

CORRESPONDENCE.

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LOUDNESS vs. INTENSITY OF SOUND.

To the Editor of "SCIENCE."

Will it seem like firing a blank cartridge at Copenhagen to urge that writers on acoustics ought more carefully to distinguish between the words loudness and intensity as applied to sound? We think not, so long as statements like the following are found in elementary manuals of physics; or so long as the language of even distinguished lecturers on Sound is not wholly free from similar indefinite expressions.

For instance, the law of variation in intensity of a sound free to move in a homogeneous medium is often stated in substance thus: the intensity *or loudness* of sound decreases as the square of the distance. As an illustration it is sometimes added, a sound at the distance two will be only one fourth as loud as at the distance one. While as a triumphant proof or verification of this law, it is often said: a single bell at the distance of ten yards will sound as loud as four similar bells at twenty yards.

It is well known that the word sound and several of the terms used in describing sound have two meanings. The word loudness primarily refers to the sensation of hearing. In order to avoid confusion of thought, I esteem it important that the use of this word be restricted to the sensation, and that the word intensity (or volume) shall refer solely to the external vibrations which are the cause of the sensation. In other words, loudness ought always to be used in a subjective, intensity in an objective, sense.

What is meant by such expressions as those above quoted? Perhaps they are simply examples of a loose use of language, but it will certainly be natural for the unwary reader to infer from them that loudness and intensity vary according to the same laws, and also that we can by the ear verify those laws. I have no hesitation in affirming that this use either purposely or otherwise, of the word loudness as synonymous with the word intensity, has been the cause of great confusion of thought, and has often loaded down the undulatory theory of sound with that which is really foreign to it. The time has come when we ought to regard that treatise on sound as a failure in one important respect which does not leave the reader thoroughly imbued with the idea that the law of variation in the intensity of a sound refers to sound vibrations and not to the intensity of the sensation of hearing.

But if those who use such expressions as have been quoted, really mean to claim that loudness, i. e., relative intensity of sensation, varies according to the same laws as the energy of the moving molecules of the sound wave, or if it is claimed that by the ear we can accurately and validly verify the law, then it will be in order to demand the proof.

In order that such physiological laws may be proved true it must be shown, either, 1, that we can accurately know when one of our sensations is a multiple of another (as when one sound is four times as loud to the ear as another), or, 2, that we can recognize sensations of equal intensity; and, 3, it must also be proved that the intensity of the sensation is proportional to the energy of the blow causing the sensation. These assumptions cannot be proved.

1. It goes without saying that any one having normal senses can tell a heavy blow from a light one, and can recognize degrees of intensity among sounds, lights, heats, tastes, and smells. But if it is claimed that there are quantitative relations between sensations of different intensity, and that we can by consciousness recognize these ratios, we at once become committed to a remarkable system of mathematics. Since experience shows that the senses

are easily deceived and that different persons disagree as to their estimates, who shall decide what are the true ratios? But we can only compare the relative intensities of two sensations by memory. Here is a fruitful source of uncertainty, for before we can be sure that one sound is to our ears four times as loud as another, we must be certain that we can by memory reproduce the first sensation and place it beside the second in exactly its true intensity. No one can be sure of this. This reasoning applies to those who have perfect senses, if there are such. When we consider the myriad degrees of nerve sensitiveness, partly congenital, partly the result of habit, and partly the result of disease, the problem becomes still more difficult, ludicrously so.

2. Can we recognize sensations of equal intensity? No doubt we can do so much more exactly than we can estimate the relation between sensations varying greatly in intensity. Yet here we meet the same cause of doubt as before,—the uncertainty of memory. The less is the time intervening between two distinct and independent sensations, the more nearly we can estimate their true intensities. In comparing sounds, somewhat more than one sixteenth of a second must elapse between them. In the photometer the lights or shadows are shown in contrast and are thrown side by side upon the screen, where we can see them simultaneously or pass from one to the other very quickly. Probably there is no way whereby we can compare two sensations more accurately than by the photometer, yet no one will claim that he can move the lights so that their intensities shall be exactly equal on the screen. All he can say is: to the eye they are equal. If then under the most favorable conditions, there is a residuum of doubt, the sense of hearing will be still more untrustworthy; I regard it, therefore, as a fallacious method of research to bring physical laws to be tested by the uncertainties of sensation. Can feeling demonstrate the accuracy of a thermometer, or can the laws of energy be verified by striking ourselves blows with moving bodies? All that we can say is that within certain limits the testimony of our senses approximately conforms to the laws which have been deduced from more accurate observations and reasoning.

