New Experiments in Sound.

-

Wm. W. Jacques.

1876

New Experiments in Sound.

A Thesis; presented by
William W. Jacques.

Class of '76.

-C-o-n-t-a-i-n-i-n-g:-

I. A proof of the Law of Inverse Squares.

II. Diffraction.

III. Velocity:-

a: Effect of [illegible]tensity upon Velocity.
b. Eff[illegible] of Pitch upon Velocity.
c. [illegible] method of [illegible] the [illegible]
fects of Barometric Pressure and
[illegible] on the State.

It is the purpose of the following pages to give an account of some new experiments in Sound.

The experiments are in three series. The first series was made for the purpose of testing the law of inverse squares; the second for the purpose of proving that the phenomena of diffraction may be produced by sound waves, and of thereby showing that the principles of Fresnel and Huyghens relating to elastic media are applicable to our atmosphere; the third series had for its object the determination of the effects of intensity, pitch, barometric pressure and hygrometric state upon velocity.

Parts 1 & 2 of this Thesis have been missing since Phila. Exposition of 1876.

CKC.

III.— Velocity of Sound.

The determination of the velocity of Sound is a problem which has generally been considered as solved. The best experimental determinations of this quantity have harmonized so completely with the theoretical results obtained by applying Laplace's correction to the formula deduced by Newton as to seem to justify us in accepting this result as final.

Nevertheless many eminent mathematicians an physicists have, by both theoretical and experimental processes, attempted to show that the problem is yet to be unfolded.

One eminent English mathematician* shows that Laplace's correction is not only uncalled for, but absolutely invalid, and deduces the surprising result that Sound waves may have any velocity whatever.

He also brings forward seemingly well founded observations to confirm his theoretical results.

Another† shows that there are two distinct velocities corresponding to the condensed and the rarified waves, and tells us that the only reason

* Earnshaw – Phil. Mag. June, July & Sept. 1860.

† R. Moon Phil. Mag.

we do not distinguish them both is that the ear is so constructed as to suppress the wave of condensation.

Other physicists have raised, on physical grounds, the questions of whether the velocity of sound is not affected by barometric pressure,[1] hygrometric state of the atmosphere,[2] variation of g with latitude,[3] the original direction impressed on the sound,[4] the nature of the surface over which the sound is conveyed,[5] and, finally, by intensity[6] and pitch.[7]

Its dependence on temperature and the wind is undisputed.

It is neither my purpose nor my place to criticise the processes of such eminent mathematicians as those above referred to, I can only place their array of arbitrary functions and assumptions and approximative steps by the side of the logical and straight-forward steps of Newton's and Laplace's deduction.

It is the purpose of these pages, however, to discuss some of the questions which have been raised on physical grounds, particularly those points on which the Author's experiments have had a direct bearing.

1. Winter - (Phil. Mag. 1814). Tracey - (Phil. Mag. 1824). Galbraith - (Phil. Mag. 1826)
2. Galbraith - (Phil. Mag. 1826.).
3. Winter - (Phil. Mag. 1814). Galbraith - (Phil. Mag. 1826.)
4.
5.
6.
7.

The very unharmonious results arrived at by the different investigators who have taken into account these various disturbing causes in deducing an expression for the velocity of sound makes it desireable that experimental determinations should be made of the magnitude and importance of the supposed disturbing causes. The Author is at present engaged in the experimental discussion of this problem, and presents, in the following pages, a description of the methods used, together with such results as have already been obtained.

There are four courses of Experiments. The first has for its object the determination of the effect of intensity; the second, the effect of pitch; the third, the effect of barometric pressure and the fourth, the effect of hygrometric state and the presence of other gases and vapours in the atmosphere than oxygen and nitrogen.

The remaining questions of the effect of the variation of g with latitude, the original direction impressed on the sound, etc. will probably, after being viewed in the light in which the completion of the above proposed experiments together with others already performed, hardly call for a seperate experimental treatment.

