Answers to the Reviews of A. Brisbourne and the Anonymous Referee #2
Seismic wave propagation in anisotropic ice – Part 2: Effects of crystal anisotropy in geophysical data

First, we thank both reviewers for their thoughtful comments, which helped us to clarify the manuscript in various aspects. A point-by-point account for the main issues follows below.

<u>General Comments</u>

<u>A. Brisbourne</u>
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.
- Our first measurement point is at a depth of 100 m, the second at a depth of 140 m and the shot position is 30 m away from the borehole. Under the present density gradient (known from the ice core) we calculate a travelway of 104,54 m for a curved ray path and 104,4 m for the straight ray assumption at 100 m depth. At 140 m depth we get 143,28 m for the curved ray path and 143,18 m for the straight ray assumption. Hence, in 100 m depth the difference is smaller than 15 cm; in 140 m depth it is 10 cm. Additionally, the error reduces as we analyse the intervall velocities. To calculate the velocity we use the difference of the travel path (straight ray 143,18-104,4), not the depth difference of 40 m. The difference in travel path, by neglecting the curved ray is thus only 4 cm at a depth between 100 m and 140 m. This error will decrease with increasing depth. This depth error is within the accuracy of the depth of the borehole geophone, as well as the picking uncertainties. We included this explanation in the text in Chapter 4 'Vertical seismic profiling'.

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.
- In Part I the jumps introduced by this classification are discussed in more detail compared to the manuscript version. We additional discuss this problem in more detail at the end of Chapter 3. We changed the sentence in P4404-L22 to make clear that the strong velocity jumps are caused by the classification.

P4402-L16 / P4405-L2 / P4406-L21:
Details of the surface geophone line are presented and introduced as a method of verification source consistency. However, no data are presented or included in the later discussion about source signature variation.

- We included a figure showing the surface seismic data.

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
- It is true that the data oscillate. More so for the booster data, than for the detonation cord data. We included a figure showing the surface data to illustrate the variations in these recordings. Further we included a Subfigure in Figure 5 showing the picked velocities from the booster and detonation cord survey separately as dots and averaged over the different picks as lines. Also the booster data oscillates significantly stronger, we like to keep that data for our comprehensive analysis. Using the 200 m moving average the booster and detonation cord data oscillate around the same mean value.

<u>Anonymous Referee:</u>
Some statements made in the paper are not as strongly supported by the observations presented as they should be. For instance, the conclusion that changes in COF are laterally discontinuous is an example of a common geophysical logical fallacy - the absence of evidence is not evidence of absence (just because we don't image it, doesn't mean it isn't there.) The COF structures imaged in the radar data are close to the noise floor. In comparison, the radar reflectivity due to conductivity changes are much higher than the noise floor. These factors alone are enough to make the COF reflection appear less continuous.
- Our radar data clearly show that the reflector from a change in COF around 2040 m at the EPICA drill site has a different signature along the profile than for instance the event related to the conductivity signal from the Toba eruption at around 1700 m. Both events are considerably above the noise floor, at around -91 dBm compared to the noise at -92.8 dBm (see Eisen et al., 2007). Whereas the latter event is continuous and parallel to adjacent internal radar layers, the former (COF) has a stronger variation in terms of vertical position. In fact, one can follow the reflector up to a distance of 2 km where it then is crossed by an additional event from above. At 3 km from the drill site it starts to buckle upwards and disappears, at km 5.5 it follows a rather horizontal track, whereas the other layers show a considerable upward slope. However, these can only be displayed in high resolution in the original data, best on a screen. Rather than having another figure displaying these features, we would like to leave it with a discussion in the text.

Similarly, statements on the possible dependence of velocity on frequency, or grain size are not well explored. Ice is currently considered essentially non-dispersive at the frequencies of interest here (e.g. Ice Physics, Hobbs, 1974). Determining a dispersive nature warrants more than a comparison of the results presented here and a single other borehole sonic-logging experiment.
- Studies determining the frequency dependency of seismic wave velocity in ice where,

e.g., done by Dantl (1968). However, he investigated the frequency dependency in a range of 5 MHz to 140 MHz. Using the results of Gusmeroli, we compare velocities in the region of kHz to those in the region of Hz. For us a dependency of the velocity on frequency is the most obvious reason for the difference we observe, based on the comparision of our results to those of Gusmeroli.

