ART. XXVI.—*On the Intensity of Sound: A Reply to a Critic;* by CHARLES K. WEAD.

SEVERAL years ago I published in this Journal* some results of experimental work on the sounds from tuning forks. The final results as given then in Table VI, col. 6, showed results agreeing far better than one might expect who knew the difficulties, both experimental and theoretical, inherent in the problem.

The want of agreement, however, was great enough to attract the attention of Professor A. Stefanini, who, without repeating the experiments, has attempted to dragoon the figures into line by mathematical processes. An abstract † of his paper indicates a total misapprehension of the problem, and the original ‡ (only recently accessible in this country) confirms this opinion of his work.

The critic first verifies my formula (5) for the potential energy of a bent fork, and then determines its mean kinetic energy during a vibration; by a laborious process he finds this to be half of the potential energy at the extreme of vibration —a conclusion that is one of the common-places of the theory of simple harmonic motion. But having obtained this formula he inserts the numbers given in my Table VI, cols. 2 or 3, then divides by the surface of the hemisphere, whose radius is 200 ft.; as the quotients for the different forks agree more closely than my results for S in col. 6, he thinks he is justified in considering that his method of discussing the observations is the better one.

Other attempts lead to similar conclusions and are based on the same utterly untenable premises. The critic considers the energy in the fork at any instant, and assumes tacitly that the sound given out is closely proportional to the energy: any one who has ever touched the stem of a vibrating fork to a sounding board, or held the fork before a suitable resonator, should appreciate readily the fact that it is *not the amount of energy in the fork* that is to be thought of in considering the intensity of the sound, *but the rate at which energy is given up* to the air. On this point text books are misleading when they say the loudness of sound is proportional to the square of the amplitude of vibration of the sounding body.

No matter how much energy the body may have, it is only the small fraction being given off at any instant that can cause the sensation of sound—and indeed only a part of this is effec-

* This Journal, III, xxvi, p. 177, 1883.
† Beibl. Ann. Phys. Chem., xiii, 636, 1889.
‡ Atti della R. Acc. di Lucca, xxv, 239–262, 1888.

tive in producing sound. Yet it is often assumed that the effective part of this fraction is a constant fraction of the total energy, for all amplitudes of vibration. The critic, I think, did this, as I can find no other possible explanation of the mistake he has made; but such an assumption is pure guess-work.

In the paper referred to, a considerable part of the work reported bore on the determination of the rate of loss of energy; it was found for example that, with Ut_3 fork at its maximum amplitude, it lost energy at a rate that would, if it had been continued uniform, have exhausted the supply in $4\frac{1}{2}$ sec.; but when the amplitude was very small, the rate too was reduced, and the corresponding time was 15 sec.

Stefanini, in a later paper,* criticises the form in which I write the exponent expressing the damping effect, viz:

$$\varkappa t = \left(a + b\,\frac{z' + z''}{2}\right)t$$

Where z' and z'' are the amplitudes at the beginning and end of the time t. He prefers to write it at^m; he finds for several forks, whose vibrations he photographed, that m was tolerably constant for each fork; for different forks it varied between 0·84 and 0·94. An important difference between the two formulas is that according to Stefanini's, the amplitudes at the end of successive equal times should be in a geometric series, or if the ratio of amplitudes is constant the intervals are constant; according to my formula in the last case, the intervals will increase as the amplitude diminishes. Stefanini used long thin forks intended for producing Lissajous's curves. From one of his tables (Tab. IX, p. 330 for Sol,) we deduce the following:

Time, arbitrary units	0	10	20	30	40	50	60
Interval		10	10	10	10	10	10
Amplitude on photograph,	48·3	32·3	22·8	16·35	12·2	9·2	6·6
Ratio		1·50	1·41	1·39	1·34	1·33	1·39

My Table II, p. 182, gives for Sol_3 the following ratios equal 2.

Amplitudes in div.	10–5;	8–4;	6–3;	4–2.
Intervals, sec.	4·70	5·34	6·31	8·17

Evidently the new formula does not fit any of the experiments exactly; like mine, it is only empirical, not rational; every experimenter must determine the formula that best fits his own conditions of working.

In this second and longer paper the author attempts to determine experimentally by forks whether the intensity of sound

* Beiblätter, xiii, 871. Atti, xxv, 307–400.

is to be measured by the energy or the momentum of the sounding body. Here unfortunately he falls into the old error, and tacitly assumes that the movement of the air corresponds exactly with that of the fork.

On this perplexing subject of the measure of a sound-sensation, most methods of experimenting are very faulty, as the following considerations will show:

First. When the experiment is to determine either the *minimum audible* or the equality of two sounds, if the sensations be the same, the causes must be the same; these causes are the movements of the particles of air near the ear, and the movements therefore being the same, their measure, whether as energy or momentum, must be the same. No discrimination then between rival theories can be made by the method of equal sounds.

Second. It is tacitly assumed in most of the experiments that I know of, whether by dropping balls or hammers, or by telephonic arrangements of various kinds, that the efficiency of the apparatus is substantially the same under all conditions, and in none has it been determined. Any one familiar with modern mechanical measures knows how much attention is given to determining under all the varying practical conditions the efficiency of electric, thermal and other machinery. But such laborious work as that of Vierordt and his followers seems much like trying to determine the laws of vision by first producing equality of illumination between a tiny incandescent lamp supplied with current from a toy boiler, engine and dynamo, and a distant powerful arc lamp with its boiler, engine and dynamo supplying it with many horse power, and then dividing the mechanical equivalent of the fuel used in each apparatus by the square of the respective distance from the eye or screen. Such a method might give the ratio of the efficiencies, but not anything of much value regarding the laws of vision. Every one knows that a ball falling on a metal plate will cause a louder sound than when falling on cloth; that is, the efficiency of the first as a sound-producing mechanism is much greater than the second; presumably if any other condition were changed, height of fall, weight of ball, material, etc., the efficiency would be changed; but experiments have been heaped up on the tacit but improbable assumption that it is not changed.

In conclusion, I desire to point out that my determinations for S, the energy per sq. c.m. per sec. at the limit of hearing, must be taken as outside values; they must be multiplied by the efficiency of the apparatus as a sound-producer; certain experiments have led me to infer that for the Ut_3 fork the efficiency was about 7 per cent, as stated in the original paper;

but this number is by no means certain, nor is it probably the same for all the forks; while the determination of its exact amount is doubtless difficult, I believe it will yet be made with fair accuracy.

Attention is asked to a few corrections noted below.

ERRATA.

To vol. xxvi, Sept., 1883, p. 179, line 1, for $2Vh$ read $2V_h$.

Page 181, equation (12), for p_2, read p^2.

Page 185, line 14, read mean velocity of the end of the prong.

Page 185, line 23, omit the exponent of l.

Page 187, Table V. column 6, footing, for 2·5, read 1·3; column 8, for Sol_4, read 1·26 and 1·33, mean 1·3.

Page 188, Table VI, column 5, 6, Ut_1, read 2550 and 1100: Table VI, line Sol_4, read 1.3; 127; ·29; 3680; 1590; 200.

Page 189, near close, read The smallest value of x to be obtained from Table VI, is (21×10^{-8} cm. for Ut_4 corrected) 9×10^{-8} cm. for Ut_5: from Allard's formula for $n=1024$ we get 21×10^{-8} cm.