A.— Effect of Intensity on Velocity.

First Series:— The method used for this determination was the automatic measurement of the velocity at different points near to the mouth of a canon by allowing the sound wave to impinge upon a series of membranes placed at different distances from its origin.

The passage of the sound wave by each membrane was recorded on a chronograph by means of suitable electrical connections.

Let us examine, in detail, the apparatus used, and then see some of the results of the experiments.

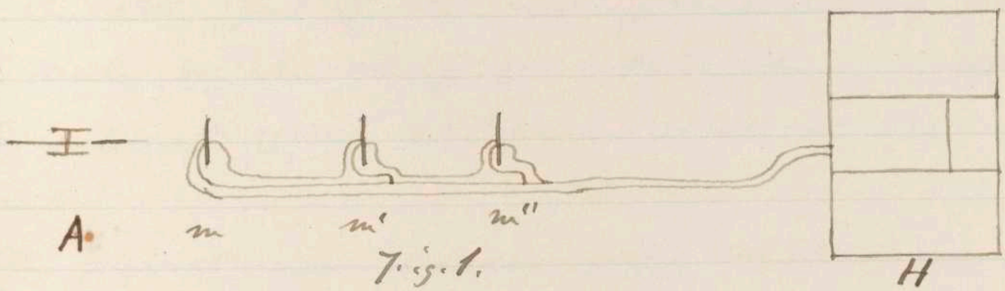


Fig. 1.

At A. (Fig. 1) was the gun, at m, m', m'' the membranes, and at H the house in which was a battery room B and an instrument room in which were mounted on stone piers a Schultz and a le Boulengé chronograph. The meteorological instruments too were placed in this room.

As a source of sound a six pounder field piece was used, fired generally with a charge of 1¼ lbs. of powder.

The membranes were constructed as seen in Fig. 2.

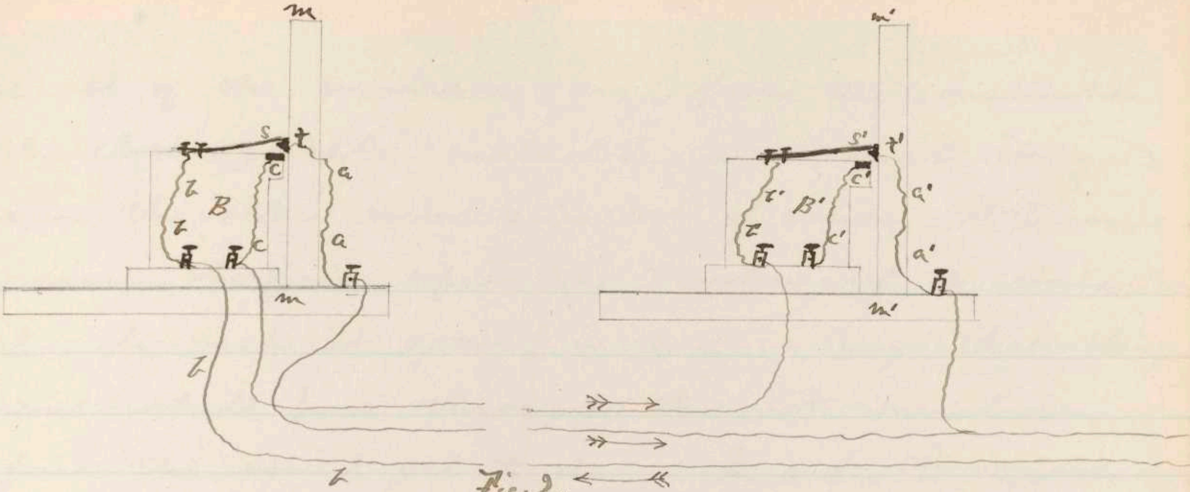


Fig. 2.

m m is a membrane of thin rubber stretched over a hoop 9.5 in. in diameter; t is a small shelf of polished brass made according to an exact pattern, so that its dimensions shall be exactly the same in all of the membranes; s is a steel spring fixed at the end resting on the block B and connecting with the wire b b, the other end of the spring is carefully polished and rests on the shelf t. This spring is as nearly as possible of equal stiffness in all of the membranes.

