The author responses are given in italics below individual reviewer comments.

General comments:

The paper presents a comparison of seismic Vertical Seismic Profile (VSP) data to ultrasonic lab measurements and a model of Crystal Preferred Orientations (CPO). The work is undoubtably a useful and novel contribution to the field. The introduction, methods and results are well described, albeit in a perhaps unconventional structure (methods and results introduced together for each technique). The discussion lacks the detail I expected given the comprehensiveness of the earlier sections of the paper. Indeed, it reads more like an extension of the results rather than a discussion. I'd recommend that the authors dive into a little more detail for why their results show what they show. Similarly, for the conclusions, I'd really like to see a few sentences on how these results have changed the field. They certainly are interesting and rigorous results, so improving the discussion and conclusions shouldn't require too much work. No more analysis is required, just some additional thought on why the observations and models agree/don't agree. As such, I am on the fence between suggesting minor or major revisions. Regardless of whether these changes constitute minor or major revisions, it shouldn't take much time/work to improve this work. With the suggested changes, it will make a useful contribution to the field and so I would then be happy to recommend it for swift publication.

More general comments/further details on the above:

Would be nice somewhere to state why horizontal cluster CPO is assumed in this study (valid assumption I think, but needs justifying).

- *Horizontal cluser CPO observations were made in core sections by EBSD (Thomas, 2021). This is now explicitly stated in Section 4.*

Discussion is a little lacking in "discussion". Much of it reads more like results. I'd like to see more reasoning for why the results show what they show than is currently included. The results are sound, so the discussion shouldn't be hard to develop further.

- *The Discussion was slightly restructured. Some elements have been included that were formerly in the Results section, e.g. Figures 11 and 12 since they are better suited to assist discussion. Please see in-text changes as well.*

The conclusions would benefit from a final sentence or two summarising the impact on the field and the new insights that their analysis could provide at other glaciers and ice streams. The work is a valuable contribution to the field, so I feel the authors should really sell that briefly in the conclusions.

- *Added concluding sentences commenting on implications of this study for the design of seismic surveys targeting CPO reconstruction.*

Specific comments:

L12: "good" – subjective. Consider removing.

- *Removed "good"*

L17: Large strain compared to what? Comparative language should be avoided unless referring to something else.

- *Removed "large"*

L15-29: Very nice opening paragraph!

- *Thank you!*

Figure 1d: Make the labelling of P and S waves clearer (maybe use different colours?). It is obvious to me as a seismologist, but would not necessarily be obvious to other glaciologists.

- *P- and S-wave are now marked in different colours in the figure.*

L53: Is the borehole seismometer three-component? I think it is from what is written, but can you state this explicitly?

- *Added "three-component" in text.*

L65: Is the sampling interval 1 / the sampling rate? Maybe better to state that you sampled with 8000 Hz rather than the rather odd unit of 0.125 ms.

- *Changed text to give sampling rate 8000 Hz.*

L66: Is the hard ice surface an ice lense from surface melting or the actual ice column surface? Important to clarify whether you have a firn layer at the site.

- *Now stated in introduction that site has no firn layer, but a thin snow cover was present.*

L74-75: Does P wave energy in the S wave explicitly preclude shear-wave splitting? If one assumed the P wave energy is linearised (which could be easily tested) then I don't think it should affect the shear-wave splitting measurement? I would also expect a resonance of the instrument to be linear and so not affect shear-wave splitting?

- *SWS analysis workflow in Lutz (2020) includes amplitude criteria with rations of L/QT. These are not met in the case of oscillations spread on all three components in the S-wave window. Changed "precludes" to "impedes" in text.*

L87: The underlying cause of the low SNR for zero-offset S waves will be the radiation pattern of the source. For a hammer shot directly above an instrument, you will be in a null for the S wave radiation pattern. I think it would be worth stating this rather than just the SNR effect of this.

- *Added description of this effect.*

L87-89: This sentence doesn't make grammatical sense. Needs restructuring.

- *Removed second half of sentence.*

Figure 2: The data show some interesting things. I like the indication of increasing travel time with depth, but wonder whether it might be clearer to present the difference in travel times, relative to a reference profile (e.g. in-flow) as it is difficult to tease out velocity perturbations in this figure.