3. Are sensations proportional to the energy of the impacts producing them? They must be, if loudness and intensity vary according to the same laws, or if equal sensations are caused by equal blows. The hypothesis is manifestly absurd as a general law, for we are unconscious of very weak blows, and very violent ones either destroy the nerves or paralyze them by what is known as shock. Even within the most favorable limits the rule can only be approximately true, and if it were true, could not be proved, for the nerves retain their impressions for a variable length of time, and this marks a limit to the intervals at which we can repeat impressions of normal intensity free from the residual effect of previous impressions. Hence if impressions be repeated too soon they will generally cause a progressive deadening of the nerve sensitiveness, or sometimes an increased sensitiveness, as in the case of the punishment of the bastinado. Even if there were nervous conditions such that the sensation was proportional to the energy of the impact, it would be difficult if not impossible to prove that the nerves were in the proper condition at any given time. Into such a tangled maze of uncertainties are we led when we try to pervert our senses, admirable in their proper sphere, into mechanisms for the quantitative estimation of energy!

If it be said that a single bell at the distance one will sound as loud as four bells at the distance two, it must be assumed that the ear is equally well adapted for receiving and transmitting all sounds, irrespective of the shape of their wave front. I will omit from the following discussion all the complications which spring from differences in the pitch and timbre of sounds and will premise a perfect ear and nerves.

According to the undulatory theory of sound, the wave

front in a homogeneous medium is a spherical surface, and the rays of sound proceed outwards in all directions and in straight lines; hence the nearer is the source of sound, the more convex is the wave front and the more diverging are the rays. When the nearly parallel rays of sound proceeding from a distant point, strike the cup-shaped outer ear, a part is reflected toward the centre and thus reinforce the rays which directly enter the external opening of the ear. If a sound proceed from a point very near the ear the rays will be so diverging that all, except such as directly enter the opening, will be reflected outwards and will be lost. Hence it is evident that a far sound will seem louder than a near one, if their vibrations are of equal intensity as they come to the outer ear. This will at once upset the theory that loudness and intensity vary according to the same laws, unless in some way the far sound shall lose its advantage after entering the external meatus; but, as they enter the tube, the diverging rays of the near sound will strike obliquely outwards against the walls and will be reflected. Thus a part of their energy will be lost, a much larger proportionate loss than will come to the more parallel rays of the far sound. When at length after various reflections from the walls of the crooked meatus, the waves are wedged between one wall and the membrane of the tympanum which is placed obliquely across the inner end of the tube, the rays will fall upon the concave outer surface of the membrane, and a part will be converged. The more parallel rays of the distant sound will be more converged than those of the near sound, and hence will reinforce the impulse at the center of the membrane more than the other; but the center is the point of greatest leverage against the hammer bone which is fastened to the back of the membrane; hence nearly parallel rays of sound would more violently agitate the tympanum of the inner ear than more diverging rays, even though both were of the same intensity before striking the concave membrane of the tympanum. The comparison by the ear of the intensities of two sounds would be still more untrustworthy if one of the sources of sound were within the outer tube of the ear.