The correlation with grain size should be quickly ascribe as non-causal and does not need to be repeated in the conclusions. The correspondence of changes in grain size with changes in COF is the likely cause of any misleading correlation (as noted by the authors.) (There is, however, a second-order effect due to the dependence of ice viscosity on grain size (Goldsby and Kohlstedt, JGR, 2001) and the influence this may have on COF evolution.)
- We changed the sentences in the conclusion to: Further, based on the derived reliable depth conversion for the seismic data and the comparison to ice core data, we conclude that observed englacial reflections in the seismic data are caused by short-scale changes in COF and are apparently not directly linked to grain size variations.

The velocity profile derived from COF shows a large amount of variability. The authors provide a plausible explanation for this due to limitations in the COF data set, and changes imposed by the methodology. The manuscript continues to state that the fit is good, or consistent. I think they should be more exact with their language. The fit is good in the upper 1800 m once the optimal elasticity tensor has been determined, and only fair in the lower portion of the core. The filtered (boxcar) COF-derived velocity provides a better fit throughout the core.
- We changed our wording throughout the manuscript to be more precise and talk of the averaged COF data, to make a clear distinction between the COF data as we picked them and the averaged COF data using the running mean.

Line Specific Comments:

A. Brisbourne:
Most of the line specific comments in the tech document were included. We don't list all the comments that we included in the text regarding grammar and spelling, only the major comments or where we have a different opinion.

P 4400, L 18: Awkward sentence
- We changed the sentence to: the temperature in the borehole was measured in 2005 (Wilhelms et al., 2007); temperature logging was repeated in January 2011.

P 4401, L 24: Re-word for clarity
- We split up the sentence and changed it to: We carried out explosive and vibroseis surveys. For the explosive surveys we used booster as well as denotation cord charges.

P 4402, L 16: be precise; what spread?
- We included information on the spread length of the surface geophone line and rephrased the sentence.

P 4404, L 3: These classifications and their boundaries should be annotated on Fig. 2
- We included different gray areas. Thus, the regions of different classification should be obvious directly.

P 4404, L 22: partly? In general it seems that the discretization introduces significant discontinuities (see major comments)
- See answer under major comments.

P 4406, L 20: What is observed on the surface 3C data? Why are these not presented or discussed here?
- We included a figure showing the vertical component of the first geophone of the surface 3C data, see major comments.

P 4406, L 29: Is this the original source of -2.3C? Kohnen (1974)?
- We changed the reference to Kohnen (1974).

P 4407, L 14: 4800m/s in ice is so high it is hard to envisage how "incoherent excitation" could cause this, more likely observational errors?
- We included a sentence about picking errors here. Due to the small depth intervals, small variations in picking can already cause higher variations in the calculated velocities.

P 4407, L 16: This sentence needs re-writing for clarification, remove brackets.
- We changed the sentence to: In this region cone fabric with large opening angles exist up to 450 m depth, below girdle structures can be observed (Fig. 2b). The VSP interval velocities show an increase to velocities ≥4020 m/s at 1800 m depth.

P 4409, L 2: Shouldn't using an RMS normalise this and therefore discount the difference in sample size?
- When calculating the RMS error, this is calculated over the complete depth. When the values over 2/3 of this depth fit well and only 1/3 doesn't the RMS error will be smaller than in the case were only 1/3 of the data values fit well.

P 4409, L15: Are these insignificant? The gradients are very high in the firn
- See answer to the major comments.

P 4414, L 28: Poorly constructed sentence
- We changed the sentence to: Hence, we are able to evaluate the lateral coherency of these radar horizons in comparison to those caused by changes in conductivity.

P4415, L 13: Can this be quantified as a lot of this comes from how the plot is scaled
- In principle it could be quantified by determining a depth-dependent window over which lateral coherence – taking into account layer slopes – is calculated. Such approaches are currently followed by different groups for the Greenland radar data. However, we can't see the benefit of quantifying this variability. We consider the qualitative description sufficient for the line of argumentation followed here.

P 4422, Table 1: flag which are calculated or measured by author's name
- As the only calculated elasticity tensor is that by Penny, we did include this information in the caption of this table.