- *Main point of the figure is to show azimuthal anisotropy not velocity perturbations.*

L97-98: Good that you state that there is no firn layer here. However, I think it would benefit from stating explicitly earlier in the text too (but don't remove from here as it is key to your assumption used to calculate velocity).

- Added statement in in Introduction that there is no firn layer.

L105: I think the uncertainty in tp and x are probably underestimated, but maybe its not too important.

- Probably they are quite optimistic, but left unchanged.

Equation 3: Factor of 2 shouldn't be in first term of velocity uncertainty (check the differentiation of Eq. 1). Otherwise equation seems sound.

- Removed factor 2

L108: Not quite sure what Table 2 adds? I don't quite understand why in one direction one can observe such a high range of velocities? There is no physical reason for this I don't think, yet your uncertainties suggest that it is real. I think this again points to the uncertainties mentioned above being underestimated.

- Table 2 was meant to illustrate that velocities are different between profiles which is an indication of anisotropy. But Table 2 is not very important as later Figures 7 and 8 show the same thing. Removed Table 2.

L109: How does this temperature compare to the ice column? Anisotropy can be very sensitive to temperature. Just worth stating that this is a similar temperature to the ice column in the field, assuming is approximately is.

- In situ temperatures are now mentioned in the text: 003: -7C, 007: -13C, 010: -15C. Freezer temperature was oberserved to not affect anisotropy in experiments, hence the difference between in situ and freezer temperature is interpreted to be unproblematic. Colder temperature facilitates acquisition of ultrasonic data as a better coupling of transducers to the ice core could be achieved.

Figure 3c-e: Nice results! Very clear anisotropic signal.

- Thanks!

Figure 4: It would be good to see the corresponding seismic observations also plotted on Figure 4. Unless I am mistaken, you have seismic measurements corresponding approximately to each of your ice core results? Why not plot them together?

- Seismic velocities from VSP data are plotted in Figure 7 and 8. VSP seismic data mainly show variation with incidence angle from vertical, whereas ultrasonic data show variation with azimuth. In Figure 4 VSP seismic data would be plotted as a spread of different values at only 4 azimuths. This is seen as not helpful. Having separate individual plots 4, 7 and 8 is regarded to show the important trends in the data in the clearest way.

Figure 4: Ah, vp does appear to vary azimuthally. This is an interesting result and should probably be highlighted in the abstract. It is potentially expected, but not really been observed at typical seismic wavelengths. This is maybe a reason why shear wave splitting might not work (nullifying one of the assumptions I made in a previous comment).

- Stated in abstract that vp (and vs) show anisotropy in the horizontal plane

L151: Need units for the velocity measurements.

- Added units.

L157: Are any bubbles or cracks observed in the samples?

- Bubbles are observered in the entire length of core, clear fractures are observed in samples from the top 10m of core. This is now mentioned in text with reference to Thomas (2021).

L161: There hasn't been clear evidence yet of the high degree of seismic anisotropy in the VSP. Could you create a figure similar to figure 4 but for the seismic data? I can see from Figure 2 that there are velocity differences, but it is unclear whether they are coherent/correspond to the various orientations of the ray paths with respect to flow. In summary, To make the statement in L161, I think you need to display your results more clearly.

- Table 2 was meant to give an indication of anisotropy, Figure 7 and 8 show this later in the manuscript. No preemptive reference is made to Figure 7 and 8 yet since the figures fit better in Model results section as they show both measurements and model results. Described in Section 2.2. The systematic traveltime differences between profiles shown in Figure 4 indicate anisotropy, i.e. t_P of Perp and Flow are lower than the arrival times at the diagonal profiles . As indicated in an earlier comment the VSP data do not display the azimuthal velocity variation very clearly but rather variation with incidence angle.

L165-167: This sentence seems a little random. Is it necessary?

- Removed sentence

L186: I was really getting convinced by the merits of the CPO modelling, until you mentioned the issues with comparing GHz vs. Hz frequency regimes. I am not well versed in the validity of comparing such disparate frequency regimes, so could you elaborate a little more in the text on why the CPO modelling can be compared to the observations.

- This issue is poorly understood generally but often applied. Picotti (2015) and Diez (2015) tested a variety of anisotropies to fit seismic data and none is catastrophically wrong, so there seems to be a consensus that it is acceptable rely on lab-derived elasticity tensors to describe seismic anisotropy. We assume that absolute values of velocities change between frequencies, however the anisotropy is constant. Our experience showed that the Gammon (1983) elasticity tensor overestimates the measured seismic velocities, but if the ratio between anisotropic velocities and the mean velocity is formed, the absolute velocity trend is mitigated and they become comparable.