Loudness, that is, the intensity of sound sensations, does not, then, depend upon the energy of the external sound vibrations, but upon the proportion of the energy which the mechanism of the ear is able to transmit to the auditory nerves, which amount is variable. The ear is so made as to relatively strengthen distant sounds and to weaken near ones, and it is so much the better an instrument because of this, for we are thereby saved from too violent shocks of the nerves, which are most likely to come from near sounds, while at the same time we retain a wide range of hearing. Such illustrations as that of the bells would not be chargeable with setting up a false test for the verification of physical laws, if it was not at the same time explained that the intensity of the sensation of hearing does not, and in consequence of the peculiar construction of the ear, cannot vary as the energy of the moving particles of the sound wave; also that at certain distances the testimony of the ear will approximately coincide, at other distances it will not coincide with the laws of intensity of sound which have been established by mathematical reasoning. The errors involved in the argument from the bells are very commonly held; it is not evident that all such arguments ought to be eliminated from treatises on sound, or at least that their true significance ought to be explained, and that the distinction should be more clearly defined between the subjective word loudness and objective word intensity.

GEORGE H. STONE.

COLORADO SPRINGS, December 1st., 1881.

NEW YORK, Dec. 19th.

To the Editor of "SCIENCE."

In the official report of my paper read before the N. Y. Academy of Sciences, published in your last issue (Dec. 16th), I notice the cost of the balloon is given at about

£12,000, whereas the amount should have been £4,000.

The report also states, "the great body of warm water that flows northward by the peninsula of Norway and Sweden strikes the lighter currents near the Pole and goes on as a submarine current, sweeping around the Pole till it goes out again through Smith's Sound." I desire to say that it is obvious that only a part of the current passes through Smith's Sound.

Respectfully,

JOHN P. CHEYNE, R.N., F.R.G.S.

To the Editor of "SCIENCE."

Sir,—In No. 12 of this year's *American Naturalist* I notice a short paragraph on 'fossil organisms in meteorites.' The subject certainly is interesting and it seems perfectly proper that the '*A. N.*' should at last take notice of it.

The only objection that I may be allowed to raise on behalf of "SCIENCE" and perhaps of myself is that the *American Naturalist* did not duly give credit for what had been reprinted from your columns.* I cannot conceive any plausible reason—unless it be an oversight—why this simple duty of editorial courtesy should be neglected by an American contemporary, while every English scientific journal takes pains to give due credit to "SCIENCE" for all the various data and notes which are gleaned from its columns (*e. g.* Jour. Microsc. Soc., *Lancet*, *Crookes' Journal*, *Journal of Science*.)

As to the sceptical remarks with which the *A. N.*'s paragraph concludes, to the effect that "a great deal more evidence will be required by biologists before crediting these alleged discoveries," I may refer all sceptics to Mr. Darwin's opinion, as reported in No. 61 of your valuable journal and to any (silicious) meteorite on which they can lay their hands and grind transparent sections from. This will go far to supply the wanted evidence.

Very respectfully,

GEO. W. RACHEL, M. D.

To the Editor of "SCIENCE."

NASHVILLE, TENN., Nov. 30, 1811.

Dear Sir,—I have to-day received from Mr. H. H. Warner, of Rochester, N. Y., \$200 (two hundred dollars), the "Warner Comet Prize" for the discovery of Comet E, 1881, on Sept. 17.

Respectfully,

E. E. BARNARD.

MUSICAL FENCES.

In the abstract of an interesting paper by Prof. S. W. Robinson, in a recent number of "SCIENCE," the author begins with the statement that "this sketch is mainly of a simple fact of observation." He gives then a clear exposition of the acoustic phenomena observed by him in walking past picket fences, and the mathematical formula expressing the law of retrogression of pitch.

The observation is by no means new. I am unable to say at what time it was first published, if at all, but am sure that it was made nearly as far back as twenty years ago. On the crisp, cold morning of December 31st, 1861, while taking a walk with Prof. Joseph Le Conte, myself being innocent of mathematics on account of my youth, we noticed the whistling sound returned by a picket fence past which we were moving, our feet striking sharply against the frozen earth. My fondness for music made me particularly appreciative of a musical fence, and I have noticed the phenomenon hundreds of times since that date, knowing its explanation qualitatively, though I did not deduce the formula. If the fence be long, and the distance between the wickets considerable, the returning whistle may be much longer in duration than a quarter of a second. The stroke of a hammer on a board

*S. my paper on the subject in SCIENCE No. 50.