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.