Table 4: The misfit uncertainties are larger than the actual values. What does this mean?!

- Changed: Uncertainties are now given, instead of limits.

Section 4.3: I have to confess that I got a bit lost here. Lots of data without much text explaining what it all means. It feels tough to read as so many symbols in the prose. I think it could do with some reworking and replacing all the "chi"s with "phase-misfit" or something. Could also consolidate figures as there is a lot of data without much perceived interpretation. The rest of the paper to here reads so well, so worth brushing up on this section.

Section 4.4: Much better than 4.3. Could you restructure Section 4.3 to be similar to 4.4?

- Made an effort to brush this section up with less technical language. See tracked changes.

Figure 10: Consolidate this figure by putting the measured and modelled data together (as different coloured scatter points). Should make the results look more compelling too.

- Done

L284-285: Make this sentence clearer, as the "however" in the middle makes it unclear.

- Removed "however" in the sentence.

L287: What's a side minimum? Be more precise.

- Replaced wording: "side minimum" with "local minimum"

L290: This sentence should be reworded. You are basically stating that when you remove some of your observations, the results become clearer. You are actually stating, I think, that the data is noisy and so removing so many azimuths reduces the apparent "noise" in the results. Also, you are not really "sparse sampling" as this is a technical term for something completely different (randomly sampling then reconstructing the signal based on assumptions about the frequency content), so try to avoid this terminology.

- Reworded sentence

L296-304: This is more like a results section than discussion. If this remains in the discussion, you need to suggest why the 90 degree results are still very scattered and "unrealistic".

- We think that it is necessary to briefly describe the results shown in Figure 13, which we think is placed most suitably in the Discussion section. Described in text that the actual set of sampled azimuths (i.e. first sample) has a large influence on the model result for wider azimuthal spacings. The models found from 90 degree sampled data show the strongest dependence on the first sampled azimuth, hence show scattered parameters.

Figure 13: What do the size of the scatter points represent? It should be mentioned in the caption.

- Different sizes represent azimuthal spacings. Now mentioned in caption.

L312-314: Again, more of a results style text than discussion. Why is the VSP CPO modelling ambiguous if only P waves are considered? Is it because P waves shouldn't exhibit significant anisotropy in this case? I think more detail, i.e. some discussion is needed here.

- This links back to the points in Figure 13. Described from line 350 that coarse sampling is a key problem to address for CPO inversion. The VSP survey happens to sample "nodal" planes of P-wave anisotropy.

L331: Do you mean "measured … (CPOs)" or "modelled… (CPOs)"? I think the latter. If so, make the change.

- This actually refers to the measured CPO in EBSD. Slightly reworded conclusions to make the point clearer.

L333-337: Change the word “matches” to “agrees” or similar throughout this paragraph. The data does not exactly match, but does broadly agree.

- *Changed “matches” to “agrees”*

L342: Change the word “degraded” to something else. Doesn’t do what you’ve done justice and sounds negative rather than positive.

- *Changed “degraded” to “downsampled”*

Technical comments:

L10: “that matches well the measurements” -> “that matches the measurements well”

- *Changed*

L16: “As result” -> “As a result”

- *Changed.*

L41: comma required before “which”

- *Added comma.*

L92: “times” -> “time” otherwise it is a confusing sentence.

- *Changed*

L94: “registered later” -> “slower”?

- *Changed*

Figure 2: The figure labels need to be clearer (i.e. a,b,c etc at top of figure and bigger font).

- *Made captions larger*

L95: Better not to refer to Equation 1 before it appears in the text. Instead, just state that this is the equation used to calculate seismic velocities. Same for other equations, especially Eq. 3.

- *Removed references to Equation 1 and 3 before their appearance*

L125: Small point, but sampling rates should be in units of Hz. You are stating a sampling interval.

- *Changed to sampling interval*

L163: Remove the word “very” – subjective.

- *Removed*

Generally, lots of examples of “…. however…” in sentences. Makes it difficult to read. Need a comma or two when using, or ideally only use at beginning of sentences.

