We are grateful for the detailed and constructive reviews by reviewer Sanne Cottaar and editor Lauren Waszek that have helped us to improve the manuscript. Below we have reproduced the comments from the reviewer and editor verbatim in italics, with our responses in red color and regular font.

Overall, we agree with the suggestions by the reviewer and editor and have implemented changes to manuscript and figures in response to all of them. The most important points that we have changed in the revised manuscript version are the following:

1) We have completely rewritten section 8 of the paper, adding the additional synthetic analysis suggested by the reviewer. In this process, we have removed the previous Figure 12 and have added two new figures instead (Figures 12 and 13 in the revised manuscript version).
2) We have replotted or modified 10 out of the previous 15 figures.
3) We have streamlined and partially rewritten the methods-related section 2.1.
4) We have added additional discussion of our results.

These changes do not affect the main conclusions of the paper, but we believe that they have led to a substantial improvement of the manuscript.

Comments from Editor:

Based on a review I have received, your manuscript may be suitable for publication after some revisions. A second reviewer was unfortunately unable to complete their review in a suitable timeframe. However, I think that the first review is very thorough, and so in order to expedite the review process I decided against finding another reviewer and have instead provided some additional suggestions of my own (that do not duplicate the reviewer). I hope that this is acceptable to you, but please let me know if you would prefer me to find a second external reviewer.

We appreciate that you took the time to comment on our paper yourself instead of the reviewer who did not reply. We respond to all your specific suggestions in detail below. The reviewer, Sanne Cottaar, has done a great job reviewing our manuscript in detail. Her comments have been very helpful to improve the paper. We therefore do not find it necessary to consult a second external reviewer in addition to yourself and Sanne (but thank you for offering!).

The reviewer suggests several edits for the figures, and clarifications and additions to the discussion. I concur with the reviewer's comments and assessments, and also have provided a few additional suggestions:

Figure 2: add SKKS to third panel. Suggest plotting phases separately as well as combined, for ease of comparison. What is the signal arriving at distances >80 at ~15s in panel b?

Thanks for your suggestions. We have replotted Figure 2 to incorporate this suggestion as well others from the reviewer. The new figure shows the seismogram plots in a more appropriate aspect ratio, has the SKKS seismic phases plotted in panel c, and displays the phase figures separately. The signal that you were referring to resulted from the specific filter we used, which made the ScS wave coda look similar to a separate arrival. We have corrected this by using a different filter (and have changed the text accordingly).

Line 123-124: some simple additional modelling would clarify whether interference from SKS is noticeable.

You bring up an excellent point here. We started the project that led to this paper by asking ourselves the question of what influence of SKS phases may have on ScS arrivals at distances <70°. However, we found that this influence is surprisingly hard to quantify. For example, setting outer core P velocities to 0 to avoid producing an SKS wave will also influence the ScS reflection. The relative influence of these two factors is not straightforward to distinguish (we have tried). If you have a suggestion about a potential way to clearly distinguish the ScS and SKS influence on the seismic waveforms through forward modeling, we will be happy to try it!

The strategy we have settled on to identify the influence of SKS on ScS is to analyze the polarization direction of ScS at a large range of epicentral distances. For SKS, the polarization direction would be SV, while the ScS polarization is mostly controlled by the initial source polarization. ScS polarizations are generally not SV for distances <70°, giving a way to distinguish the two. This is now explained in the revised manuscript: "At epicentral distances between 60° and 70°, ScS initial polarizations are approximately opposite the S wave polarization as controlled by the source (Figure 5) due to the approximate SV sign-flip (Figure 3). However, because the sign-flip of SV is not exact (Figure 3), and because of the potential interference with SKS in some of the epicentral distance range (Figure 2), this pattern is by no means perfect. These two effects are hard to distinguish; however, analyzing them in isolation is not required to understand the conditions under which ScS can be used for analyses of lowermost mantle anisotropy, which is the main goal of this study." (lines 200-206). We now also refer to this text in the line 135 (previous lines 123-124).

