

would remain there and form a sheet if the surface were not too much agitated, except for a current or movement in the body of water sufficient to maintain it in a constant state of intermixture. Even when flowing in a regular channel there is a continued interchange of position of the different parts of a stream, the retardation of the bed caused variations in the velocity which produce whirls and eddies and a general instability in the movement of the water in different parts of the section. The result being that the water at the bottom soon finds its way to the surface, and the reverse. I found by experiments on straight canals in earth and masonry that coloured water discharged at the bottom reached the surface at distances varying from ten to thirty times the depth.¹

In natural watercourses, in which the beds are always more or less irregular, the disturbance would be much greater. The result is that the water at the surface of a running stream does not remain there, and when it leaves the surface it carries with it the needles of ice, the specific gravity of which differs but little from that of the water, which combined with their small size, allows them to be carried by the currents of water in any direction. The converse effect takes place in muddy streams. The mud is apparently held in suspension, but is only prevented from subsiding by the constant intermixture of the different parts of the stream; when the current ceases the mud sinks to the bottom; the earthy particles composing it, being heavier than water, would sink in still water in times inversely proportional to their size and specific gravity. This, I think, is a satisfactory explanation of the manner in which the ice formed at the surface finds its way to the bottom; its adherence to the bottom, I think, is explained by the phenomenon of *regelation* first observed by Faraday; he found that when the wetted surfaces of two pieces of ice were pressed together they froze together, and that this took place under water even when above the freezing point. Prof. James D. Forbes found that the same thing occurred by mere contact without pressure, and that ice would become attached to other substances in a similar manner. *Regelation* was observed by these philosophers in carefully arranged experiments with prepared surfaces fitting together accurately and kept in contact sufficiently long to allow the freezing together to take place. In nature these favourable conditions would seldom occur in the masses of ice commonly observed; but we must admit, on the evidence of the recorded experiments, that under particular circumstances pieces of ice will freeze together or adhere to other substances in situations where there can be no abstraction of heat.

When a piece of ice of considerable size comes in contact under water with ice or other substance it would usually touch in an area very small in proportion to its mass, and other forces acting upon it and tending to move it would usually exceed the freezing force, and *regelation* would not take place. In the minute needles formed at the surface of the water the tendency to adhere would be much the same as in larger masses touching at points only, while the external forces acting upon them would be extremely small in proportion, and *regelation* would often occur, and of the immense number of the needles of ice formed at the surface enough would adhere to produce the effect which we observe and call anchor-ice. The adherence of the ice to the bed of the stream or other objects is always down stream from the place where they are formed; in large streams it is frequently many miles below; a large part of them do not become fixed, but as they come in contact with each other, *regelate* and form spongy masses, often of considerable size, which drift along with the current and are often troublesome impediments to the use of water-power.

Water-powers supplied directly from ponds or rivers or canals frozen over for a long distance immediately above the places from which the water is drawn, are not usually troubled with anchor-ice, which, as I have stated, requires open water up stream for its formation.

UPON A MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES²

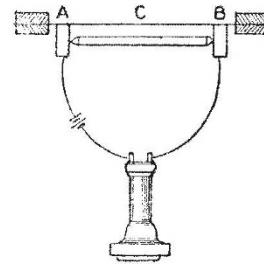
IN August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight,

¹ Paper cix. in the *Transactions* of the Society, 1878. Vol. vii., pages 109, 168.

² A paper read before the Philosophical Society of Washington, D.C., June 11, 1881, by Prof. Alex. Graham Bell.

and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm (Amer. Assoc. for Advancement of Science, August 27, 1880). Shortly afterwards Lord Rayleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating (*NATURE*, vol. xxiii. p. 274). This explanation has recently been called in question by Mr. Preece (Royal Society, March 10, 1881), who has expressed the opinion that although

Fig 1.

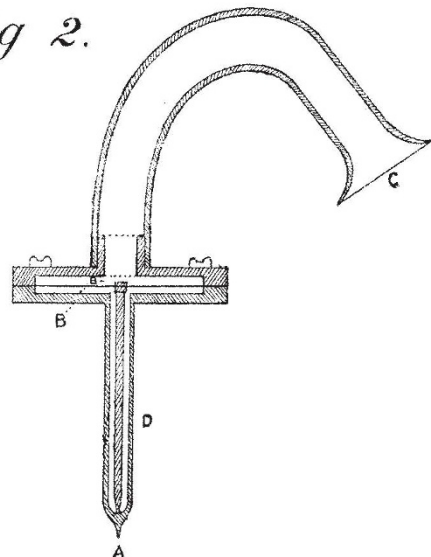


A B, Carbon supports; c, Diaphragm.

vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him the aerial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm, every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Rayleigh on account of the failure of experiments undertaken to test the theory.

He was thus forced—by the supposed insufficiency of the explanation—to seek in some other direction the cause of the

Fig 2.



A, Stiff wire; B, Diaphragm; c, Hearing tube; D, Perforated handle.

phenomenon observed, and as a consequence he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science, April 21, 1881, that audible sounds result from the expansion and contraction of the material exposed to the beam, and that a real to-and-fro

vibration of the diaphragm occurs capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the supports (A B) of the microphone might touch portions of the diaphragm which were practically at rest. It would of course be interesting to ascertain whether any such localisation of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated (see Fig. 2).