- *Made an effort to check for usage of and remove “however”. See tracked changes in text.*

Table 4: Could the uncertainties be displaced consistently with the rest of the paper (i.e. + and – rather than []).

- *Done*

L325-326: Sentence could be improved to communicate the idea better. Basically: greater azimuthal sampling of VSP required to improve CPO model constraint.

- *Reworded sentence.*

The author responses are given in italics below individual reviewer comments.

These authors conducted a combination of field, laboratory and modeling studies on the shear margin of Priestley glacier. In the field, they shot seismic waves in different directions from the borehole and receive the waves in the borehole. In the laboratory, they shot and received ultrasonic waves around the cored sample. Using a forward model, they could find a CPO that fits the observed seismic velocities. This agreement suggests that the CPO and its resulted anisotropy can be constrained by seismic survey. Understanding the CPOs of fast-moving glaciers is crucial to the predictions of the velocities of these glaciers and therefore the balance of ice masses. These authors are establishing a method that could constrain the CPOs beneath the surface without drilling the glaicers. This manuscript continues the work published by the authors in 2020 on JGR Earth Surface. I am happy to see that these authors are making improvements and progresses on this project. I highly recommend publishing this manuscript but would ask the authors to consider the comments listed below, which in my humble opinion could improve the manuscript.

1. Writing. I can basically understand the wording of this manuscript, but in some cases there could be misunderstanding, which can be easily clarified by rewriting the sentence. Besides, a large portion of this manuscript was not written in proper scientific written language. I hope the authors could make some improvements. In the manuscript file, I have commented some places I think would cause confusions. Some general issues are included but not limited to the followings. (1) When a multi-word compound noun (or adjective plus noun) is being used as an adjective to describe another noun, then one need to hyphenate it. For example, “vertical seismic profile experiment” in line 43, “Horizontal Cluster CPO type” in line 48, etc. (2) Please try to avoid indefinite antecedent, that is, the ambiguous “this” and “that”. For example, “This precludes …” in line 73. (3) Some verbs often infer logical relationships, so extra cautions are needed. I have commented some places in the manuscript file.

- *The comments in the attachment are adressed in the text, please see the tracked changes in pdf. We have followed the reviewer’s suggestions and have now hypthenated vertical-seismic-profile, Horizontal-Cluster-CPO.*

2. Some figures lack sufficient descriptions in the main text. Figures often contain a lot of information, useful and useless. It would be better if the authors could lead the readers through a figure, so that we know what the important features are. (1) Figure 4. Most of the descriptions of Figure 4 are in Section 3.2. When I look at Figure 4, the first thing that caught my eye is the velocity difference between samples, the second thing is that all three sample share the same trend of velocity with degree, and the third thing is that the peaks of red and yellow data are mostly at the same angle, while there seems a “phase lag” in angle for the blue data. However, these features were not mentioned in Section 3.2. So I don’t know if I get the correct messages from the figure. I see some of what I concerned were discussed in the Discussion section, but it is necessary to describe these in Section 3.2, which is a “results” section.

- *Features of Figure 4 are now briefly described in Section 3.2.*

   (2) Figures 7 and 8 are very complicated but are only mentioned in two paragraphs from 208 to 222. The figures are very pretty with cold to warm colors indicating the depth. However, I could not get how to relate the depths with the three model curves. The authors did not

mention the use of depth in these figures (apology if the authors actually used the depth info but I missed it). If the depth info is useless, then there is no need to show it in the figure. If the depth is useful, then please describe the results in the text.

- *Depth is now discussed in text: An important observation is that trends in velocity are consistent across the entire depth. The variation with incidence angle is an indication of seismic anisotropy. The combination of these two observations is interpreted to show that a single CPO must be present across the entire depth.*

(3) Figures 11 and 12 should belong to discussion section but were only briefly mentioned in lines 243 to 248. I think it would be interesting to discuss possible causes for the differences between the three.

- *Possible causes for differences in Figures 11 and 12 are discussed in Section 5.1. Figures 11 and 12 are now moved into this Section to move them closer to the discussion in text.*

3. Because three aspects of work were combined in this study, the manuscript is structured a bit differently. Based on my understanding, section 2, 3 and 4 are methods and results for field work, laboratory work and modelling, respectively. In section 2, the authors talked about data collection method, travel-time differences and velocity differences. The section is entitled “Seismic anisotropy informed by a vertical seismic profile shooting”. When we talk about seismic anisotropy, two aspects should be included: the radial anisotropy, i.e., velocity difference, and the azimuthal anisotropy, i.e., fast direction. If the authors would like to keep the section title as it, I would ask them to add some descriptions of the fast direction in Section 2.3, so that this subsection is a complete description for anisotropy. If it is not easy to reconstruct the fast direction, I would suggest changing the title of the section and the subsection. The same issue applies to Section 3, in which only wave velocities were described.