Line 196: quantify variation by event depth.
Thanks – we now do so in lines 128-133 of the revised manuscript. We also mention that PS does not arrive at the relevant distances for events with focal depths deeper than 200 km, as pointed out by the reviewer.

Figure 6: suggest greying out left bottom two panels for ease of visualisation.
Thanks – we have done so in the revised Figure 6.

Line 252-253: what is the separation of ScS and S? Please quantify.
Thanks, we agree that this is good point to explicitly mention. We now point out that the "In the most extreme case, for a source-receiver distance of 60° and a surface event, S and ScS raypaths are up to 250 km apart at the bottom of the transition zone, so that the assumption that S and ScS raypaths are sufficiently close together may only be valid in cases of relatively generally homogeneous upper mantle anisotropy." (lines 274-277).

Comments from Reviewer (Sanne Cottaar):
Wolf and Long present a largely overview of synthetic tests to illustrate various issues and assumptions when it comes to ScS splitting measurements of the lowermost mantle. The calculations and tests look at polarisation and reflection effects, apparent splitting measurements, and how to correct for upper mantle anisotropy on the source and receiver side. The authors describe the limitations that need to be set to making these sorts of measurements and apply this to a global data set. This leads to a small set of (scattered) observations of lowermost mantle anisotropy.
The synthetic approach seems largely good (although I do question the test in Chapter 8). The results are interesting but lead to several further questions. The real observations are a bit discouraging as they show a lot of scatter. I think more discussion is needed here.

My comments are largely on how the work is presented. More care and thought could have gone into this. The methods could be better structured. Some of the figures could be simplified for the reader and still show the messages the authors want to convey. In some cases, some parts of the discussion are lacking and leave me with questions. These are laid out in more detailed comments below. Hopefully these will improve the paper and maybe increase the readership of such a technical paper.

Thanks a lot for these general comments and the specific suggestions below, which helped us to improve the manuscript. In particular, the suggestions regarding our figures and section 8 of the manuscript have been very helpful. We give a detailed response to each comment below.

Figure 1,
- Maybe show earthquake beach ball? Or SH and SV radiation patterns for specific distance range. Or maybe stations could be coloured by initial source polarisation to link to other plots.

We like this suggestion! To be consistent with the later figures, we have colored the stations by initial polarization.

In subplot C I don't understand the locations of the circle and cross if simple shear is assumed.

Yes, this was wrong, thanks for catching. We do not assume horizontal simple shear for the lowermost mantle elastic tensors. Instead, we orient the elastic tensors such that robust splitting can be measured. We have now changed the caption of Figure 1 accordingly.

Potentially subplot 1b could show the modified +/-3% models as well, saving space repeating them in future figures.

Good suggestion, we do this now in the revised Figure 1b.

Lines 105-107. There is some vague language about running the models. Why say you 'mostly use PREM'... and 'sometimes replace...
.. Why not say, 'we do isotropic simulations for
PREM, PREM modified by +3%, PREM modified by -3%, and S40RTS. For each model we run both earthquake depths. Potentially a table and labelling of all the models that are discussed in the paper would be useful.

Thanks, we have rewritten this part of the methods description in the revised manuscript, see lines 100-116. Specifically, we have added a clear enumeration of the synthetic models that we are simulating, following the above advice. We also now repeatedly refer to this section later in the text (e.g., line 327) instead of repeating ourselves.

Caption of Figure 1 also has the text: 'which is often used in our isotropic simulations' (makes it sound like there is some probability involved).

Thanks, we have adjusted the caption accordingly.

Lines-108 onwards: For each of the mineral physics based anisotropic models, I presume you take the anisotropic part of the tensor and add this onto PREM, and ignore the absolute velocity variations that such a model would give? Please clarify.

