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Reply on RC2

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Author comment on "Radial anisotropy and S-wave velocity depict the internal to external zone transition within the Variscan orogen (NW Iberia)" by Jorge Acevedo et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-116-AC2, 2022

REVIEWER #2

The paper entitled “Radial anisotropy and S-wave velocity depict the internal to external zones transition within the Variscan orogen (NW Iberia)” by Acevedo et al. conducted ambient noise tomography using recently deployed seismic arrays to constrain the velocity and radial anisotropy of the upper crust in NW Iberia. The resulting seismic image shows a good correlation with major geological domains and known structural variation in the Variscan orogen. Interestingly, the seismic model shows a clear structural transition from the hinterland and external zones of the Variscan orogeny. This radial anisotropy model provides new seismic constraints to the study region and adds knowledge to the deformation processes in orogens. The topic is a good fit for the journal of Solid Earth. The manuscript is well structured and is generally well written. I think that this manuscript is suitable for publication after some minor revisions. I summarize my main concerns below, which are about the resolution analysis and the interpretation of anisotropic structures in regions with a suboptimal resolution, and hope these are helpful to further clarify some points and strengthen the paper.

1.- The actual inversion of group velocity used a grid size of 0.1 degree. In the checkerboard test shown in the supplementary material, the size of the grid to construct the anomaly seems to be quite big. Was the checkerboard test also using 0.1 gird, or the inversion gird was set to the same size as the anomaly? This needs some clarifications.

All the chequerboard resolution tests were performed using the same grid size (0.1º x 0.1º) and regularization parameters (damping = 0.001, smoothing = 0.1) of the group velocity inversions, in order to ensure the representativeness of the tests. The grid size and the inversion parameters do not change with the cell size of the initial chequerboard model. This information has now been added to the captions of Fig. S3 and Fig. S4.

2.- Shear velocity structures of Vsh and Vsv are inverted separately. The results look reasonable, but could the authors elaborate on how the inversion parameters were properly chosen to ensure the same degree of amplitude recovery between the two models? In other words, how could you make sure that the velocities obtained from two separate inversions are comparable between each other?

This is a good point because it is true that the sensitivity of Love waves decays more rapidly with depth than Rayleigh waves, and that can affect the resulting shear wave velocities. We are aware that some studies try to compensate the decrease on Love wave group velocity sensitivities by varying the inversion parameters at depth (e.g. Wang et al., 2020). However, the analysis of the Rayleigh and Love sensitivity kernels (Fig. S5) shows that, at the depth range that has been investigated in this study (1-12 km), Love and Rayleigh sensitivities are comparable, ensuring the extraction of comparable Vsh and Vsv velocities. For that reason, we have maintained identical inversion parameters for Vsv and Vsh velocities, like other radial anisotropy studies (e.g. Lynner et al., 2018).

3.- The resolution near the edges of the imaging area is really not ideal, and structures there may not be well constrained by the data. Therefore, I am a bit worried about the interpretation of small-scale anomalies in these regions of suboptimal resolution. For example, on lines 414-415, a deeper transition depth of anisotropic structures beneath the CIZ-GTOMZ is used as an argument for the presence of a basal detachment fault. This is a good observation, but I feel that this may run into the risk of over-interpreting structures that are not well constrained by the data. Similarly, on lines 420-424, the resolution in CIZ and GTOMZ are relatively low, as also acknowledged by the authors, yet detailed interpretations are given here. Unless the authors can substantiate the robustness of these structures, I would suggest minimizing the discussion of structures with suboptimal resolution.

As reviewer #2 has pointed out, the resolution in the edges of the models is reduced due to the lack of interstation paths in those areas. As suggested, we have minimized the discussion about structures and anomalies in regions that were not well resolved, such as the CIZ-GTOMZ domain. For example, the interpretation of the transition between anisotropic structures as the result of the presence of a basal detachment can be supported with observations from the CZ, the best resolved area in our models (lines 440-441). The discussion about the CIZ-GTOMZ area has also been reduced and we now clearly state that the resolution in the area is not optimal and further investigation is recommended (lines 445-453).

4.- In figure 6, I suggest using the same color range when plotting the two models. It is difficult to compare them.

