Azimuthal Anisotropy From Multimode Waveform Modeling Reveals Layering Within the Antarctica Craton, by Beghein and Xu

Response to Reviewers

The authors thank the reviewers for their detailed comments. Our response to each comment is below in blue font.

Reviewer A (Joseph Asplet)

In this paper, the authors invert for a new 3-D model of shear-wave velocity and azimuthal anisotropy for the upper ca. 300 km of the mantle beneath Antarctica. The shear-wave velocity results broadly agree with previous work, showing a similar east-west change in velocities. This work then allows for the inversion for azimuthal anisotropy, which also shows significant east-west changes in fast direction. For East Antarctica, changes in azimuthal anisotropy with depth indicate layering within the East Antarctica craton. The paper reads well and is a contribution of value that will be of interest to the readers of *Seismica*. The title and abstract are appropriate for the contents and the conclusions are supported by the data presented.

I enjoyed reading the paper and with a few edits it will be a very nice contribution to *Seismica,* congratulations!

Main comments:

1. The software used for the inversions is not readily accessible. I would strongly recommend the authors consider creating a Zenodo repository for the codes used in this paper, as it will ensure results can be reproduced even if there is further code development.

   The MCMC measurement code we developed will be made available on GitHub upon the manuscript's acceptance. A sentence with a link was added at the end of the paper.

2. Similarly, the data used is not readily available, however, just about enough information is provided for a sufficiently motivated reader to reproduce the dataset from the IRIS DMC. I would suggest the authors include a second supplementary table detailing the exact event used in the final dataset.

   The lists of events and stations used are now shared in Supplementary CSV files.

3. Non-technical summary. This reads well, but in lines 27,29,30,32 you refer to anisotropy which is too technical to include without explicitly defining what seismic anisotropy is. This needs to be better joined up with the explanation in lines 25-6. I think something along the lines of “this is also known as seismic anisotropy” at the end of the sentence would suffice.

   Thank you for noticing this. We added “known as seismic anisotropy” in the first sentence of the non-technical summary.

4. The explanation of two types of seismic anisotropy in section 1.3 could be clearer. Transverse isotropy (line 103) is just one kind of symmetry that can be invoked (and is often the most convenient to use), whilst azimuthal anisotropy is much

more general. For example, a horizontal transverse isotropy fabric (where the symmetry axis is vertical) would give azimuthal anisotropy. Perhaps a more general comparison of radial and azimuthal anisotropy would work better?

After re-reading what we wrote, we agree that it was not clear enough. We have rewritten these few sentences to focus on how anisotropy manifests itself in surface waves instead of keeping more general.

5. For lines 104-111 it should be worth noting that whilst LPO olivine is a common mechanism of upper mantle anisotropy it is not the only one. SPO of melts is commonly interpreted as a mechanism for upper mantle anisotropy beneath the Main Ethiopian Rift with broadly rift parallel fast velocity directions (e.g., Kendall et al., 2005).

   This is correct. Thank you. We added this to the paragraph and specified that LPO is the generally accepted explanation for observations of anisotropy at a global scale.

6. In the inversions attenuation (Q) is not accounted for, as far as I can tell. Presumably, this is justifiable but for those less familiar with surface wave inversions a comment explaining this would be valuable. Given that your results show a stark contrast in structure from West to East Antarctica (e.g., the variation in LAB depth) one might expect a heterogenous Q also. Would this leak into the inversions if not accounted for?

   As explained in Xu and beghein (2019), because the amplitude of the synthetic seismograms depends on the scalar moments of the event, and because inaccurate scalar moments in catalogues might cause an increase in the data misfit, we implemented an energy equalization method (Lebedev et al.2005) before calculating the misfit between observed and synthetic seismogram. The aim is to equalize the energy of the synthetic with that of the data by multiplying the amplitude of the synthetics by a constant (Eq. (5) in Xu and Beghein, 2019).

   Additional discrepancies between synthetic and real seismogram amplitudes could be due to inaccurate Q values. Instead of adjusting/inverting the attenuation in the model, which was beyond the scope of the study, we opted to apply a quality control criteria and remove paths for which the misfit was too large after energy equalization.