Thanks, we now clarify this point in lines 114-116 of the revised manuscript, which read: "The use of these elastic tensors leads to slightly different lowermost mantle velocities than PREM. The main goal of these simulations is to evaluate the influence of realistic lowermost mantle anisotropy on ScS seismic waves; the isotropic effects are analyzed in the previous set of simulations."

Figure 2
Consider stretching out the plot vertically, and/or reducing the scaling, as it will be easier to read with less overlap between the seismograms.

Thanks, we have done so.

Tough to see with the overlapping wiggles, but it looks like what you interpret as PS is also on the transverse component? Is this maybe another phase?
Would we not expect some SP to be on the radial as well?

We replotted Figure 2 with more useful plotting conventions. The wiggle that looked like PS on the transverse component was caused by the interference of S and ScS. In the new Figure, it is now clear that PS arrives on the radial component only. We have also rechecked arrival times around the predicted PS phases and find it likely that some PPS energy also influences the amplitudes. We agree with the point that SP arrives at the same time and explicitly mention this now in the text, see lines 129-133.

Line 122: Discussion on PS. This interference should surely depend on source depth? Please discuss the results for a 500 km source depth. Also, in my experience interference from surface reflections is always more significant in synthetics than in data. Have you looked for PS where you expect it in data?

Thanks, your comments on PS have helped us get a clearer understanding about the "mix" of seismic phases that potentially interfere with ScS at epicentral distances between 70° and 80°. We have now changed the text at the previous line 122 (now lines 128-133). This text reads: "At an epicentral distance of around 75°, interference from the PS and PPS phases, which arrive very close together in time at these distances, can be observed. Additionally, some SP energy (which arrives contemporaneously to PS for a 0 km deep source) likely arrives on the radial component. While PS interference can be observed in the record section shown in Figure 2, the phase is not observable at this distance range for events with focal depths deeper than 200 km, although some PPS and SP energy may still be relevant."

Figure 3.
This figure could be massively simplified to be in line with what is discussed in the text:

- *The imaginary reflection coefficients are also represented by the phase shift. Why show both of these?*
- *Figure 3 also shows three different models, but these are not discussed. It is a pain to compare these between the subplots. Maybe just say you ran for models with -3% and +3% velocity deviations and it does not change significantly and not show them. Or it you think the fact that at +3% the reflection reduces up to 65 degrees is significant, then discuss this and*

show the 3 models in one plot maybe only show one depth earthquake and say it does not change significantly with earthquake depth).

- *Reference for equations used in the main text, not in the caption.*

Thanks, we have replotted a streamlined version of Figure 3, implementing all the changes suggested above. We have also implemented the suggested changes to the text – see lines 162-163.

Figures 4 and 5.
Again I am not sure the figure design is in line with what is discussed and benefits the reader. Why not plot differential polarisation with respect to the initial polarisation (circles/lines could still be coloured by initial polarisation, similar to figure 9a)? Currently the reader is left to difference the dots for each colour, which is a lot to process. Plotting differential polarisation would also take out the dramatic looking swapping between the red and blue SH polarisations that occurs in both figures but isn't significant. Or one could show the polarisation direction with just a small stick that rotates and keep the vertical axes as a function of initial polarisation as it is now. Or the sticks could also be shown in a map view of the array as is done fast axes later on.

Thanks, we follow these suggestions in the revised paper version. In the new Figures 4 and 5, we now plot differential polarization and have streamlined the plotting convention to add results for a 500 km deep source.

Presumably the label should say longitude not latitude (also in Figures 6,8and 9)?

Thanks for catching – fixed.

There is again no discussion in the main text on the differences between the subpanels. The way it is plotted, makes it challenging to compare for the reader. In Figure 4 there seem to be no significant differences, and in Figure 5, I think there might also not be any interesting differences. Would it not be more interesting to show the different source depths, as PS interference should not occur with the deep event (Figure 6 suggests this will differ for the deep event)?