- *The section titles have now been changed to “Analysis of ...”. The reviewer is correct that no full description of seismic anisotropy is given in sections 2 and 3.*

4. OK, Section 4 is my biggest concern of this manuscript. Here is my understanding for section 4. (1) A CPO pattern was decided: horizontal clustering of c-axes. (2) Calculate the seismic anisotropy induced by this CPO, given different cluster orientation angle and opening angle. (3) Find the best fit for the actual anisotropy. (4) The CPO that gives the best fit is the same the CPO analyzed from the core. The logic is clear. However, I cannot agree with step (1). I think one purpose of this manuscript is to demonstrate that CPOs in a glacier can be constrained by seismic experiments in the field without analyzing the actual core. So I think a reasonable approach is to start with different CPO patterns, like what these authors did in their 2020 JGR paper. In sheared ice, two CPO patterns could form, a single cluster of c-axes and double clusters of c-axes. Given that in the real case the glacial valley may narrow or widen, compressional or tensional deformation may also occur, leading to elongated clusters. So there could be different patterns to start with. I understand that considering multiple clusters may significantly increase the amount of calculations, but it is at least necessary to let readers know why this pattern was decided (Please do not start by mentioning the actual CPO, which is the final answer to check the models). Also, if no other

CPO patterns were considered, some discussions are necessary to explain the current limitations of this modeling approach.

- This is a very valid point, but an improvement of modelling strategies is not the scope of this paper. The data show great potential to investigate different CPO, but instead of focusing on CPO modelling we wanted to achieve a better understanding of seismic anisotropy caused by CPO. We start one step ahead to the situation that is generally encountered, since we know the cluster geometry from EBSD measurements of core samples. The question we now wanted to answer in detail is if seismic data can find a realistic representation in this case where we have all the information. The short answer is: not if we rely entirely on P-waves. We wanted to conduct a detailed study of geometry and phases needed to constrain CPO and this shows that there are ambiguities caused by sampling geometry. The limitations are very apparent within this Cluster CPO type (which we know is correct). Adding more CPO models simply would go beyond the scope of this paper. There is an ongoing collaboration to use the data described in this paper to develop new modelling strategies, so the reviewer's comments will very likely be reflected in a future manuscript.

5. One thing could be helpful for readers to understand this manuscript better is to include some introductions of the wave velocities along different crystallographic axes of ice. In many papers on seismic anisotropy of the upper mantle for example, the wave velocities along different crystal axes of olivine were always introduced, although these people know olivine very well. Ice is not studied as much as olivine for seismic anisotropy. So I think it is necessary to briefly introduce the anisotropy of ice single crystal. Moreover, during the introduction of anisotropy of single crystal, it is also necessary to explain why only c-axes were considered in the modeling, but not a-axes or others.

- A short paragraph and figure on seismic anisotropy in ice was now added to the Introduction. It is mentioned that the anisotropy of ice exhbits hexagonal symmetry: only c-axis is important.

6. Currently, the seismometers were placed vertically in a borehole. The authors also found that no significant bending of the wave pathway (line 100). So the application is a bit different from the seismic survey people do for the crust. I wonder will it be possible to apply this method without a borehole, that is, seismometers on the surface receiving reflected waves? I know this question is steps ahead of the current stage of the method. But the future of this method would be worth discussing.

- In principle yes, and has been done by e.g. Bentley (1971) and Blankenship (1987). There are reflection seismic data from the site using explosive sources. We believe it would be exciting to apply our modelling approach to these data, since this would extend our observations to the bottom of the glacier. However analysis of reflection seismic data comes with challenges: using only surface seismometers, it is harder to get absolute velocities without performing raytracing. The assumption of a homegeneous ice mass used in this work also becomes questionable when the entire thickness is sampled.

Line by line comments are marked on the manuscript file.

- Please see tracked changes in pdf for line by line comments.