# Incorrect interpretation of physics is applied to justify the hypothesized existence of a traveling wave, while correct application of physics leads to a phase wave.

*Willem Chr. Heerens* *August 26, 2012*

The above title expresses clearly why I strongly feel the need to react on prof. Werner Hemmert's comments before I bring in the second part of my reaction on the dichotic stimulus perception.

Prof. Hemmert stated:
** I really don't want to get into deep discussions about theories, but I feel that it is important (at least for list subscribers who have not spent some time inside the inner ear) to clarify what "the consensus of the field" is, at least from time to time. **

In this statement prof. Hemmert gives me the impression that he observes me as not well informed about cochlear mechanics and that I need to spend more time inside the inner ear.
If this is really his impression, I think my following comments, based on a correct combination of applied physics and commonly known cochlear mechanics, will refute his expressed prejudice.

Prof. Hemmert stated:
** The traveling wave "theory" is based on many OBSERVATIONS and MEASUREMENTS and provides a viable framework to explain lots of them. Yes, you can actually see a "traveling wave" in a cochlea, and you can measure responses in the inner ear with high precision with more advanced techniques (see Békésy, Khanna, Rhode, Patuzzi, Ruggero, Cooper, to name only a few). **

Certainly I studied papers and other published material of some of these coryphées to become familiar with the issue, and no I nowhere mentioned that you cannot observe something that looks like a traveling wave. I have stated that the observed wave – always running from base [RW] to apex [helicotrema] – cannot be a traveling wave that is carrying the sound energy from one place on the basilar membrane [BM] to another place. For instance if you study carefully the extremely high precision laser interferometry BM movement measurements of Tianying Ren* – what I did because in 2004 he asked me to do so and to give him my comments – you will observe that these movements were restricted to a 0.6 mm interval around the center resonance frequency in the 16 kHz region of Mongolian gerbils.
How can you hold upright the hypothesis that a traveling wave is carrying the sound stimulus along the BM if only a very limited part really gets into motion, while on both sides of that area the BM remains at rest?
You can only explain that behavior as an epiphenomenon caused by a stimulus everywhere on the BM in combination with the resonance frequency distribution along the BM. High near the RW and low near the helicotrema. And that epiphenomenon is nothing else but a phase wave. The crowd on the gallery in a stadium produces the same type of phenomenon if they initiate a 'wave' by standing up and sitting down in an along the rows propagating phase order.

* See the paper : Ren T. (2002) Longitudinal pattern of basilar membrane vibration in the sensitive cochlea; Proc Nat Acad Sci USA 99:17101-6

Prof. Hemmert commented:
** Where Willems states that "Without any doubt this is indicating that at least squaring of the input stimulus plays a dominating role" this was not observed in these measurements. **

May I tell you that you must be very careful in the way you set up your experiments and you must take the consequences into account very seriously as well? In all experiments in which you want to observe BM movements evoked by an acoustic or mechanic stimulus it is almost inevitable that the observation area is severely affected by the preparations.
If these preparations result in a situation that a push-pull of perilymph along the BM is no longer possible, and the stimulus exists of a force directly working on the BM, the conditions for the non-stationary Bernoulli effect do not exist anymore. And then the differentiation and squaring of the sound stimulus isn't possible anymore as well and cannot be observed.
The cat experiment of Wever and Lawrence # in 1950 didn't have this problem and in that experiment the quadratic relation between both AC and DC cochlear potentials and the combined stimuli on both OW and RW were present without any doubt.
Wever and Lawrence mentioned a 6 dB [that means a factor 4] increase in both AC and DC cochlear potential differences in case the stimulations of OW and RW added up to a two times greater perilymph push-pull.