   In general, it is possible that inaccurate Q may cause the predicted waveform to appear slower or faster than the observations since it can make the waveform broader or narrower. Thus, if attenuation is held fixed incorrectly, structural inferences can be biased. However, in our case, because we removed along-path measurements with high misfit before constructing the phase velocity maps, this is not an issue that could affect our results significantly.

   We added a paragraph in section 3.1 to clarify this point.

7. Line 301. I think I know what you mean by "slightly anisotropy" but a definition of what constitutes "slightly" would be useful.

We went back to Montagner and Nataf (1986) to confirm their definition of "slight anisotropy" and included this in section 3.2. It reads:

"In an isotropic medium, surface wave motion decouples into two independent sets of Love and Rayleigh waves, with Love waves recorded on the horizontal components of the seismogram and Rayleigh waves on the vertical component. In an anisotropic medium, with a few percent anisotropy (Park and Yu, 1992), they can interact, yielding quasi-Love waves on the vertical component in the Love-wave arrival window and quasi-Rayleigh waves on the horizontal components (Crampin, 1977). Montagner and Nataf (1986) showed that, if the anisotropy of the medium is slight enough that quasi-Love and quasi-Rayleigh modes can be defined, the azimuthal variation of SV-wave propagating horizontally with velocity V_{SV} can be expressed as…"

8.
   a. In the discussion (lines 474-478). I agree that the change in fast directions between 75 and 150km suggests a second layer of anisotropy. It is interesting that anisotropy at the base of the craton aligns with the APM. An allusion is made to similar observations for the North American craton, but a little expansion on how similar the observations are would be nice. (i.e., Does the basal layer in North American craton also align with APM?)

      We expanded the discussion to include details about the similarities and differences between out model for Antarctica and the North American craton results from others.

   b. Adding some of the annotations from Figure 1 or 14 to Figure 13 would help readers orient themselves when you discuss the LAB depth beneath the GSM. I found myself flipping back/forth to make sure I was looking at the right spot.

      Done.

9. The disagreement between the SKS splitting observations (e.g., the ~N-S line of splits from Barklage et al., 2009 and Lucas et al., 2022 extending from the Ross Sea) and the prediction, for both fast polarisation and delay times, from your model is striking. Some more discussion of how results from both SKS and surface waves can be reconciled would be nice.

   We expanded the discussion on that topic.

   Copyediting notes:

1. The acronym LSQR on line 305 should be written out the first time it is used. Done
2. In line 41 there is a double bracket following Accardo et al., 2014. Removed.
3. The corrections e.g. -> e.g., should be made in the following lines: 39, 52, 58, 99, 113, 114, 115, 117, 118, 120, 122, 138, 271, 354. Done

4. Line 89 the references are not in chronological order. Corrected.
5. Line 249 'oaths' -> 'paths'. Corrected.
6. Line 268 Double bracket following PREM reference. This was necessary as we wrote" the Preliminary Reference Earth Model (PREM, Dziewonski and Anderson (1981))"
7. Line 335 i.e. should be followed by a comma. Done
8. Line 354 double bracket following Yuan and Beghein (2014). This was necessary: "instead (e.g., Yuan and Beghein (2014))."
9. Line 485 double bracket following Savage (1999). This was necessary: (see review by Savage (1999))

Reviewer B:

In the manuscript entitled "Azimuthal anisotropy from multimode waveform modeling reveals layering within the Antartica craton", authors Caroline Beghein and Haotian Xu used a novel method in surface wave tomography to image the anisotropic nature of the Antarctic craton by leveraging the growing amount of available seismic data. Their method reinvents the two-step approach to surface wave tomography by first incorporating waveform fitting in a reversible jump McMC scheme to invert for path-averaged structures (and hence, neglect the need of estimating local surface wave dispersion measurements), followed by a linearized procedure to simultaneously obtain isotropic velocity and azimuthal anisotropy parameters. They recovered complex patterns of azimuthal anisotropy, exuding depth variations particularly across the East

Antarctic Craton. The manuscript is organized, well-written, and almost ready for publication in Seismica. It would be much appreciated, however, if the authors could clarify some aspects of the study, followed by a minor amendment of the manuscript.

