due to an unconstrained resonance of its prime tone, the method would, without modification, be reliable, but difficulty is introduced through the pitch not being of this

* One great object in Regnault's observations was the determination of the value $\frac{C_p}{C_v}$. His result is 1.3945. Laplace's value being 1.42.

character. In the figure, let A B represent a closed pipe,

nominally of quarter-wave length, and B C an added half-wave length; let the pitch of the prime tone of A B = 100 vibrations. If there were no contraction of area at the speaking end of the pipe, the next proper tone would be of 300 vibrations, but through the modification of area at the speaking end the harmonic series is somewhat disturbed. Assume this disturbance to be such that the twelfth has a pitch of 309 vibrations instead of 300, and that the prime and the twelfth are of equal force; the resulting pitch, or the pitch of the pipe as actually spoken, will lie between 100 vibrations and 103 vibrations—i.e., it will equal 101.5. A half-wave length, B C, is now added and tuned to correspond with the pitch of A B as heard (101.5); but from the foregoing considerations it will be seen that the true pitch of the prime of B C must be 100, and that the registered pitch of 101.5 is due to a constrained resonance. Thus the length measured, while nominally proper to a half-wave of 101.5 vibrations is strictly the length of a half-wave of 100 vibrations. The instance is exaggerated but will serve to illustrate the difficulty. To eliminate this source of error, one of two things must be attained; we must deal either with pure tones or be careful to have resonating pipes of such form as to have their various proper tones strictly in accord with the harmonic series, and not merely approximately as is the case with organ pipes.

The tubes with which I experimented were of four diameters—.45, .75, 1.25, and 2.08 inches respectively. After detecting the possible source of error which has just been described, and trying a few modifications of the ordinary organ pipe, the speaking end or first quarter-wave length of the three smaller tubes was modified in form from cylindrical to pear-shaped, approximating to the shape of Helmholtz's resonators, and by this means a pure tone was obtained. The blast was obtained from a fan, the wind from which passed through a regulating bellows with automatic valve action, and it was found that great care was necessary on this point. The pressure in the bellows was 1.55 inches and in the speaking mouth in every case very small, as will be seen in the table of results :—

TABLE IV.

VELOCITIES AS OBSERVED AT TEMPERATURES FROM 65° TO 72°
FAH. REDUCED TO 60° FAH. IN INCHES PER SECOND.

		DIAMETER OF TUBE IN INCHES.			
		2.08	1.25	.75	.45
		PRESSURE IN INCHES OF WATER.			
		.45	.05	.07	.05
1	13408	6	13356	12	13265
2	13405	7	13361	13	13249
3	13405	8	13357	14	13265
4	13407	9	13347	15	13260
5	13413	10	13357	16	13295
		11	13349	17	13282
				18	13229
				19	13233
				20	13202
				21	13225
				22	13232
				23	13218
				24	13248
Mean in inches—		13408	13354	13269	13227
Mean in feet—		1117.3	1112.8	1105.7	1102.2
Correction for Moisture—		3.1	3.5	2.9	2.9
Dry air at 60° Fah. in feet—		1114.2	1109.3	1102.8	1099.3

SUMMARY.

DIAM.		VELOCITY IN DRY AIR (32° Fah., or 0° Cent.)	
ins.	m. m.	in metres	diff.
2.08	54.1	330.37	
			1.50
1.25	32.5	328.87	
			1.92
.75	19.5	326.95	
			1.03
.45	11.7	325.92	

The temperature was observed by means of a thermometer entering the tube, so that the actual temperature during vibration might be recorded. The wet bulb temperature and barometric pressure were also taken. The pitch generally used ranged about 260v, a carefully tested Koenig's fork of 256v being the standard, and the tubes were set to give a beating rate of from 3 to 5 per second. The lengths were read in inches by means of an ordinary drawing scale divided to hundredths of an inch, the added half-wave only being used for measurements, but by using a screw micrometer greater accuracy of measurement could easily be obtained.

