REVIEW

This is an interesting paper where the authors show the presence of radial anisotropy in the firn from the analysis of ambient noise measurements. They obtained Rayleigh- and Love-wave dispersion curves from the computed cross correlations, picked the fundamental and higher modes and conducted 1D MCMC inversions. The obtained S-wave velocity curves show that Vsh is always higher than Vsv, both in the along flow and across flow directions. The authors conclude that radial anisotropy is present, and propose some explanation for that. The results are similar to what we found on the Whillans Ice Stream (WAIS), although we used active seismic data and other analytical methods (Picotti et al., 2017). Moreover, in our case we observed that the medium can be considered VTI, with the major differences in SH- and SV-wave velocities at the firn bottom, which we attempted to reproduce using Backus theory.

Overall, I find this work original, but the authors should be more critic about the approximations adopted and the uncertainties associated to the final results.

After reading the manuscript, I have the following major concerns:

1- Lines 125-129 : This is an interesting hypothesis, but not enough supported. Could you please add some references?

2- Lines 160-161 : "To avoid anomalous measurements, we further remove the frequencies for which the dispersion measurements look anomalous. The remaining values selected for the inversion are presented as orange curves". Could you please better explain the removal criteria?

3- The orange curves in Figures 4 and 5 represent the smoothed values used as the input for MCMC inversion. I noticed that in some cases these curves are quite different than the original picking of the maximum group velocities. Why? How much does this difference affect the final results? I have the impression that this mismatch is important.

4- Could you please explain why you used group velocities instead phase velocities (Figures 4 and 5)?

5- To my opinion data parametrization for MCTC could be better explained. Table 1 shows 6 layers and S-wave velocities ranging between 0.1 and 1.9 km/s for all layers. However, the authors do not justify these choices. In particular, why so wide ranges for the Vs at all depths? Have the authors considered to better constrain the S-wave velocities versus depth? For example, by using the density profile and the empirical relationship from Diez et al. (2016). Then, it is unclear whether the densities (Figure 6) were actually used in the inversion procedure. If density effects were ignored, how much this approximation affects the final uncertainties?

6- P waves were never mentioned in the article, which suggests that the contribution of P waves was likely ignored in the inversion. Again, how much this approximation affects the final uncertainties? Although Rayleigh waves weakly depend on P waves, I think that the authors should relate uncertainties of the inverted Vsv wave velocities also to the P to S wave velocity ratio. However, P waves can be easily modeled by using, for example, the density profile and the empirical relationship from Kohnen (1972), and can be included in the inversion.

7- The computed differences between Vsv and Vsh are small if compared to uncertainties. In the along flow direction, this difference is below the experimental error, with the error bars overlapping, i.e., the two curves are almost identical. Have the above approximations (i.e., ignoring density and P-wave effects) already considered in the estimation of uncertainties? The estimated weak anisotropy is already at the limit of detection threshold. My main concern is that even larger uncertainties may lead to indistinguishable Vsv and Vsh curves, and to a negligible anisotropy.

8- The error bars are larger between 20 and 60 m, and thinner above and below this depth interval. Moreover, errors are similar at surface and at the firn bottom. Since the deeper parts of the firn column are excited by the lowest frequencies, which have the largest uncertainties (the gray bands in Figures 4 and 5 are very wide below 5 Hz), one expects errors increasing with depth. Could the authors comment on that?

9- Following points 7 and 8, the sensitivities shown in Figure A1 looks quite different for Rayleigh and Love waves. Thus, the inversion reliability is different for the two wave types and changes vs. depth. Could you comment on this? The authors should be more specific about the depth interval in which the estimation of the anisotropy is actually reliable.

10- Lines 207-208 : "The terms of that covariance matrix are adjusted along the inversion to stabilize the acceptance ratio around 25%". Could you please expand this sentence please?

Minor comments

1 – Line 138: Signal-to-Noise ratio (SNR).

2 - Line 205 : (Herrmann, 2013) is redundant.