

The Cryosphere Discuss., referee comment RC1
<https://doi.org/10.5194/tc-2021-7-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on tc-2021-7

Valerie Maupin (Referee)

Referee comment on "Acoustic velocity measurements for detecting the crystal orientation fabrics of a temperate ice core" by Sebastian Hellmann et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-7-RC1>, 2021

The article compares two methodologies to measure the seismic anisotropy in an ice core. Considering the relation between ice dynamics and anisotropy, and the fact that seismic investigations provide a non-invasive methodology for mapping glacier properties, this is of course an interesting and important contribution. It shows also intriguing results.

The article is very well written. The methodology is very well described and the results are well presented. The sonic records shown in Figure 1 are very nice and should ensure very good quality data. My only major comment concerns the interpretation of the difference between the results of the two methods. I am not completely convinced by the explanation that the limited number of grains in the ultrasonic case is the reason for the difference, and, from the figures, I think that the difference is larger than the text gives an impression of.

Figure 4 is central to compare the results of the two methods. The ultrasonic tests show a rather wide band of measurements, which is absolutely normal, and they have a clear trend, but I notice that the COF do not even fall within this band for some azimuths, at 2 and 65m depth. The authors argue that the discrepancy between the two methodologies come from a less representative sampling of the ultrasonic measurements. In favor of this, I notice that the ultrasonic measurements show a higher amplitude of anisotropy, which would fit with the fact that they represent one orientation, rather than the averaging done by COF, but they also claim that Figure 7, where more measurements were done, confirm their hypothesis. I agree that the amplitudes match better in the b) and c) plots than in the original a) plot, but the dominant shapes and positions of the maxima of the blue and red curves do not change from plot to plot. What rather strikes me is actually the consistency of the red curves between the three plots in Figure 7. That would suggest to me that the number of grains is not the main factor creating the difference, and that the difference is a systematic difference in how the two methodologies view the anisotropy. The authors show the dimension of the Fresnel zone in Figure 5. The Fresnel zone is actually a volume that also extends in the vertical direction. Waves propagating in this volume propagate in slightly different directions. I would therefore assume that the velocity seen by the ultrasonic tests is not exactly the one calculated in the source-station direction, but an average around this direction. I notice in Figure 2 that

maximum velocities (considering all dips) often occur in an azimuth not very different from the azimuth of the maximum of the ultrasonic measurements. In particular at 45m depth, the max is at about 135degrees, in the same azimuth as the max velocity for the ultrasonic measurements. Of course the dip with respect to the horizontal plane is not small. The anisotropy is rather small here. I do not expect this would distort the shape of the Fresnel zone or give a very different group and phase velocity direction. I do not claim contributions from off-plane directions is a good explanation, but I think it would be worth exploring it a bit in the text, as an alternative.

Figure 4 is a very central figure. The data are actually duplicated from a 0-180 to a 0-360 degrees range. I think this might increase artificially the impression of fit and should be avoided. It would be interesting to have the vertical velocity in the same figure, as an extra small column to the right for example, in order to exploit more the vertical direction velocity in the interpretation.

Minor comments:

line 14: "concise": should be "consistent"?

Figure 2 is cited before Figure 1, as far I can see, and you should normally exchange the figure numbers. As Figure 1 is a good overall summary of your set-up, find a way to cite it before?

line 44-45: rephrase. "since..." does not really make sense with beginning of sentence.

line 59: benchmark to what?

line 101: move sentence to line 128, as this gives the impression you won't give any details, but you give them afterwards, and they are necessary.

lines 129-130: I do not understand what you are saying here. Your step 4 is a Voigt average (linear average of elastic tensors); when you say here "seismic velocities", do you mean you take the Voigt average (and Reuss and Hill) to calculate the isotropic mean velocities?

line 133: at this point you have not said you measure at -5deg. You have said you have frozen the core to -30deg.

line 163: unclear sentence. The small wavelength does not favor that the individual measurements are a good integrated representation of the whole sample. Do you want to point out here that the wavelength is smaller than the grain size? Anyway, it is only the Fresnel zone dimension that matters to see if the wavefield sees one grain at a time along its propagation, not the wavelength.

line 186: it seems there are many dark points within the clusters. It is not clear to me why they have been removed.

line 199: would be good here to have the pure ice value for comparison.

line 218: indicate which uncertainty you have on this diameter, and if variation in diameter correlates in any way with the anisotropy.

line 228: The coincidence is not as good as stated by this sentence. The maxima for the COF and measured coincide only at depths 2 and 22m. For the three other depths, they do not coincide at all. At 45m, the maximum for the measured coincides with a minimum in COF.

line 230: One curve does not look like a smooth version of the other; I do not think you can blame the smoothness for the difference in amplitude.

line 230: This section is about the horizontal velocities, that do not increase with depth. You might remove this sentence.

line 247: you say that the air bubbles not associated with grain boundaries are spherical, but what about the grain boundary bubbles?

line 288: Is figure 5 representative of the number of grains? The mean grain size in Figure 6 does not really fit with the fact that a section of an ice core would have just a few grains. Is it such that grains in a given orientation cluster also tend to cluster in space? That would strengthen your theory if adjacent grains have the same orientation. Would it be relevant to calculate the velocity from the COF of one cluster only and compare with ultrasonic?