Dear Valerie Maupin,

Thank you very much for your valuable comments and suggestions. In the following, we include your comments in italic followed by our answers in normal font.

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 intriging 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.

We have considered your point on off-plane reflections. Indeed, this is an important point that required clarification. However, the ultrasonic measurements do not really suffer from off-plane effects. Other than the COF-analysis, the ultrasonic experiments allow a full three-dimensional measurement of a particular amount of ice. They are more suitable to consider the shape of the branched, large grains. We already tried to discuss this in our discussion section, but did not state this so explicitly. In our revised version, we have added that the Fresnel Zone is a volume and that in particularly the very branched and interlocked grains in the temperate ice would lead to a significant difference between the velocity derived from 300 µm thin ice core samples and a cm^3-large ultrasonic volume.

However, even though the Fresnel Zone is a volume of a few cm^3, the total number of grains is rather limited. We are still convinced that the large grains and the associated clustering effect due to recrystallization (SIBM-N) are the major driver for the differences.

Nevertheless, we also agree that the COF-derived profiles are lacking some information from out-of-plane effects, but this is again an issue driven by grain size and grain clustering. In polar cores with grains of a few mm, we expect that the effect is much less important when assuming a statistically uniform distribution of the different clusters in the samples. We plan to test this hypothesis on polar cores in future projects.

In conjunction with your comment for L288, we also added some further details about the “parent grain” pattern observed in the ice core. Large grains with a certain orientation are often surrounded by many smaller grains with a similar orientation. This is an observation within the context of recrystallization mechanisms in the temperate ice → strain-induced boundary migration with nucleation of new grains (SIBM-N, see our first paper Hellmann et al., 2021).

Here are our answers to your line-specific comments:

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.

We adjusted the Figure (0-180° range) and added the vertical measurements to Fig. 4 (and Fig. 6). For clarifying that these measurements contain a periodicity, we have added the first/last measurement to the end/beginning of the profile.

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

Indeed, this should be consistent, changed.

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?

We added a reference to Fig 1c before referencing this Fig 2a-g here.

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

We rephrased this sentence as follows:
These methods investigate the elastic parameters of the ice. Since elastic parameters and COF are directly related, the methods can also be employed for COF analyses.

line 59: benchmark to what?

We replaced this term by “relevant measurement parameter”.

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.

We agree that this sentence may confuse the reader. Therefore, we combined parts of this sentence with text from line 128 and formulated a new sentence in line 129: “The calculations for the polycrystalline tensor and acoustic velocities are described in more detail in Maurel et al. (2015) and Kerch et al. (2018)."

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?

The seismic velocities derived from the elasticity tensor vary around a mean value, called Voigt bound, and those seismic velocities calculated from the compliance (stiffness) tensor provide variations around the lower Reuss bound. These two ways of calculating seismic velocities from the two reciprocal tensors provide an upper and lower bound. Both ways are possible and we use their results to get a velocity range for our calculated values. The mean isotropic values can be regarded as baseline for each calculation. The anisotropic values vary around this baseline.

We rephrased this sentence as follows:
The seismic velocities can be calculated from the elasticity tensor or the inverse compliance tensor. Both approaches provide velocity profiles oscillating around an upper (Voigt bound) and lower (Reuss bound) mean velocity (Hill, 1952). We calculated the seismic velocities from both tensors to obtain these upper and lower bounds of the potential velocity range and further derived the velocity profile from the Hill tensor (the mean of elasticity and compliance tensor).

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

This is true. We rephrased the sentence and added in parentheses that the measured temperature is -5°C as described below. However, at this stage the exact temperatures are not necessary and in addition, they vary between ultrasonic and in-situ experiments.

line 163: unclear sentence. The small wavelength does not favour 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 wave field sees one grain at a time along its propagation, not the wavelength.

We agree here, although we also want to point out that the size of the Fresnel Zone also depends on the wavelength (e.g. Lüth S., Buske S., Giese R. and Goertz A. 2005. Fresnel volume migration of multicomponent data. *Geophysics* 70, S121–S129).
However, in this part of the paper, we wanted to point out that we need to have a reasonably small wavelength to avoid effects as a result of wavelength vs sample size, e.g. if the wavelength is too large, stationary waves or waves that cause vibrations within the ice samples may be induced by the transducers. Those waves do not contain information about the COF. Furthermore, as we want to compare these ultrasonic measurements with in-situ data at a later stage, we also wanted to have a similar order of magnitude for the ratio core size – wavelength as in the in-situ seismic data (wavelength of around 3.8 m vs 100 m glacier thickness → factor of ~20, too).

We rephrased the sentence for clarification: "Thus, the wavelength is small enough to measure an integrated seismic velocity. This velocity can be regarded as the integrated velocity of the individual grain velocities. Much larger wavelengths may introduce geometric issues such as stationary waves, which are not representative for acoustic waves travelling through the glacier and thus would later inhibit a comparison with in-situ data."