All good points. We have added additional discussion of these figures (see lines 200-212, 227). Also, we now plot results for different lowermost mantle velocities in a single panel. This gives us space to add results for a source depth of 500 km in the same figure. It is correct that PS interference is not expected to occur for deep events. We have adjusted Figure 5 accordingly. We also have changed our interpretation of PS interference later in the text; see, for example, lines 206-209, which read: "For distances between 73° and 79°, interference with PS can lead to estimated polarizations close to SV (Figure 5a). For deep sources (Figure 5b), no PS energy arrives; however, PPS and SP may still influence ScS waveforms around this distance range. Exceptions are observed at the stations at azimuths for which the initial polarization is purely SH, as the (P)PS amplitude is zero for them (Figure 5). For distances >80°, S and ScS merge, with S dominating, leading to polarizations that are close to the S initial source polarization (Figure 5)."

Lines 200-207: Lots of overlap with the methods reintroducing the models. You could just simply refer to which models you will now analyse instead of saying we conduct synthetic simulations...

We have streamlined these lines, now after line 249 in the revised manuscript.

Figures 6 and 8
It depends on how keen you are to show error bars, but again I might prefer map views of the splitting parameters as sticks as you do for the real data, which easily allows assessment of phi and dt at the same time. Particularly for the results in figure 8a, it looks dramatic that the results fall at +90 and -90 degrees, while these fast directions are close together. (I might be wrong on this, hard to say without seeing the plot)

Thanks for your suggestion. We have experimented with the plotting conventions for these two figures and prefer the (now modified) versions of the previous figures. The reasons are that the geographic version of the figure looked busy, which made it hard to immediately catch distance-dependent patterns. (These distance dependent patterns, however, are one of the main points we are making.) Additionally, it seems useful to plot uncertainty intervals to make it immediately apparent to the reader that the reported measurements are well-constrained. We believe that the modified versions of the figures are easier to digest and get our main points across more powerfully than their original versions.

Figure 7
To me the best fitting phi-dt space looks a lot like what is expected a null measurement, with a small bump found close to the initial polarisation. Is this the case for all these 'isotropic measurements? Can they not be identified as such by looking at these plots or rejecting measurements that are near initial polarisations?

This is a good observation, which we now discuss in line 227 of the paper. This observation is especially true for the isotropic PREM cases; however, not for the 3D tomography model. We have replotted Figure 7 for a case of apparent shear wave splitting in an isotropic 3D tomography model.

Liens 232-234 Suggested these models are 'replacing' PREM, please clarify if the anisotropic tensor is not added onto PREM. (If the models are properly introduced earlier, you could just mention which models you are now analysing).

We now clarify this in section 2.1, see lines 113-116.

Figure 8
For the bridgmanite model, it looks like some significant phi and dt results are plotted when there is little splitting intensity (dark red). The phi found also looks to be very close to the initial polarisation. Are these null results?

Yes, thanks for pointing these out. We have taken them out in the modified figure.

Lines 242-247: These statements bring up further questions:
- *Is there a relationship with phi as a function of initial polarisation? Eventually the authors propose two-layer modelling can potentially be done. It seems to me the authors have the perfect synthetic test here to plot results as a function of polarisation and show this is true.*

Once a sufficient number of measurements can be obtained in a particular region to conduct this type of modeling, a detailed synthetic evaluation will be necessary. For the purpose of our study, such an evaluation would open up a whole new research direction, which is not applicable to the limited number of real data measurements presented in section 9.2 (we have tried!). Therefore, we feel that this test is beyond the scope of this work (in particular given that we are already hitting the 10000-word limit). We plan to undertake this in followup studies, though.

- *Can any interpretation be given to the mean of the measurements (any relationship to the direction of flow??*

No, there is no clear meaning (unfortunately!)**.** We now explain this in lines 264-267 of the revised manuscript, which read: "Therefore, the mean splitting measurement as often determined in ScS splitting studies (e.g., Nowacki et al., 2010; Wolf et al., 2019; Pisconti et al., 2023) does not have a clear meaning for the interpretation of mantle flow directions, since the same measurement can be obtained for a variety of anisotropy scenarios and initial polarizations of the wave."