Main point 1:

In Section 3, readers who may not be entirely familiar with surface wave tomography might think that the path-averaged velocities listed in Section 3.1, especially Vs, are isotropic. If I understand it correctly, the inverted 1D path-averaged Vs structure is inherently anisotropic, which is why it is used as data (in Section 3.2) to invert for the isotropic velocity V_0, and the parameters associated with the perturbation term in Equation 6 assuming a slightly anisotropic medium (i.e. coefficients A and B). If this is the case, it would be better to emphasize in Section 3.1 that the path-averaged 1D Vs structure may already be intrinsically anisotropic.

We clarified this by adding one sentence after Eq. (6): "Both the path-averaged velocity, $V_l(z)$, and the local velocity along the path may depend on the azimuth of propagation."

Main point 2:

Personally, I really do not see the need in constraining the 4Psi terms in Eq. (6) when you invert the intrinsically-anisotropic path-averaged Vs structure since the 4Psi terms are mainly sensitive to the parameter N associated with Vsh. Furthermore the amplitude of the sensitivity kernels associated with 4Psi are relatively low especially in the upper mantle (e.g. Montagner and Nataf, 1986; Montagner and Tanimoto, 1991; Maupin and Park, 2007). While it was a good exercise to test how it maps in your region of interest, I personally think it would be best to only show A1 and A2 in Eq. (6) (since you invert only Vsv), and remove most of the content of Section 4.1 for brevity.

We prefer keeping Eq. (6) general since Visser and Trampert (2007) showed that for higher mode surface waves, the 4 Psi terms may be needed.

Main point 3:

As someone who is also interested in probabilistic approaches to geophysical research problems, I would be eager to know what hindered the authors in making the inverse problem fully probabilistic. This removes ad-hoc constraints in regularization, streamlines the process of going from waveforms to 3D anisotropic structure, and it would also be beneficial to quantify the uncertainty limits and the resulting marginal posterior distributions of the isotropic-averaged V_0, and the azimuthal anisotropy coefficients A1 and A2.

That is a very interesting idea and it would indeed be a great way to quantify uncertainties on anisotropy. However, it would require a lot more computer power and speed would likely be an issue as computing time in model space searches increases very rapidly with the number of unknowns. Here, we are talking about searching for V_0, A_1, and A_2 (and ideally B_1 and B_2) over a number Ng of grid cells and at a certain number of depth nodes (which can vary with the method used here). One step toward this would first to replace Mineos with a computationally less expensive code, but the options to calculate synthetic seismograms by normal mode summation are very limited. I am only aware of Hermann's code from the "Computer Programs in Seismology" package. It would also be beneficial to replace the rj-MCMC method with a Niche Genetic Algorithm as well, as done in Li and Beghein (2022), but it would still be computationally very demanding to search for that many parameters.

Main point 4:

In Section 5.2 second paragraph, WANT may be a potential hotbed for edge-driven convection or EDC, i.e., small-scale convections induced adjacent to a thick and stable lithosphere due to temperature variations along the boundary (e.g. King and Anderson, 1998). The localized deformation induced by EDC could generate olivine LPO that may be subparallel to the direction of flow, which in this case, could be perpendicular to the APM (i.e. towards EANT). Could this be a potential reason for the alignment of the fast azimuth perpendicular to APM in the immediate east of WARS and immediately above EWM at shallow depths? (i.e. 75 km and 150 km) (Figure 10). Kindly comment on this!

That is an interesting idea that we had not considered and we thank the reviewer for the suggestion. We have now added a few sentences in section 5.2 to discuss EDC as a possible explanation for our WANT results.

Main point 5:

In Section 5.3 third paragraph, could you elaborate more on the origin of depth-varying azimuthal anisotropy? Is it intrinsic or extrinsic in nature? LPO vs SPO? That is, could structural layering associated with small scale heterogeneities across EANT be mapped into large-scale azimuthal anisotropy? Furthermore, could basal erosion across cratonic roots create small-scale isotropic heterogeneities that may be mapped as SPO-induced extrinsic anisotropy (e.g. Magali, JGR:SE,2021; Wang et al, GRL, 2013; Faccenda et al, JGR:SE, 2018,)? What are the implications of the authors' models regarding the above-mentioned?