The current now passes from the chronograph through b b b b, s, t, a a, back to the chronograph.

A slight movement of m m however suffices to withdraw t and allow s to fall, and so to interrupt the current. This circuit, which we have broken, forms part of the primary coil of an induction coil, and its interruption causes a spark between the terminals of the secondary coil, which is registered on the chronograph in a manner to be described below.

The breaking of the circuit at the first membrane, since all of the membranes are, from the construction of the chronograph, included in a single circuit, throws the other membranes out of action. It becomes necessary therefore, after the breaking of the contact at t, to make it again in such a way that the current shall pass through the next membrane before the sound gets to it. In order to accomplish this the spring s, as it falls, strikes upon a plate of brass at c and so allows the current, which formerly traversed the path zzzz, s, t, aa, to pass through zzzz, s, cc, along this wire to the next membrane, and through z'z', s', t', a'a', back to the chronograph. The contact at t' is, in its turn, broken and the current shifted to the next membrane.

It will be readily seen that the vital points in this apparatus are that the time of action of all the membranes should be the same, and that the current should be shifted from each membrane to its successor in less time than it takes sound to travel over that distance.

In order to accomplish the first, all of the conditions of the membranes, such as stiffness of springs, dimensions of catch and tension of membranes were made as similar as possible. By making the springs of proper stiffness the second difficulty was overcome. The membranes were set on stools raised some three feet above the ground.

The chronograph used was a "Schultz" (of which a full description may be found in Benét's "Electro-Ballistic Machines and Schultz Chronoscope.") which consists essentially of a polished silver cylinder to which a combined rotatory and horizontal motion may be given by means of swift moving clockwork.

The cylinder is covered with lampblack and upon this a tuning fork is allowed to draw its curve as the cylinder rotates. By the side of the fork a fine wire is led through a glass tube to near the cylinder. This wire is one terminal and the cylinder the other of the secondary coil of the inductorium of which the circuit through the membranes is the primary coil.

As the sound wave passes each of the membranes, the breaking of the connection causes a spark between the wire and the cylinder, and leaves a small dot on the lamp black.

Between the dots so formed are other smaller ones due to the making of the circuit.

A micrometer is attached to the instrument for the exact measuring of the intervals between the dots, and these intervals are translated into times by comparing them with the sinuosities of the fork. The tuning fork makes about 250 vibs. in a second and, as each of these recorded vibrations may easily be divided into 100 parts, it becomes perfectly possible to measure time with accuracy down to 0.0004 of a second.

Besides the above apparatus, thermometers, a barometer and a hygrometer were used in the experiments. As the experiments could only be made when the air was perfectly calm, the velocity of the wind of course was not measured.

Let us now see some of the results of the experiments. On the first day, which was, as a whole, very calm, although broken by occasional light breezes, shots were fired at intervals during the whole day; times being chosen when the atmosphere was as nearly as possible at rest. The distances being so short, never more than 30 feet, it was quite possible to find sufficiently long times of almost perfect quiet. When such a time came, the man in charge asked of those at the membranes if all was ready; upon receiving their answers, he immediately signalled to another signal man stationed just outside the chronograph house, which was distant about 300 yards, who gave the command to start the chronograph and, upon seeing that it was started, instantly signalled back to the man in charge, who gave the command, "fire!" The time occupied in this signalling was occupied probably not more than one and certainly not more than two seconds.

The first day's firing gave us no guartitation results; the only law that could be deduced from them being that, the farther the sound travelled the faster it went, which however, we shall see there was a reason for.