Lines 255-257: It seems like given the work you have presented phase shifts corrections using PREM would still be more accurate then assuming a flip? Even though some variations occur because of the velocity variations at the base. It seems like limited add effort to the work flow?

Our results for a distance range close 60°, at which the PREM-predicted phase shift corresponds to a precise sign-flip (Figure 3), do not indicate that ScS splitting measurements could be substantially improved by implementing the PREM-predicted phase shift. We now point to this in lines 280-282 of the revised manuscript, which read: "Additionally, our results for a distance range close to 60°, at which the PREM-predicted phase shift corresponds to a precise sign-flip (Figure 3), do not indicate that ScS splitting measurements could be substantially improved by implementing the PREM-predicted phase shift."

Line 258 Do you mean to say that splitting parameters for ScS can be inferred from S?

That's correct, we have adjusted the text as suggested.

Figure 11
Caption refers to Figure 11a, which should be figure 10a?
Also in the caption 'obtained' typo.

Thanks for catching – fixed**.**

Chapter 7
Would deep earthquakes be another way around avoiding source side anisotropy as well as PS interference)? From your final data set, this might significantly increase the global coverage.

We now discuss in lines 316-320 of the revised manuscript the fact that the use of deep events may help with avoiding source-side anisotropy, although seismic anisotropy has been detected in the transition zone and uppermost lower mantle in some regions in previous studies. Therefore, S splitting measurements would still be necessary to make sure that source-side anisotropy is not present. Measurements from deep events are in fact included in our analysis in section 9, so we have essentially already implemented the reviewer's suggestion.

Chapter 8
Is there a good argument after all the synthetic tests to test this on real data?
Why are ScS corrections based on S wave splitting not for source and receiver side? Would it not make more sense to do a synthetic test with a deep event (no source side anisotropy) and correct using S for receiver side anisotropy? This seems the only part of the paper that relies on using SKS.

Thanks for your comment, which has pushed us to make this part of the paper more clear. We have now added a synthetic test of explicit receiver-side corrections in the rewritten section 8 of the paper (lines 326-354) and the new Figures 12 and 13 (instead of the old Figure 12). In this process, we try to implement a test that is as realistic as possible. Because receiver-side anisotropy is usually characterized using SKS, we therefore also rely on this seismic phase in the new synthetic test.

Figure 13
The grey lines are very hard to see on the blue/white background.
Personally I would pick a different map projection. The poles with little coverage are given a lot of space here.

Thanks, we agree and now present the source-receiver configuration using a more suitable map projection. We have also chosen darker gray colors for the raypaths.

Chapter 9.2
I genuinely curious how many data is lost by each of the constraints placed by the tests in this study. As mentioned before, I think more can be gained when using deep earthquakes.

This is a good question. We now conduct and discuss a rough calculation, see lines 423-431 of the revised manuscript: "Due to the constraints that we impose in our approach to the measurement of ScS splitting, a large majority of seismograms cannot be used to reliably measure ScS splitting due to lowermost mantle anisotropy. A back-of-the-envelope calculation suggests that approximately 15 million three-component seismograms are currently publicly available for seismic events with moment magnitudes over 6.0. In this work, we obtain 130 robust ScS splitting measurements for seismic events with such moment magnitudes, using all null stations known to us (which may not be all that exist). Following this line of reasoning, under the constraints used in this study, only one out of every 100,000 seismograms is expected to yield a robust ScS splitting measurement of lowermost mantle anisotropy – a very small minority of available data.

As described above, we already include constraints from deep earthquakes in our analysis, which is why – unfortunately – we cannot add them to obtain more reliable measurements.

Lines 354-355. Authors state they can identify four regions that show at least some evidence of anisotropy. Looking at the global map, they are mainly identifying regions with lots of measurements that can be interpreted further. There is not much to say about these regions being more or less anisotropic than others.