Figure 2: somewhere on this diagram it would be good to annotate with the fabric assigned to each depth interval, as discussed in the text
- We included gray areas so it is easier to spot which fabric is assigned to the different

areas.

Uncertaintes? Gammon tensor?
- Compared to the difference of the elastic constants measured by the other authors the uncertainties given by Gammon are very small. That's why we concentrate on discussing the differences between the different elasticity tensors and not the uncertainties of one elasticity tensor.

Figure 4: Why not show the 3C data here too - perhaps each shot into the furthest geophone?
- We included a Figure showing the vertical component of the geophone closest to the shot position.

Figure 5: repeat plot with points not lines? does this discriminate methods? add minor tick labels as referred to in text
- We included a subfigure showing the picked velocities. Hence, it is possible to see the higher oscillatory nature of the booster data compared to that of the det. cord. data. As the Figure already includes a large amount of lines and dots we did not include extra tick labels. It should nevertheless be possible to identify the depth points we discuss.

<u>Anonymous Referee #2</u>

The abstract needs a sentence near the start outlining the comparison made between radar and seismic reflectivity as this is a significant part of the paper. (Or move and reword/clarify the sentence that begins on line 10. 'With this validation ...')
- We reworded the sentence. However we do not want to move it close to the beginning of the abstract as it destroys the logical order of the abstract that follows the structure of the paper.

Section 2.2 The first paragraph should be clear about what seismic data are presented in this study. (There is no vibroseis data presented here.)
- We included a sentence saying which of these data will be used here for the following analysis.

Sec 4. It is not surprising that repeat shooting at the same location leads to changes in the source signature. The type of and size of adjustment (static) required should be detailed. (Note that no difference is visible in shot 44 in 4a, enlarge figure?).
- We included a Figure showing the vertical component of the surface 3C data from the geophone closest to the shot, to show the variations and explain them in more detail.

Sec 4. pp 4406, L21. An increase in frequency content with depth is somewhat surprising.
- Yes, but that is what we observe. Finding an explanation for this behavior wants further investigation. As it is so uncommon we think that the repeated shooting at the same location at the surface with a change thereof contributes to the feature, as stated in the text. The first shots were the deepest in the borehole. An undisturbed surface might have led to better coupling and thus, higher frequencies for the first shots compared to the later once. As this is the first high-resolution VSP in Antarctica, it is obvious that there is room for optimization for future experiments.

pp 4406, L25. Change ’damping’ to ’attenuation’ (and elsewhere).
- We change damping to attenuation.

Sec 4.1 pp4407, L13-14 This excitation is not apparent in the figure (zoom?)
- We included a figure showing the surface 3C data. Here the differences are visible.

L15 The fit between the ’filtered’ EDML and VSP interval velocities is good.
- We changed the sentence to: The vertical EDML and averaged VSP interval velocities show good agreement... . We also checked the rest of the manuscript to make the distinction between the VSP interval velocities and the averaged VSP interval velocities.

Change Bennett 1988 to 1968.
- This was mixed up in the review process we changed it back to Bennett 1968.

Sec 5.2 PP4413 L8. ’both’ three are given above.
-We changed it to: The reflection coefficient for the interfaces of event B and C are two orders of magnitude smaller than those of the ice-bed transition.

pp4413 L20. ’20 m’ 50 m sampling, where does the 20 m come from?
- Some regions were sampled in a denser interval. We changed this in Ch 2. 1 'Ice core and radar data' and state that most samples are in a 50 m interval, but some are sampled more densely, in 10 m intervals.

pp4414 L20. See Gow and Meese ’07 J. Glac and other physical properties studies from ice cores for grain-size and COF changes at the base of ice cores. (Migration recrystallization.).
- We included this reference.

Figures. Fig 4. Show zooms of important regions.
- We included the additional Figure showing the surface 3C data. Here the important regions are visible.

Fig. 7. caption needs work, shift letters to the start of relevant sentences. Shift sentence ’The seismic trace...’ to text along with other detail that does not describe the figure.
- We shifted the letters to the start of the corresponding sentences and shifted the sentences 'The seismic trace ...' as well as 'The calculation of depth ...' to the text.

Fig. 8. Label reflectors.
- We included the information of the labels of Fig. 7 (now Fig. 8) to easier connect the same reflections of these two Figures.