The Journal of Laryngology and Otology
October 1989. Vol. **103.** pp. 991–993

Letters to the Editor

Tuning forks: the origin of interfering waveforms

Dear Sir,

I was pleased to read the paper—'Tuning forks: the problem of striking' by Samuel and Eitelberg (1989, **103:** 1–6). In particular I was interested in the comment that there was 'no significant difference in the sound of intensity whether the auditory axis of the ear was identical to the acoustic axis of the fork, or perpendicular to it.' This is a point which should be noted by all otologists but particularly by those who have occasion to preside at fellowship examinations. I must disagree however with the authors interpretation that the maximum sound intensity, in the direction perpendicular to the acoustic axis, is the result of the 'geometric sum' of the two sound intensities arising from each prong.

If we consider the space between the two tuning fork prongs; This space experiences alternating contraction and expansion of it's volume as both prongs of the fork vibrate. This continuous volume change of the space between the two prongs produces compression and rarefaction in the air perpendicular to the acoustic axis of the fork. In effect, the space between the two prongs of the fork act as a source for the sound which is heard maximally perpendicular to the axis of the fork.

The advantage which this explanation has over the mere 'geometric sum' of the sound from each prong is that it explains the phenomenon of the 'deadpoint' of low sound intensity in the oblique positions of the tuning fork. This oblique position of the tuning fork actually represents the point of interference of two wave sources, one arising directly from the prong and the other arising indirectly from the space between the two prongs. These two waveforms, although of the same frequency, are by virtue of the mechanism described above completely out of phase with one another. Hence at the nodal point produced by their interaction no sound is heard.

Overall, I think it is important to make these seemingly fine distinctions. Otherwise, explainable physics' phenomena become enshrouded in mystery.

Yours faithfully,

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Dear Sir,

I find the article by Samuel and Eitelberg (1989, **103:** 1–6) interesting but wish to clarify the following:

(i) What was the type of Mic used? Was it condenser, piezo-electric of capacitor?

(ii) What does the statement . . . 'Since many of the measurements were near the sensitivity limits of our equipment, the unavoidable circumstances let the recorded waves appear slightly non-smooth' . . . mean? What do they intend to state when they say sensitivity limits? Also, what are those unavoidable circumstances for recording? Recording in an anechoic chamber or use of filters eliminates unwanted features and improves the recordings.

(iii) Were the waveforms recorded by direct placement of the stem of the Mic? If so, does ambient noise affect such recordings? If the idea was to record vibrations, would an accelerometer be a better choice?

(iv) the problem of striking, viz, the force with which a tuning fork is activated—remains, in spite of this article. The authors do not address another common problem; what is the correct amount of force needed to activate a 256 Hz tuning fork without overtones, assuming that the consistency of the surface used for striking does not matter?

Yours faithfully,

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Dear Sir,

Dr Samuel has passed your letter of 5 May 1989 with the questions of Dr Rao to me, since it concerns only the technical aspects of our paper on tuning forks and I was responsible for the actual measurements and their physical interpretation. Since I have left the university where the tests were carried out, I have to rely to some degree on my memory:

(i) I think it was a condenser type microphone. In any case it was one of the best used in the audio work, most definitely covering the 20 to 20 000 Hz range.

(ii) I did not bother to use any audio amplifiers, since the digital storage oscilloscope's maximum amplification was alone just enough. The non-smoothness comes mainly from the relatively low 8-bit digitization and from the (ever-present) interference. Neither of them affected the results of our paper, hence there was no need to improve our recordings. The suggested anechoic chamber and filters cannot be justified from my (engineering) point of view.

(iii) During the 'bone conduction' simulation the tuning fork stem was placed directly on the microphone

and on the bone behind my ear—with identical results. Unfortunately we could record only the first of the two. The ambient noise does affect the recordings but not the results of our article. We could have used an accelerometer—with no advantages and it would have cost me a few extra hours of unnecessary work.

(iv) It is indeed correct that our article does not address the problem of the correct striking force. There are at least two reasons for this:
   (a) the first author has not asked me to do it;
   (b) measurement or dosage of the correct force should make this equipment more expensive than an electronic sound generator, which can very easily contain tuning knobs for frequency, volume, overtones, etc.

   If this electronic 'tuning fork' is not yet available on the market and there are at least 200 buyers, I will almost certainly produce myself an electronic four-tone battery-operated 'tuning fork' with no overtones that will cost less than the set of four mechanical tuning forks usually sold to the medical doctors. Please let me know!

