

yond that these latter are placed closer than usual, and of greater strength, there is not much that differs in principle from other suspension bridges. The roadway in suspension bridges is usually hung to the ropes and tie-rods, and there is an end of the work. In Mr. Barlow's bridge, however, a new principle is introduced, which almost, if not quite, does away with the lateral and vertical motion so dangerous to ordinary suspension bridges, and which has rendered some in this country and many in America almost useless for heavy traffic. This consists of taking under the floor of the bridge what may be called two powerful longitudinal box girders, one on each side. The sectional area of each of these is 40 inches, and each is 2 feet 3 inches deep by 18 inches wide. These diminish any upward or downward movement to a *minimum*, and absolutely check all lateral swing. To these girders, which are, in fact, the backbone of the whole structure, the lattice tie-rods we have described are fastened, and thus such rigidity is given that, calculating according to the strain wrought iron ought to bear per inch, it is said that the whole floor of the bridge, if laid sideways, would even then be strong enough for its traffic.

Between these main box girders, which run from end to end of the whole structure, wrought iron cross girders are laid at intervals of four feet apart. On these again are wrought iron plates for the roadway, which is paved with a wooden pavement, set in mineral pitch, so as to give elasticity to the thoroughfare, while securing the iron-work beneath it from the action of either air or water. The footways on each side have a width of 6 feet, though they certainly do not seem to have even this narrow limit. After the spacious sidewalks of new Westminster, these appear like mere alleys by comparison. These footpaths on each side are carried on cantilevers or iron brackets projecting from beneath the roadway. Everything being made to do some duty in the strength of this singular bridge, the parapets of the footways are formed of wrought iron lattice work, which in itself gives a support and rigidity to the otherwise light path. The paving of the footways is of Portland stone from old Westminster Bridge, cut in thin neat slabs. In the ornamental scrollwork of the brackets which carry them, the mains of the Lambeth Gas Company, 18 inches in diameter, cross the river, one under each side of the bridge. From the river these have a rather ornamental moulding appearance,—a matter in which the whole structure is, to say the least, deficient.

MECHANICS, PHYSICS, AND CHEMISTRY.

For the Journal of the Franklin Institute.

Comments on Professor Tyndal's Lecture on Force. By ELI W. BLAKE.

The lecture of Professor Tyndal, published in late numbers of this Journal, is entitled a "Lecture on Force." It might, however, be more appropriately entitled a Lecture on the Dynamic Theory of Heat. I had proposed to myself to examine some of the bold and extravagant

statements put forth in this lecture in exemplification and support of this theory; but as a popular lecture on an abstruse topic is not, perhaps, a fair subject of scientific criticism, I pass these by and make the lecture rather the occasion than the subject of this communication.

Whatever foundation there may be in nature for the dynamic theory of heat, it is certain that arguments have been employed in its behalf which are based on error. To correct one of these is the object of this paper.

Professor Tyndal states that a German named Mayer, to whom he gives the credit of having originated the dynamic theory of heat, computed the mechanical equivalent of heat from the velocity of sound. Other advocates of this theory have alleged that the mechanical equivalent thus found is in exact accordance with that deduced from other sources. No argument in behalf of the theory has been used by its advocates with more confidence and effect than this alleged coincidence; and it is not to be denied that such a coincidence, if the computations were founded upon correct and sufficient data, would be significant, and indicative of a general law governing the relations of heat to force. It is proposed to show that those who have computed the equivalent from the velocity of sound, have not been in possession of correct and sufficient data for that purpose; and consequently that the alleged coincidence, however obtained, has no such significance as has been ascribed to it.

In order to compute the mechanical equivalent of heat from the velocity of sound, it is necessary to know, first, the true theoretical velocity of the sonorous wave independently of any acceleration by heat; and secondly, we must find by experiment the actual velocity of the same wave. Then, assuming the difference of these velocities to be due to heat, and knowing the ratio in which the elastic force of the air is increased by the heat evolved by its compression, we may compute the mechanical equivalent of the heat.

Let us look at the character of the data upon which these computations have been founded. Newton, who was the first to attempt to solve the problem of the velocity of sound, arrived at the conclusion that waves of sound move with the velocity which a body would acquire by falling through half the height of a homogeneous atmosphere. But upon examining his reasoning on this subject, as given in the *Principia*, we shall be struck with the fact that it lacks the precision and clearness of logical sequence which usually characterize Newton's mathematical arguments. Other mathematicians, not satisfied with Newton's reasoning, yet not perceiving the true nature of its defect, have attempted to solve the problem by processes differing from that employed by Newton, yet involving the same defect. As they all arrived at the same conclusion as Newton did, it has been accepted as an established truth that the theoretical velocity of waves of sound is that which a body would acquire by falling through half the height of a homogeneous atmosphere (or about 946 feet per second), and that all sonorous waves have the same theoretical velocity. This, no

doubt, was assumed to be the theoretical velocity of sound by those who have computed from it the mechanical equivalent of heat.