In the large tube the disturbing influence of the partial tones may have had an effect in raising the rate, and the pressure at which it was sounded being greater than that used with the other tubes may have been sufficient to cause a sensible increase. I have not yet had an opportunity of experimenting with this tube under exactly the same conditions as the others. The object, incompletely carried out thus far, was, by carrying out a series of observations with

tubes of different diameters, to deduce a law or scale of differences from which might be estimated the velocity in free air of a sound not of sufficient intensity for the velocity to be influenced thereby. At the same time, the method might be applied under different intensities if we had a very delicate pressure gauge.

The theoretical and experimental conclusions we have been considering are applicable, strictly speaking, to plane waves, or waves in which all the points having a similar position in the wave lie in a plane at a given moment of time, but in practice the air waves which affect us as sound are usually not in this condition but emanate from an origin which more or less nearly approaches a point, this point being the centre of a system of spherical waves.

We may refer to the vibration in conical tubes as bearing upon this point. A complete cone, speaking its lowest note, with its apex or closed end for the position of the node, is of twice the length of a closed cylindrical tube of the same pitch, and has the same succession of harmonics as the open cylindrical tube (see Proceedings of the Musical Association for February 4, 1878). This property is independent of the proportion of base to height in the cone, and if we assume the base of the cone to be a portion of a spherical surface described from the apex, we may continually increase the conical angle until the cone becomes a sphere. Now, if we consider the effect of this principle and apply it to divergent waves, it will be found that such waves cannot have exactly the same velocity for all pitches, but that the lower the pitch the greater will be the velocity, owing to the difference of velocity between the first and succeeding quarter-wave lengths. To take a numerical example, we may choose two wave-lengths of 32 feet and 8 feet respectively:—

QUARTER-WAVE LENGTHS.						Excess in
		Cylinder.			Cone.	Cone.
32 ft. wave	...	8 ft.	...	16 ft.	...	8 ft.
8 ft. wave	...	2 ft.	...	4 ft.	...	2 ft.
Difference						6 ft.

Or, if we assume a velocity in free air (away from origin of vibrations) of 1,120 feet per second, we shall have:—

Velocity of sound-wave of any pitch, not including first quarter-wave	...	...	...	...	1,120 ft.
Velocity of 8 ft. wave, or wave of 140 vibrations measured from apex of cone, $1,120 + 2 \dots$	...	...	...	...	1,122 ft.
Velocity of 32 ft. wave, or wave of 35 vibrations measured from apex of cone, $1,120 + 8 \dots$	...	...	...	...	1,128 ft.

We thus find that waves near their origin are not of normal length.

In gun-fire experiments the pitch of the explosion is not known, and there may therefore be a variation in the recorded velocities which is due simply to an effect of conical or spherical divergence. This somewhat difficult point, however, I bring forward but tentatively, and especially for discussion, from which I hope to derive information.

DISCUSSION.

The CHAIRMAN.—Ladies and Gentlemen, we have had a very interesting communication from Mr. Blaikley, and there are several points in it which are especially interesting to me, and I am sure there are other points which, from a practical point of view, require elucidation and will be of interest, and I hope will receive a very good discussion. I should like to draw attention to one point which I consider very interesting on the scientific side, and that is as to the difference between the results obtained by Newton and those obtained by Laplace. In every sound-wave where the air is most condensed the pressure is greatest, and there is a rise in temperature which was not allowed for by Newton in his calculation, and in every part of a wave where the air is most rarefied there, in consequence of the rarefaction, is a cooling, so that the temperature is below the common temperature of the surrounding air, and from both those causes the velocity is increased, not, as might be thought, increased in the one case and diminished in the other, and that, therefore, the average would remain very much the same, but because in the one case where the pressure is increased the temperature is also raised, and in the other the air is rarer and therefore cooler. Those two actions taking place at the same time, both have the effect of increasing the velocity of sound. So the correction of Laplace, we may say, arises from both of those causes combined, and the difference between his result and Newton's is completely explained by that change of temperature. There are some other very interesting points; for instance, I may allude to the method employed by Kundt to determine the velocity of sound, or rather to compare the velocity of sound in different materials. It is an exceedingly pretty experiment which any one can try. You have only to take a glass tube of, say, one-inch bore and to take a glass rod, put it into a cork, and place this glass rod inside the tube with a portion of it projecting at the end, and then introduce lycopodium dust on the inside of this glass tube, scattered along the length of the tube. It is then only necessary to cause the glass rod to give out longitudinal vibrations, and the air on the inside of the tube will take up those same vibrations, and