Yours faithfully,
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Dear Professor Eitelberg,

Thank you for directing a copy of your letter addressed to Dr Booth, to me, in reply to the questions that I asked of Dr Samuel, in connection with the article on 'Tuning forks' in the January issue of 1989. Please permit me to air a few points that I think are pertinent with respect to your article. I, too, have erased the questions I had posed to Dr Samuel, from my floppy and therefore, have to rely on sheer memory as to the contents of my questions in my earlier letter. Anyway, I shall limit myself to a few comments on the answers that you wrote.

(i) THE ARTICLE SET OUT TO THROW NEW LIGHT ON THE METHOD OF PRODUCING SOUND WITH NO OVERTONES, using tuning forks. Then, does it not stand to reason to GUARANTEE THE ABSENCE OF OVERTONES, BY QUANTIFICATION (whatever the expenses) the amount of correct force being delivered to the forks? Why the first author did not ask you to measure/quantify/guage the amount of force delivered to striking the tuning fork, is unfathomable, at least to me. The basic concept upon which the paper is based is illogical without ensuring elimination of overtones . . .

(ii) AC testing by electronic tuning forks is already in widespread use (with perhaps much better accuracy than conventional ones); the picture would be complete if BC testing can also be handled with such engineering marvels. When both AC/BC mode testing is available on the same model, tuning forks will be history.

Yours sincerely,
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Guillotine and dissection tonsillectomy compared

Dear Sir,

It is encouraging to read that guillotine tonsillectomy still carries support, and that Wake and Glossop's one year study of the technique's use in 50 children concluded that there were advantages of diminished blood loss, operation time, and postoperative pain, when compared with a similar number of tonsillectomies by dissection (Journal of Laryngology and Otology, June 1989).

A detailed, illustrated account of guillotine tonsillectomy, published in your Journal (McGuire, 1967), includes a Cardiff series of 12,523 tonsillectomies in children less than 15 years old, performed over a nine-year period, with no mortality and only a 0.25% rate of tonsillar reactionary haermorrhage. The conclusions have lost none of their validity with time: the technique is rapid and safe, and causes the minimum of tissue trauma, but requires a skilled anaesthetist and an experienced guillotinist. The crushing blade and correct angulation of the Popper guillotine aids haemostasis; in children as in adults, small, flat, or immobile tonsils, or a past history of quinsy are indications for dissection instead.

Both techniques continue to be practised in our Department, and the Popper guillotine is used on an unselected group of approximately 50 children every year, with equally favourable results. It would be interesting to know in how many Departments the technique survives, and thus its chances of continuing as a worthy alternative to dissection.

Yours faithfully,
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References

McGuire, N. M. (1967). A method of guillotine tonsillectomy with an historical review. *Journal of Laryngology and Otology*, **81**: 187–195.

Dear Sir,

Thank you for your letters of the 5 June (passed on to me by Dr Samuel) and the 19 July, 1989. I had a response to Mr G. Moriarty ready almost immediately, but wanted to check my theory by some measurements. I ordered a medical tuning fork which has still not been delivered. Due to the urgency I have decided to respond before I can carry out the quantitative measurements, although the following statement has been verified by the ear of Dr Samuel.

I am pleased by the interest of Mr Moriarty in our

paper and thank him for pointing out my superficiality in explaining one of the observed phenomena. I don't have any difficulty in accepting Mr Moriarty's observation that "the space between the two prongs of the fork act as a source for the sound . . ." as correct (among other possible interpretations) but must reject his conclusion that at some "oblique positions of the tuning fork" no sound or very low sound is heard. The simple reason is in the small dimension of the relevant radiating area—in the order of 1 cm.

All bodies that are small, compared to the wave length, act for practical purposes as point sources. That means that the mechanism, described by Mr Moriarty, and other relevant mechanisms become significantly directional for wavelength's that are shorter or comparable to 1 cm. Since the velocity of sound in the air is about 340 m/s, the corresponding frequencies are above 340/0.01 = 34 kHz. Hence in the audible region below 20 kHz in the air the directionality of the ordinary tuning forks is not significant. It is true that one can hear some directional dependence, mainly if the tuning fork is held very close to the ear. This does not seem to be significant, because the differences are comparable to the sound intensity changes in one or a couple of seconds in time.

Yours faithfully,
Dr.-Ing. habil. Ed. Eitelberg,
Head of Department.

https://doi.org/10.1017/S0022215100110722 Published online by Cambridge University Press