We appreciate the suggestion and we added a section 5.4 to further discuss possible origins of the anisotropy signal we observe. However, since SPO modeling so far has been performed for horizontal layering, which yields radial but not azimuthal anisotropy, such studies are not directly applicable to our results.

Line comments:

Line 108-109: While this is true for some fabrics, please specify that this is the case for low-pressure olivine at dry upper mantle conditions (i.e. A-type Olivine).

We added this specification to the text with a reference to Jung and Karato (2001)

Line 113-114: SPO-induced extrinsic anisotropy is also noteworthy (e.g. Auer et al., 2015; Wang et al., 2013).

Thank you for the reminder. We have now included this in the second paragraph of section 1.3

Line 252-253: Is the prior a uniform distribution? Yes, and we have now added this to the text in the paragraph below equation (3).

Line 470: Please cite an example in regions analogous to the shallower portions of EANT where frozen-in anisotropy might be evident, and provide a reference.

We have now referred to Yuan and Romanowicz (2010) and their study of the North American craton in section 5.2, at the beginning of paragraph 3.

Figure 4: Maybe it would also be good to show the waveform fit associated with the mean model after the burn-in period.

The figure was updated accordingly.

Figure 9: Are the color scales dlnVs for Fig. 9 b-d? Please add an identification beside the color

scales.

Thank you for noticing that the axis label was missing. This has been corrected.

Recommendation: Revisions Required

Reviewer C:

The seismic events with magnitudes ranging from 5.5 to 7.0, recorded by both permanent and temporary seismic stations spanning the years 2005 to 2020 are used by this manuscript. Utilizing these records, the authors tried to construct a 3D isotropic velocity and azimuthal anisotropy model beneath Antarctica, extending down to 600 km. Employing a multimode Rayleigh waveform fitting technique, they first utilize a MCMC approach to obtain average vertically polarized S-wave velocity profiles fitting fundamental and higher mode Rayleigh waveforms between 50s and 200 s for each ray-path. Subsequently, these profiles are

inverted a 3D velocity and azimuthal anisotropy model. Notably, the study successfully resolves azimuthal anisotropy within the 60-300 km depth range and utilizes the midpoint of the interval over which VSV decreases as a proxy for the LAB depth. The isotropic velocity model and estimated LAB depths are found to generally align with previously observed variations across the Antarctica craton from east to west.

Furthermore, the authors interpret the observed variation in the anisotropic model as indicative of distinct deformation histories between the eastern and western blocks. Particularly significant is the depth variation in azimuthal anisotropy, which the authors assert as a pivotal finding. This, combined with shear wave splitting results of core-refracted phases, may illuminate the geodynamical implications of seismic anisotropy in Antarctica.

The manuscript is well-written, the used dataset and applied methodologies are clearly explained, and overall I think the authors make a compelling case for a set of known structural and tectonic features that might be related to the observed velocity anomalies and anisotropy variations. I recommend eventual publication of this work and suggest that the final paper should be supplemented by the estimated 3D velocity and anisotropy model so that it can be used by future studies in the same region. However, as there is always room for improvement, the figures can be improved to make the manuscript easier to read for the readers less familiar with the Antarctic geologic features. I offer a number of suggestions below that I hope will be of help to the authors in making their work more broadly accessible. My comments are linked to line numbers (L.) in the PDF I received for review.

L. 206-210: Elaborating on the favorable conditions for the generation of higher-mode surface waves might be informative.

We added some of those details in the Data Selection section.

L. 254: resolved → resolve? corrected

(1989)?

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I am not sure which line this comment refers to, but the reference above is for scaling radial anisotropy parameters, which we do not deal with in this study. We scaled P-wave velocity and density anomalies to shear-wave velocity anomalies as in Montagner and Nataf, 1986.

Please expand on how the scaling factors at different depths are extracted.

We have expanded this paragraph and added references in addition to correcting a typo regarding the VP to VS ratio.

L. 291: What is the number of iterations used as the burn-in period?

The burn-in period was 80,000 models for each event-station pair. We added this to the revised text.