The instrument is a modification of the form of microphone devised in 1827 by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire (A), one end of which is rigidly attached to the centre of a metallic diaphragm (B). In Wheatstone's original arrangement the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body, like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are conveyed to the ear through a rubber hearing-tube (C). The wire passes through the perforated handle (D), and is exposed only at the extremity. When the point (A) was rested against the centre of a diaphragm upon which was focussed an intermittent beam of sunlight, a clear musical tone was perceived by applying the ear to the hearing-tube (C). The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area, on both sides of the diaphragm, the sounds became weaker and weaker, until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes microphone (see Fig. 1) no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may therefore be reconciled with the positive results obtained in America by Mr. Painter and myself. A still more curious demonstration of localisation of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram.) and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

In this experiment, as in the case of the thin diaphragm, absolute contact between the point of the microphone and the surface explored was necessary in order to obtain audible effects. Now I do not mean to deny that sound-waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Rayleigh actually occurs, and that it is sufficient to account for the audible effects observed.

EXPERIMENTAL DETERMINATION OF THE VELOCITY OF WHITE AND COLOURED LIGHT¹

THE method employed in this research to measure the velocity of light resembled the method of M. Fizeau, subsequently employed by M. Cornu. A revolving toothed wheel is employed in the same way to alter the intensity of the light reflected from a distance. In the present method, however, there are two distant reflectors instead of only one. They are separated by a distance of a quarter of a mile. The observing telescope and the two reflectors are almost in the same line. The observer sees two stars of light, which go through their phases with different periods as the toothed wheel is revolved at increasing speeds. One star is increasing, while the other is diminishing, in intensity, with increase of speed of the toothed wheel. The speed required to produce equality of the light is determined by means of a chronograph.

By choosing such a speed as gives a maximum of one star at the same speed as a minimum of the other, a pair of observations

¹ Abstract of a paper by Dr. J. Young, F.R.S., and Prof. G. Forbes, read before the Royal Society, March 19.

eliminates all cause of doubt arising from varying brightness in the stars, and ratio of the width of a tooth to the width of a space. The distances were observed by triangulation with the Ordnance Survey 18-inch theodolite, using as a base line a side of one of the Ordnance Survey triangles. The source of light was an electric lamp. The velocities (uncorrected for rate of clock, and reduction to a vacuum) measured are as follows:—

187,707
188,405
187,676
186,457
185,788
186,495
187,003
186,190
186,830
187,266
188,110
188,079

Mean 187,167 miles a second.

The correction to vacuum is + 54 miles a second. The correction for rate of clock to a mean solar time is + 52 miles a second.

The final results for the velocity of the light from an electric lamp *in vacuo* is 187,273 miles a second, or 301,382 kilometres a second.

Using Struve's constant of aberration 20'445", we obtain for the solar parallax the value 8'77", and for the mean distance of the sun 93,223,000 miles.

On February 11, 1881, the reflected stars were seen to be coloured, one reddish, the other bluish. The particular colour of a particular star depended upon the speed of rotation of the toothed wheel. That star which was increasing with increase of speed of the toothed wheel was reddish, that one that was diminishing with increase of speed was bluish. This seems to be caused by the fact that blue rays travel quicker than red rays.

A number of tests were made to judge of the accuracy of this conclusion, and they confirmed it. In the final arrangements, the electric light was acted upon by a bisulphide of carbon prism, and part of a pure spectrum was used. Differential measurements were then made to find the difference in velocity of rotation of the toothed wheel, required to produce equality of red and of blue lights. The most convenient method was to use a driving weight slightly in excess of that required to produce equality of the light, then to fix to the pulley carrying the weights one end of a piece of stout india-rubber tubing, the other end being fixed to a point above. This gradually diminished the effective driving weight. The equality of red lights was first noted, the colour of the light was changed, and the interval of time until the blue lights were equal was measured. The rate at which the india-rubber diminished the speed was afterwards measured by the aid of the chronograph, and thus the difference of speed determined. The mean of thirty-seven determinations in this and other ways gave the result that the difference in velocity between red and blue lights is about 1'8 per cent. of the whole velocity, blue travelling most rapidly.

The general conclusion seems to be supported by a comparison of the velocity of light measured by M. Cornu and Mr. Michelson, where the source of light usually employed is taken into consideration. These are the only accurate measurements of the velocity of light hitherto published. They give us the following results:—

	Usual Source of Light.	Velocity in kilos. a Second.
Michelson's research ...	The sun near horizon ...	299,940
Cornu's ...	Lime light ...	300,400
The present ...	Electric light... ..	301,382

Classifying the sources of light used by Cornu, we get the following approximate relative velocities:—

Source of Light.	No. of Observations.	Approximate Relative Velocity.
Petroleum	20	298,776 kilos.
Sun near horizon ...	77	300,242 "
Lime light... ..	449	300,290 "

All these results seem to support the view that the more refrangible the source of light, the greater is the velocity. But the evidence of the present observations, indicating an excess of