at the lengths of the half-waves will be the nodal points, a great number of them throughout the length of the tube, and if the tube is of considerable length, and these are carefully measured, we may get the comparison between the velocity of sound in the glass rod and in air. The method is an exceedingly pretty one. There are some other practical points here which I hope will be taken up. You heard just now the purity of sound which came from this tube. I think that is a very important point, putting in the resonance chamber, in fact to absorb, I may say, the harmonics so as to get rid of everything except the fundamental tone. The purity of tone I think was very apparent just now in that instrument, though unfortunately the air was rather full of sound from a variety of causes. I am sure I may express your thanks to Mr. Blaikley for bringing this interesting subject before us this afternoon.

The vote of thanks having been carried,

Dr. BRIDGE.—It would be quite impossible for one like myself to enter into such a discussion of these matters as our Chairman has led us into. I have been very much interested in the subject of the Paper, and I am sure all practical musicians feel now-a-days that it is necessary we should enlarge our minds in this direction. I do not know that I have any particular questions to ask with regard to any matters which have been brought forward. Mr. Blaikley has not enlarged very much on the application of this matter to organ pipes, but that has been discussed so often that I do not know that there is much more to be said about it. It is a very great point to arrive at anything like a determination of the velocity of sound, as we often have in practical music to consider it. With regard to the arrangement of numbers of voices, for instance, I remember quite lately I was very much exercised in my mind as to the result of an experiment I intended to try when we were doing Gounod's "Redemption" at the Abbey. Some of you may be aware that I at that time placed a choir of instruments—harps, trumpets, and so on—up at a very great altitude in the triforium of the Abbey to render the great "Redemption" chorus. Many of my friends—amateurs and clergymen—not of necessity great musicians, dissuaded me before-hand from attempting it. I was anxious to hear whether this chorus of instruments would be good, and whether above all things the tone would come down in strict time with the large body of voices I had down below. I was somewhat anxious about it, and I took the boys in quietly one evening and tried it. The result was, I venture to think, very satisfactory; I do not know that I had any particular data to go upon, but, at any rate, it was a point I was very anxious to clear up by actual experiment.

Mr. J. HIGGS.—I should like to know to what extent the purity of the tone we have heard is possibly due to the exceptionally light wind. It has scarcely any practical comparison with any wind used in organ work.

Dr. BRIDGE.—What is the weight of that wind?

Mr. BLAICKLEY.—It is from about one-tenth to one-twentieth of an inch.

Mr. J. HIGGS.—Would not the light weight of the wind contribute to the purity of the tone?

Mr. BLAICKLEY.—It would no doubt. The tone would not be so pure with more strength of wind, but I do not think you would have the harmonics in any strength even then.

[The experiment was repeated.]

Mr. J. HIGGS.—My question was the degree to which the light pressure or the altered form affected the purity of the tone.

Mr. BLAICKLEY.—All my experiments were with the same light pressure, or very nearly as light, but yet I did not get so good a result with ordinary pipes as with the form of pipe now used.

Mr. J. HIGGS.—It might be an interesting point if we could know how far ancient organ-builders in the curious mouths they sometimes put in pipes in the fronts of organs were feeling after the same result.

The CHAIRMAN.—In the case of the conical pipes, I think the length of a conical pipe and the length of a cylinder would be nearly the same to give the same note. They would resound in the same note, would they not?

Mr. BLAICKLEY.—Yes, if open pipes. I have to thank you for the kind way in which you have received the Paper. The experiments are rather rough, and not so complete as I could have wished, but the subject has been brought forward rather as a stop-gap as I found myself unable to read the Paper originally promised.

Dr. BRIDGE then proposed a vote of thanks to the Chairman, which was seconded by Mr. Southgate and carried unanimously.