# See the paper: Wever EG, Lawrence, M. (1950) The acoustic pathway to the cochlea. JASA 22: 460-7.

Prof. Hemmert commented:
** If you got the impression from Willems response that "slow" traveling waves with wavelengths much smaller than a meter violate all laws of physics, you can calm down. Just do an experiment with yelly to convince yourself from the opposite (and yes, the material properties of the inner ear structures are closer to yelly than steel, where wavelengths are actually in the dimensions Willems stated). **

We can observe a vibrating jelly object in a New Scientist video with the title: 'Sound of jelly wobbling recorded for the first time':

http://www.youtube.com/watch?v=eDn8y7j8kSk

How it is done precisely remains somewhat obscure, but the showed motion has typically the character of a stable vibration resonant mode, that fades away by dampening during a restricted number of periods.
However even then the velocity of sound energy carrying waves in jelly cannot be reduced to the dimensions that fit in the experiments of Tianying Ren:

With a frequency of 16 kHz and a measured wavelength of 0.6 mm the sound velocity in the 'cochlear' jelly would result in 9.6 m/s instead of 1500 m/s for perilymph.

Let us then do the following thought experiment:
Set-up a 1 meter thick wall of jelly, place a speaker in front of it and – if the dampening in jelly wouldn't play a devastating role – at the other side of the wall you would hear the sound with a time delay of 0.1 second, which is normally obtained in air after a distance of 33 meters. It won't happen.

Such a thought experiment is based on the completely erroneous interpretation of the equation in which is expressed that the wave velocity equals the frequency multiplied by the wavelength.

This equation must be interpreted in the following way:

The speed of a sound wave that moves through a medium isn’t dependent on its frequency and its wavelength. The speed of sound – hence also the speed with which sound energy is transported – is a material constant and it therefore only depends on a number of properties of that medium. And the only way to change that speed is to change the properties of the medium.
Once the speed of sound in a medium is determined the above mentioned equation expresses the relation between the sound frequency and the wavelength.
The two have an inverse relationship. Given the frequency of the wave, the wavelength is equal to the speed of sound in the medium divided by the frequency.
Or in reverse: Given the wavelength of the wave, the frequency is equal to the speed of sound in the medium divided by the wavelength.

Measuring both the wavelength in the ‘wave’ of the BM evoked by the frequency stimulus and subsequently calculating the propagation speed of the ‘slow wave’ by multiplying wavelength with frequency has nothing to do with correct physics.

The speed of sound in fluids and solids is given by the square route of the compressibility modulus [in Pascal] divided by the density [in kg/m$^3$].
As an indication: this results in a speed velocity of 1858 m/s for glycerin and 870 m/s for paraffin oil.

Prof. Hemmert continued with:
** And again, there is data, and you can actually see such microscopic waves, watch the Video from Freeman's lab:
http://www.pnas.org/content/suppl/2007/10/08/0703665104.DC1 **

Here again my concerns are that:

1. Here the tectorial membrane [TM] is examined. And if you start to investigate the TM separately you have directly eliminated the possibility of a push-pull stimulus in a tube that is running in front of a membrane located in the wall. Besides that, from the Fig.1 in the paper the stimulus is evoked by a piëzo-actuator at one side of the TM strip, hence no stimulus of endolymph along the TM is present.

2. In the paper we can find that the total length of the TM strip under observation is 450 micrometers in length. And the authors state that if one convolution of a ‘wave’ fits on that 450 micrometers the wavelength is determined.
Instead of using the longitudinal wave propagation in which the compression modulus $E$ and the density $\rho$ play the decisive role, the authors are using the shear force and shear modulus together with the shear viscosity in case of the higher frequencies.

3. What we see in the calculations of the paragraph with the title:

Waves Intrinsic to Dynamic Material Properties of the TM.

is that the authors calculate the ‘wave propagation velocity’ out of the under the microscope measured ‘wavelength’ together with the used stimulus frequency.
And out of this velocity they calculated estimations for the material constants of shear modulus and shear viscosity.

So once again I can repeat the general principle of physics:

Measuring both the wavelength in the ‘wave’ of the BM evoked by the frequency stimulus and subsequently calculating the propagation speed of the ‘slow wave’ by multiplying wavelength with frequency and finally out of this wave propagation speed estimating material constants – like the authors have done – has nothing to do with correct physics.