The shots were not wasted for they showed us that the membranes would work, though not entirely to our satisfaction, and, moreover, they showed us in what directions to look for errors. The results were so much at variance with what we had been expecting, and the record from the breaking of the circuit was so intermingled with the spots caused by secondary currents due to our having used too strong batteries, that it was not considered worth while to record them, and they were accordingly erased when the cylinder was cleaned.

The second day's firing gave us the same result with regard to the velocity at different distances from the mouth of the piece - i.e. - the velocity increased with the distance. These results, like those of the other day, were not recorded, but an approximate calculation was made from one of the observations, which gave what seemed an absurd value, but which, after having applied to it the corrections determined by later experiments, becomes more reasonable and might; had it been accurately recorded, have been of considerable value.

The principle result of the second day's firing was the exact adjustment of the batteries so as to give a clear record and get one free from spots due to secondary currents. It was also found an advantage for the man in charge to give the signal to the man at the chronograph directly who, upon starting the machine, gave the command "fire."

In all of these experiments readings were taken of the meteorological instruments immediately after the report of the gun.

On the third day everything seemed favourable to the success of the experiments. It was quite calm, the membranes seemed to be in good condition and the record was clear and free from secondary spots.

Five shots were fired and three velocities, two of which were from the second and the other from the third shot, were measured; but a shower coming up, the rain affected the insulations of the wires so that no record was obtained from the last two shots, and so the experiments were discontinued. The removal of the Schultz chronograph to Sandy Hook prevented any continuation of these experiments till the apparatus should be modified so as to adapt it to another form of chronograph.

The record which was obtained showed, as before that the velocity increased with the distance;

it was carefully copied however, and it is upon these three velocities that the result of the first series of experiments directly depends.

A discussion of them shows that they are of considerable value and, if they be confirmed by experiments of the second series, they will prove of value in the settlement, not only of one question, but of two - the questions of the effect of intensity and of the original direction impressed upon the sound.

The measurements made were between a membrane A, 10 ft. from the mouth of the gun, and one B, 40 ft. from the same point; and a simultaneous measurement between B, placed as above, and C, 70 feet from gun.

Also between A and C directly.

As calculated directly from the record on the chronograph, these velocities were

$$V_{A\ to\ B} = 1034.7 \text{ ft}$$
$$V_{B\ to\ C} = 1122.9 \text{ "}$$
$$V_{A\ to\ C} = 1089.2 \text{ "}$$

Upon reducing for temperature the second gave a value somewhat above and the first and third considerably below the received value of the velocity of sound.

It will be remembered that the first membrane, in the two cases where there is so considerable a discrepancy was 10 ft behind the gun. Now, if we suppose that the air, rushing out from the gun, retards the sound so that, in the immediate vicinity of the gun, it is not propagated as fast as it otherwise would be, we have not only a qualitative but a quantitative explanation of the discrepancies for, if we assign a numerical value to this retardation, it will be found to be nearly the same for both.

This too would explain the fact so constantly observed, that the velocity increased with the distance.

Correcting the two velocities by this quantity and reducing for temperature, we have for the results of our experiments

$$V_{A \text{ to } B} = 1092.9.$$
$$V_{A \text{ to } C} = 1103.2$$
$$V_{B \text{ to } C} = 1108.8$$
$$\text{mean} = 1101.9$$

According as we take the previous determination of velocity as 1090 or 1093 ft: our measurement exceeds them by 11.9 or 8.9 ft.

This increase of velocity over that of previous experimenters is what we should expect if we admit that the velocity varies with the intensity.

This difference however is comparatively small and, when spread over a distance of 5 to 20 miles becomes too small for consideration; and we are therefore warranted in accepting the results of measurements made over many miles as a basis on which we may depend in such further work as involves the consideration of the velocity of sound.

If we admit that the retardation, for which we have applied a correction, is due to the direction impressed upon the sound, we have arrived at some idea of the magnitude of the error due to the original direction impressed upon the sound.