In the next place we would inquire, what have they assumed to be the actual velocity of sound as found by experiment? Numerous experiments have been made by different observers, but their results differ widely, varying from 1100 feet or less, up to 1474 feet per second. Now it cannot be shown that one of these observations is less correct than another. By what rule, then, has one of them been accepted and the rest rejected? If a mechanical equivalent of a prescribed value was sought for, no doubt, within this wide range of experimental velocities, one could be selected that would meet the exigency; but what would be the value or significance of a coincidence obtained in that way?

But this is not the only nor the principal objection to the legitimacy of such a mechanical equivalent. It is not true, as here assumed, that waves of sound have all the same velocity. On the contrary, their theoretical velocities vary through a still wider range than those found by experiment.

Waves of sound are of two kinds, viz: waves of rarefaction and waves of condensation. Waves of rarefaction vary in their theoretical velocities from that which would be acquired by falling through one-fourth the height of a homogeneous atmosphere (about 668 feet per second), up to that which would be acquired by falling through one-half the height of a homogeneous atmosphere (about 946 feet per second.) Waves of condensation vary in their theoretical velocities from that which would be acquired by falling through half the height of a homogeneous atmosphere, or 946 feet per second, upward without limit. The cause of the variation in the velocities of waves of sound is their difference in intensity; that is, in the extent to which the air is condensed or rarefied in the wave. The law of variation is this, viz: the velocity of the wave is as the square root of the ratio of the density of the wave to the natural density of the air through which it moves. The velocity, 668 feet per second, is that which pertains to a wave whose density is half the natural density. This is the smallest velocity any sonorous wave can have, for the reason that if the cause which at any point originates a wave, produces at that point any greater degree of rarefaction than corresponds to a density of one-half, still the wave which will be propagated from that point will be one whose density is half the natural density. The velocity supposed by Newton and others to pertain to all sonorous waves, viz: that which a body would acquire by falling through half the height of a homogeneous atmosphere, is that which belongs to a wave whose density is equal to the natural density. This lies at the boundary between waves of rarefaction and waves of condensation. Strictly, it is the only velocity in the whole range which no wave can have, as in this case the intensity of the wave is 0.

The laws of waves, as here stated, are susceptible of rigid demonstration. The course of reasoning by which they are established may be seen by reference to an article in the *American Journal of Science*,

2d Series, Vol. 5, page 372. The waves contemplated in the article referred to, are waves of condensation only; but the same course of reasoning, *mutatis mutandis*, applied to waves of rarefaction, will give the results above stated as pertaining to that class of waves, with the exception of the lower limit to their range of velocities. That there is such a limit follows from the principles developed in another article in the same Journal, 2d Series, Vol. 9, page 334.

From what has been stated it follows that in order to know the true theoretical velocity of any wave of sound, we must know its density. And as the densities of the waves whose actual velocities have been observed, have not been noted by any observer, it is obvious that we do not possess the requisite data for determining the mechanical equivalent of heat from the velocity of sound.

Proceedings of the Manchester Association for the Prevention of Steam Boiler Explosions.

From the *Mechanic's Magazine*, November, 1862.

At the last ordinary monthly meeting of the executive committee of this association, held on Tuesday, Nov. 25, 1862, Mr. L. E. Fletcher, chief engineer, presented his monthly report, of which the following is an abstract:—

During the past month there have been examined 365 engines and 547 boilers. Of the latter 8 have been examined internally, 60 thoroughly, and 479 externally, in which the following defects have been found:—Fracture, 5 (1 dangerous); corrosion, 38 (3 dangerous); safety-valves out of order, 13; water-gauges ditto, 31; pressure-gauges ditto, 9; feed apparatus ditto, 6; blow-off cocks ditto, 47 (1 dangerous); fusible plugs ditto, 3; furnaces out of shape, 6 (2 dangerous); blistered plates, 3; deficiency of water, 1. Total 162 (7 dangerous). Boilers without glass water-gauges, 10; without pressure-gauges, 2; without blow-off cocks, 38; without back pressure-valves, 78.

An explosion has occurred this month to the boiler of a first-class passenger locomotive engine, by which 3 persons were killed and others injured. It was considered to be perfectly safe, had been on duty the previous day, and was being cleaned ready for work at the moment the explosion occurred.

It will be remembered that reference was made in the July, 1861, report, to another explosion of a locomotive boiler, which took place while the train was running; and since that time three others have occurred in addition to the one first alluded to, thus making five during that period with this class of boiler.

The cause of explosion in four of these cases proved to be thinning of the plates from internal corrosion. I have only had an opportunity of examining the plates of one of these exploded boilers, but from official reports, it appears that the corrosive action had developed itself in a very similar manner in each instance, which in the one personally examined was as follows:—The corrosion had eaten grooves or furrows parallel with and close to the edge of the overlaps of the