This is a good point. We agree that we mainly find seismic anisotropy in those regions in which coverage is good and mention this now in lines 390-391 of the revised manuscript. Unfortunately, with the limited number of measurements that we have obtained, it is difficult to make any more specific statements than whether seismic anisotropy is present or not. For example, we have tried to apply the two-layer splitting technique (that we have proposed), but this was unsuccessful due to the small number of suitable measurement. We have also tried to display splitting intensities as a function of initial polarizations for well-sampled regions. However, we would need extremely good sampling (in terms of number of measurements and difference of initial polarizations) to reliably constrain the two-layer splitting case, which we have not. We discuss this point now in lines 477-487 of the revised manuscript.

We emphasize, however, that this paper is not necessarily the final word on ScS splitting globally, as discussed in section 10. There is still work to be done in the future that may well enable additional measurements and allow for the kind of analysis that we are discussing here (such as the two-layer splitting technique). Specifically, there may well be additional "null" stations that are suitable for ScS splitting analysis; future studies that are targeted towards specific regions of D" may identify additional stations that can be used to create larger datasets. Another potential area for future progress is the application of array processing techniques such as beamforming, which can average out (and thus minimize) the contributions from upper mantle anisotropy beneath the array of stations. So, the outlook for these types of measurements is not entirely negative, even given the challenges that we've articulated in this paper.

I appreciate that the authors wanted to show real data. The amount of scatter in these is a little discouraging. There are some places with good coverage along a corridor (in B and D).Is it worth showing the splitting results as a function of initial polarisation? Does this explain some of the scatter observed?

Great suggestion – we have tried, which has unfortunately not helped with the interpretation of seismic anisotropy in terms of multiple layers (see above). We mention this point now in lines 470-473 of the revised manuscript. As the reviewer suggests, the observed scatter can easily be explained by the different initial polarizations, as discussed in lines 436-439 and 466-467 of the revised manuscript. Moreover, we now discuss that the analysis of shear wave splitting for different initial polarizations can be especially helpful to find isotropic lowermost mantle regions. This discussion in lines 481-487 reads: "In our study, unfortunately, the number of well-constrained (φ, δt) measurements in any particular region is insufficient for the implementation of such an approach. As mentioned above, SI scattering is often straightforward to explain by different initial polarizations; in contrast, a precise characterization of the seismic anisotropy is challenging unless a large number of SI values for the same region can be obtained. Much easier is the detection of isotropic regions through initial polarization analysis, such as the isotropic region east of region D. The reason is that no more than a handful of null measurements with mutually different initial polarizations need to be obtained for the reliable characterization of an isotropic lowermost mantle region."

Generally, I would appreciate more discussion on how to understand the observed scatter. This seems more of interest than comparing fast axes to previous studies (that could have been affected by various issues raised in this study). Interpreting the fast axes seems like a over-interpretation after looking at the results in Figure 8.

We agree that the comparison of fast axis is not a particularly meaningful and have taken out these comparisons in all except one of the cases. We decided to leave this one comparison in the revised paper version to reinforce the point that such comparisons are quite difficult (line 466).

Line 414 -typo in 'construct'
Line 418 'will will'

Thank for catching – both are fixed.

Line 420 'ensuring that ScS polariations (approximately) agree with the backazimuth'? What is meant by this? Based on MT predictions?

Asplet et al. (2023) determined this through analysis of ScS particle motions. We now mention this in line 395 of the revised manuscript.

Sanne Cottaar

And just a plug for the book chapter by Nowacki & Cottaar (2021) which presents comparable forward modelling approach to the mineral phsics runs here and could be cited here. - > Nowacki, A., and Cottaar, S., 2021, Towards imaging flow at the base of the mantle with seismic, mineral physics and geodynamics constraints, AGU Monograph Series, pp.329-352. Preprint is available.

Thanks, we have added this citation to the manuscript. (AGU does not make this chapter very accessible, so we are particularly grateful for the heads up!)