Before the above results can be admitted to rank equally with those of former experimenters they must of course be confirmed by a considerable number of observations. A modification of the apparatus so as to adapt it to the le Boulengé chronograph will, it is hoped, enable this to be done.

The results will form Series Second.

Note. The above article would be incomplete without reference to the experiments of Regnault on the decrease in velocity of sound in pipes. Regnault finds that by causing a sound to travel back and forth by successive echoes, through a given pipe, there is a slight decrease in velocity each time it traverses the path, which he attributes partially to a decrease in intensity. He finds however that the smaller the pipe, the greater the retardation and thus seems to be

7. Effect of Pitch on Velocity.

The question of the effect of pitch upon velocity has been discussed by Biot, who caused several airs to be played on a flute at the end of a pipe 3120 ft. long and found that no derangement in the intervals of the notes was noticeable at the other end.

Similar experiments have been made in the open air and it is a well known and often recorded fact that a piece of music played at a considerable distance from an observer will come to him with no appreciable change in its intervals.

It has been generally admitted that velocity is entirely independent of pitch.

It is evident however that the only legitimate conclusions to be drawn from the various experiments and observations that have been recorded is that the variations of pitch in these particular cases was not noticible. Any wider conclusion may be objected to on the grounds that the variations in pitch there contrasted were necessarily small, and it is a well known fact that Nature has so arrayed the variations of her laws as, in general, to make them simplest in the regions of which the senses take ready cognizance. It is often only as we search the border land of our sensuous experience that we begin to find variations and exceptions to her laws.

The following method of investigating the subject has little in it that is new; it proposes merely to test the question of the effect of pitch by using sounds near the extremes of audibility. Its advantage over Bioli's method is that it furnishes us with a far wider range of pitch than the sounds which that experimenter used could have done.

The experiments were made in one of the sections of the new Sudbury River conduit. This is a brick tunnel having a height of about eight feet and a breadth of nine feet. It is, at present, about two thousand feet long. The apparatus consisted of a number of steel bars 2 cm in diameter and varying in length from 6 - 10 cm. When such a bar is suspended by a silk cord and struck with a small hammer, two sounds are heard, the thud of the hammer, a sound of extremely low pitch and the ring of the bar, which is of a very high pitch. These two sounds of course are produced simultaneously.

If there is any difference in the velocity of propagation of them, an observer at a considerable distance, ought to notice an interval between the arrival of the two sounds, or, at least, to notice a difference in the character of the combined sound caused by such an interval though it be extremely small.

At one end of the conduit-pipe an assistant was stationed who struck such a bar at intervals of a few seconds, while the observer walked into the pipe a considerable distance. The first bar used was an Ut_9 giving 16384 double vibrations per second. The note of the stroke of the hammer was probably between Ut_1 & Ut_2.

At a distance of about 900 feet the ring became inaudible, but, up to this point, not the slightest interval between the two notes was noticeable.

A Sol_8 bar, giving 12288 double vibrations was then used, and the distance extended to 825 feet, at which point both the notes became inaudible.

At a distance of 800 feet they were distinguishable however, but no interval between them could be made out.

C.— A method of studying the effects of Barometric Pressure and Hygrometric State on Velocity.

