Review of: Seismic wave propagation in anisotropic ice: Part II. Effects of crystal anisotropy in geophysical data
Author(s): A. Diez et al.
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General Comments
This paper follows the detailed background and methodology outlined in Part 1 with application to a comprehensive and possibly unique data set. Seismic VSP and reflection data, along with radar data, are compared with measured anisotropy from core analysis. The authors assign one of three anisotropic fabrics with varying opening angles to measured crystal alignment eigenvalues derived from core analysis. From this they use a widely accepted averaging technique to derive the polycrystal elasticity tensor for each fabric from previously measured or calculated elasticity tensors of ice. Seismic velocities are then derived and compared to observations in terms of absolute velocity, velocity trend and reflectivity. Radar data provide an additional control and the authors demonstrate how an understanding of the local anisotropy allows the discrimination of isochrones from COF fabric reflections.

The manuscript is well-structured, presenting a detailed analysis and combination of the available data. The shortcomings of the data are clearly outlined although not always fully presented to the reader. The combination of borehole, radar and multiple seismic data sets has allowed the authors to highlight the strengths and shortcomings of the use of seismic data to investigate the anisotropy of ice.

Combined with Part 1 this paper presents a thorough study of the effects of COF on seismic data and the problems linking the different resolutions inherent in combing seismic, radar and borehole data. The pair of papers will likely become a starting point for future work by other practitioners. This is a thorough and informative piece of work and I suggest the manuscript is accepted for publication once these comments have been addressed.

Specific Comments
At several points in the paper the straight-ray assumption is applied rather than deriving true curved-ray paths which result from the high velocity gradient present in the upper 80 m firn layer. The effects of this assumption may be negligible in the context of the uncertainties presented here. However, the authors need to highlight the validity of this assumption or present modelling results demonstrating its validity.

P4403-L12 / P4404-L22:
Implicit in the method presented in Part 1 is the discrete classification of fabric types. This is discussed in Part 1. This classification is applied in Section 3 of this manuscript and presented in Figure 2b. This methodology introduces significant discontinuities in seismic velocities with depth which are essentially an artefact. Although at P4404-L22 it is described as “partly” causing calculated velocity changes, it is by far the - At 450 m depth there is an obvious change in the measured eigenvalues, at 850 m there is no such obvious change but the discretization of fabric type introduces a significant step in calculated velocities. Although this is implicit in the method used here I would like to see a paragraph discussing the significance of these assumptions and discussing how they may be surmounted in future work.

P4402-L16 / P4405-L2 / P4406-L21:
Details of the surface geophone line are presented and introduced as a method of verification of

source consistency. However, no data are presented or included in the later discussion about source signature variation.

P4407-L12 and P4427-Fig. 5:
These data present a problem for the reader as there is an obvious oscillatory nature to the observations. What is the cause of this? In Figure 5 it may be pertinent to present the same data as a scatter plot with points not lines, distinguishing the different picks and also the different survey days. Is the oscillation a result of two different days recording? Methodology? Choice of pick phase? If adjacent measurements from one combination of phase and survey oscillate so much can they be trusted? Should one data set be thrown out? The authors have obviously determined that all the data are valid and should be included but the reader needs the evidence for this to be presented. This ties in to the previous point about presenting the surface 3C data. The booster data are obviously problematic. Could they be disregarded? Are they only problematic on the VSP data?

Technical comments
These are annotated on the attached pdf. The entire manuscript needs careful editing for grammar and spelling mistakes.