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

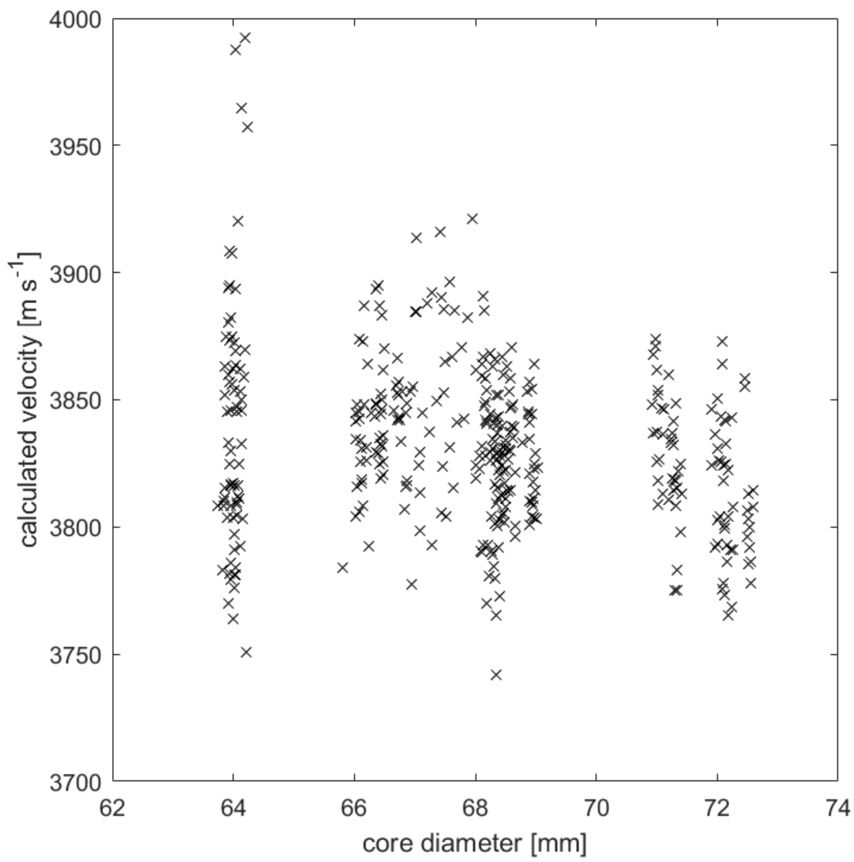
Indeed, we excluded all grains below 0.5 mm^2 (< 1250 pixels). Those grains usually occur in fissures and as patches within the ice core. However, we reviewed the effect of these small grains and realised that they only minimally affect the calculated velocities (because the grain size is used as weighting factor for the sum over the velocities of the individual grains), visible changes only appear at 22 m and 45 m depth. Therefore, we also include these grains to avoid confusion. We changed Fig 2, 3, 4 and 6 respectively.
During this review, we realised that we used a wrong input file for the velocity calculations in 33 m and corrected this issue.

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

The given values in this section (incl. Table 1, Fig. 2) are the pure ice values. We clarified this in the captions of Fig 2 and Table 1. We applied the air correction for the first time, when comparing the data with the ultrasonic measurements (Fig. 3).

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

The diameter was determined manually for each individual measurement. In addition, the Figure below indicates that there is no correlation between the calculated velocity and the measured ice core diameter.



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.

Together with the new Fig. 4, this discrepancy becomes more obvious and we extended this sentence as follows (to correct for this imprecision):
"All 5 samples show a set of 2 maxima surrounded by 4 minima and 2 local side-maxima. For the samples at 2, 22 and 65 m the positions of the maxima for measured and COF-derived profiles coincide within a range of a few degrees of azimuth (≤15°, Figs. 4a, b, e). At 33 m, there exist a significantly larger azimuthal shift (30°, Fig. 4c) and for the sample at 45 m maxima of one profile coincide with a minimum of the other (Fig. 4d)."

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.

We rephrased the respective sentence: "The COF-derived profiles are in general rather levelled with smaller differences between the minima and maxima."
We do not want to say that one profile is a smoothed version of the other, but rather state that the COF-derived profiles are in general levelled profiles with smaller variations between max. and min. This observation is important for our discussion.

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

We have removed this sentence.

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

Indeed, small grain boundary bubbles seem to be elongated as observed in many ice cores before. However, the larger bubbles, which are more frequent, seem to be hardly influenced by the grain boundaries. They are transecting and still appear round (see typical example in Hellmann et al., 2021, their Fig. 10). The reason is most probably the high temperature, that enhances the dynamics of the cycle of sublimation on large radius segments versus condensation on small radius segments, which takes place inside all bubbles and keeps them round as good as the dynamics allow.
We have added a sub-sentence in parentheses to clarify that bubbles on grain boundaries are influenced by the grain boundary processes and that other processes complicate an interpretation: "(and therefore not pinned to and affected by the boundary pathways)".

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?

The example in Fig. 5 is representative. The large number of grains arise from combining 9-12 of those thin sections (4 horizontal, 8 vertical cuts). In some of those thin sections, fracture traces with many small grains (especially at 22 and 45 m) were observed. Such fractures and patches of smaller grains further increase the total number of grains.

Due to the existing horizontal and two perpendicular vertical sections in south-north and east-west orientation (in total 3 perpendicular thin section from adjacent ice core samples, see Hellmann et al., 2021, their Fig. 4), we can actually see that the large grains are surrounded by smaller grains with a very similar orientation. Furthermore, the large grains are interlocked and branched and the ultrasonic measurement might be more sensitive to detect such an irregularly formed grain than a state-of-the art fabric analysis.

We also analysed the contribution of individual clusters and found that the measured velocity profiles can be explained with a combination of some of the four clusters. Usually, we observe that two clusters dominate, and the others seem to contribute less to the measured velocity compared to their occurrence (i.e. the relative grain area) in the thin sections. However, this is a rather qualitative investigation (no 1:1 comparison as the COF and ultrasonic experiments were not obtained on exactly the same ice). We do not have a profound physical explanation and assume that this is rather a statistical effect (several combinations of the clusters lead to similarly well-fitting results). Nevertheless, we added a sentence about this investigation.