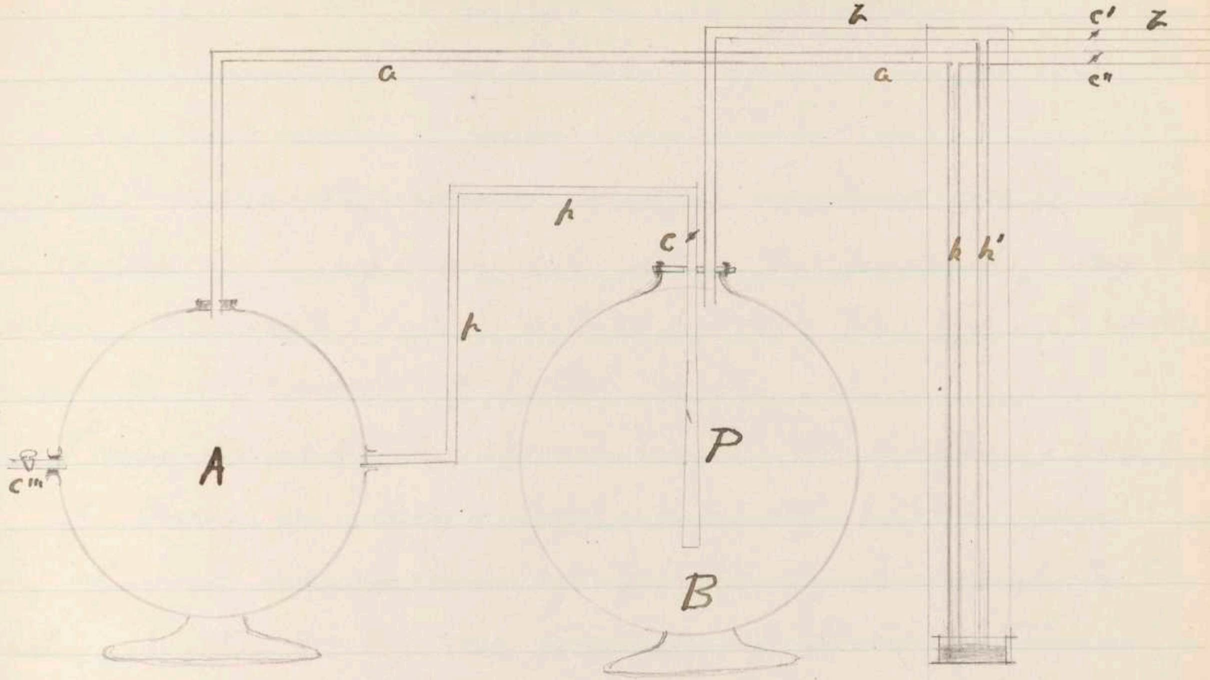
The questions of the effects of barometric pressure and hygrometric state of the atmosphere on the velocity of sound have, so far as the Author is aware, never been put to experimental test.

Physicists have raised the questions and mathematicians have calculated the results, but the discrepancies of such results show that the questions admits only of an experimental solution.

The apparatus and method to be used in the investigation of the effects of these two causes is essentially the same for both. No experiments have as yet been made as the apparatus is not yet completed.

The velocity is measured by the indirect method of determining the pitch of an organ pipe, blown in and with air under varying conditions of pressure and humidity.

Fig. 1. shows the arrangement of the apparatus. A and B are two large glass globes which connect with Bunsen pumps by means of the tubes aaa and bbb; h and h' are barometers connected respectively with A and B so as to



measure the pressures in the globes. In the globe B is placed a stopped lead organ pipe and this is connected by means of the tube pp with the globe A; cocks are placed at c, c', c'' and c'''.

Inside and outside of the globes are thermometers and, for the experiments on the effect of change of humidity, the globes also contain an apparatus for measuring the relative humidity.

For the first series of experiments a series of drying tubes is connected with c''', and all of the air used in the experiments is admitted through them.

Outside of the globes is a pipe precisely similar to P and connected with a gas holder as before described. If now the air be exhausted from A and B, that in A being left at a pressure slightly above that in B, and then, if the cocks c be opened, we may, by properly regulating the cocks c, c', c'' and c''', blow the pipe P with air at a pressure indicated by h.

Sounding, at the same time, the outside pipe P', if there is any difference of pitch resulting from variation of pressure, it ought to be made evident by the beating of P with P'.

Knowing the pressure h, the temperatures inside and outside the globe and the number of beats we have data for calculating the effect of change in barometric pressure.

If we admit at c''' successive portions of air containing more and more moisture, we may, in a similar manner, study the effect of change in hygrometric state.