Look at the results given by the authors: They calculated that the shear modulus in the experiments typically vary from 18 to 40 kPa, however for most of the materials the shear modulus is in the order magnitude of somewhat less than one to even in the tenth of GPa. Giga Pascals, hundreds of thousands to several millions of times higher than the 18 to 40 kPa!
And what to think about the calculated typical shear viscosity – a material property – in the TM? It is calculated as varying between 0.18 Pa.s [apical] to 0.33 Pa.s [basal].
I assume that the TM material cannot be spread better than peanut butter. However peanut butter has a shear viscosity between 150 to 250 Pa.s. That is 757 to 833 times higher than the TM tissue shear viscosity.
It simply doesn’t fit at all.

On this fundamental mistake the in the paper given TM movement interpretations cannot be considered as ultimate proof for the existence of traveling waves with extreme small wavelengths in the cochlea.

Moreover, if we consider the TM – similar as the basilar membrane – to be a strip of material in which a systematic distribution of resonators exists due to place dependent differences in stiffness and size [like the authors also mention in their paper], stimulating a strip of the TM tissue will create a wave phenomenon, that can be interpreted as a complete ‘convolution of a wave’ only on the condition that the evoking frequency corresponds with the resonance frequency of a local resonator present in the TM specimen.
However that isn’t an energy transferring traveling wave at all. It is exactly the phase wave I have described. As long as the stimulus frequency is lower than the lowest resonance frequency in the TM specimen the phase retardation remains less than $^{\pi}/_{2}$. Once the stimulus frequency is higher than the resonance frequency of one of the local resonators, the phase retardation in the TM vibrations will be distributed between 0 near the base [area of highest resonator frequencies] via $^{\pi}/_{2}$ at resonance location to $\pi$ near the apex [area of lowest resonator frequencies].
The plot of such a characteristic of phase vs. stimulus frequency will show a striking resemblance with Fig. 2 C in the paper of Ghaffari et al.

4. There is also one peculiar phenomenon in the video that shows us 6.5 complete convolutions passing by. However next to the ‘wave’ on the TM surface a number of ‘UFO’s’ do appear and disappear with exactly the same periodicity as the ‘running wave’ possesses.

The only conclusion I can draw out of this is that the authors let us believe that there is running a traveling wave by repeating at least 6 times exactly the same scene.
One such scene separately observed can also lead to the conclusion that it concerns here a phase dependent motion distribution.

Can we call this an undisputable example of micro dimensioned traveling waves? I don’t think so.
The same experimental data given in the paper are completely applicable for explaining the observed wave character with the existence of a phase wave instead of a sound energy transporting traveling wave.

Prof. Hemmert’s last reaction:
** My specific answer to Randy:
There is good news: to solve your question, you don't have to speculate, you can use data and experiments. Especially, I recommend you to install one of the models, which generate nerve-action potentials (e.g. from the Carney or Meddis groups). They are based on measurements (of course they will never be perfect, but for your question, they will do the trick). You can use them to simulate your idea with sufficient precision. If you have done that, you might conclude that your question can not be resolved by the cochlea alone, it highly depends on the processing of the grey material coming afterwards. **

The calculation I did of the movement of perilymph fluid in the cochlear duct has absolutely nothing to do with speculations. It’s just a straight forward exact analytical solution of the non-stationary Bernoulli equation for which all the conditions and parameters – obtained from data out of cochlear mechanics literature – are fulfilled within that cochlear duct.
And the result is that inside the perilymph part of the cochlear duct the in physics terms named ‘periodic potential flow’ is present. This presence evokes everywhere in the perilymph duct a decreasing pressure variation proportional to the kinetic energy in the local fluid motion. And that kinetic energy is proportional to the incoming sound energy.

Therefore a substantial number of the existing stimulus phenomena, which can be observed in the cochlea, are based on hydrodynamic principles, however in a different way than is commonly hypothesized. And what that means for dichotic hearing phenomena to my opinion you can read in my next answer promised to Randy Randhawa.

Kind regards

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