L. 303: Can you briefly describe the relations between the coefficients in eq.6 and the elastic parameters and different wave velocities?

We added this to section 3.2

L. 361: Why are perturbations calculated relative to PERM? Would it be more informative to calculate them relative to the average velocity at each depth? Couldn't this approach potentially enhance the identification and interpretation of high and low velocity anomalies?

We performed a test using the average velocity model at each depth and the resulting isotropic maps did not differ significantly from the ones obtained here with PREM. We added a sentence to explicitly say so in section 4.2.

L. 306-307: Plot a map showing the 2D triangular grid.

We added a panel to Fig. 3 that includes the 2D model grid.

L. 357-363 (Fig.8): Please mark all the discussed locations on the maps (WARS, MTZ, ...)

Done

L. 364-373 (Fig.9): Please mark all the discussed locations on the map in Fig. 9A

For clarity of the figure, we only labeled the GSM on the map and we modified the figure by rearranging the panels. Now panel (a) corresponds to path A-A', panel (b) is for B-B', and panel (c) is for C-C'.

L. 377 (Fig.10): Please mark WILK on the map

Done

L. 397-403 (Fig. 12): The manuscript would benefit from further clarification and discussion regarding the vertical resolution of the model. Additionally, the checkerboard cell size is not specified, and it would be advantageous to include the results of checkerboard tests at additional depths beyond 150 km and 250 km. Expanding the depth range covered in Fig.11 and Fig.12 would provide a more comprehensive evaluation of the model.

The checkerboard cells shown here span 30 degrees in longitude and 8 degrees in latitude. The anomaly at the pole has a radius of 8 degrees. This information was added to the figure caption. Fig. 12 now includes additional checkerboard results at 105 km, 250 km, and 400 km depth.

L. 404-408 (Fig. 10; 11; 14): Specify the grid size for the estimated anisotropy.

The grid size was the same for isotropic and anisotropic components.

L. 417: Refer to Fig. 9 at the beginning of the paragraph.

Done

L. 471-474: Given the LAB depth range of approximately 160 km to 180 km (from Fig. 13) and considering the depth sensitivity and uncertainty associated with the fast directions, it remains plausible that the fast directions beneath EANT and GSM, which are subparallel to the APM, originate from sublithospheric flow.

This is correct and is discussed in the third paragraph of section 3.

L. 489 (eq. 10): Shouldn't the integration be performed along the (assumed) vertical ray-path in the resolved depth range of anisotropy? If so, would be better to modify the integral bounds in eq. 10.

This equation, which is now number 17, is the general equation. The total splitting delay time is the integral over all depths. In our case, the elastic parameters are not constrained throughout the entire mantle and are assumed zero below our max sensitivity, but if there was anisotropy at greater depths, we would need to integrate over a greater depth range. We prefer leaving the equation in its general form because a reader who is not familiar with this could be misled if we adapted the integration range to what we think we can resolve.

L. 502: What criteria are used to determine "good quality" splitting results?

We used the quality A and B measurements as defined in Accardo et al. (2014). This is now clarified in the text.

L. 551 (Data and code availability): I recommend supplementing the paper with the estimated 3D velocity and anisotropy model. This will enhance its utility for future studies in the same region.

The model will be made available to the public on Beghein's website after acceptance of the manuscript. A link has been added at the end of the paper.

Table 2: Mark the significant results (the first 4 rows of the second column and the first row of the third column)

We marked them in bold font.

Figure 4b, 9b, 9c and 9d: Add label to the colorbars. Mark the location of the features mentioned on top of the cross sections on the map in panel a.

As mentioned to reviewer B, for clarity of the figure, we only labeled the GSM on the map, and we modified the figure by rearranging the panels. Now panel (a) corresponds to path A-A', panel (b) is for B-B', and panel (c) is for C-C'.

Label the beginning and end points of the cross section (e.g., A at distance 0 and A' at distance ~4000 in panel b). Correct the caption "….which samples EANT, and C-C'(c)…."

→ "….which samples EANT, and C-C'(d)…."

Done

Figure 14: Mention the depth range of the predicted splittings in the caption.

Done

Recommendation: Revisions Required