The suggestion is interesting, and we have created a figure depicting both models with the same color range (Fig. R3). The main feature of the models, which is the velocity variation in the external-internal zones transition, is still visible. However, Love wave velocities are higher than Rayleigh wave velocities, and this results in a general color range that is too wide to display some velocity anomalies in the Vsv maps, specially at higher periods. Considering that these velocity variations may have important implications from a geological perspective, we believe that using two different color ranges for the Vsv and the Vsh models renders more information to the reader. Nonetheless, we have modified slightly the Vsv and the Vsh color scales. The previous ones were constructed by extracting the minimum and the maximum velocity values within all the inverted slices. Now, we have only considered the depth slices depicted in Fig. 6 to select these values, enhancing the visibility of the high velocity anomalies in the Vsv – 12 km map.

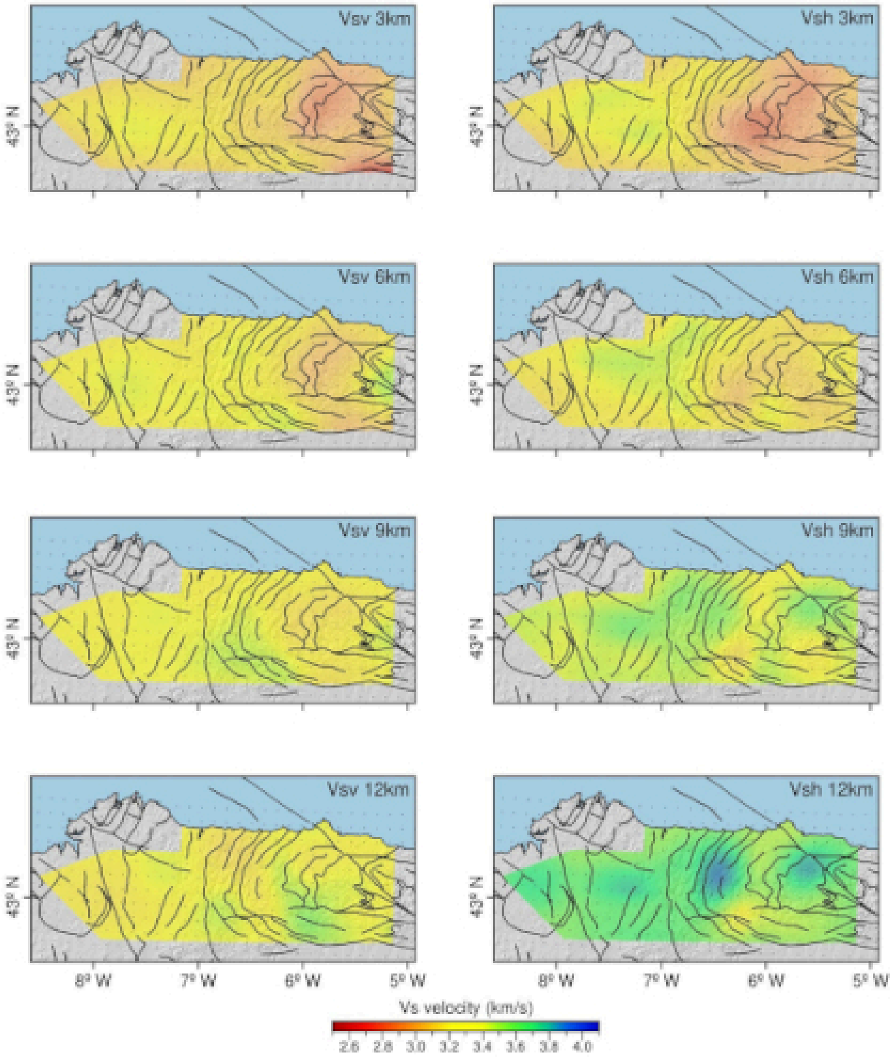

Figure R3. *Inverted Vsv (left panels) and Vsh (right panels) tomographic maps for depths of 3, 6, 9 and 12 km. A common color scale has been used in all the maps.*

5.- Figure 8, please label geographic locations such as CCB, NA, Allande and Vivero faults on cross-sections. Also, the top 1 km of the model is not shown, any reason for this?

The mentioned geographical locations and structures have been labelled in Fig. 8. Moreover, the trace at depth of the labelled faults has been represented in the cross-sections. In many ambient noise-based studies, the top kilometer of the models is not represented due to the difficulty of extracting dispersion measurements at low periods (< 2s in our case). The absence of high-frequency measurements leads to a lack of information in the shallowest part of the models that compromises their reliability near the surface.

I also have some minor suggestions referring to the line number.

6.- Line 18, "orogenic grain" -> "orogenic belt"

The word has been changed

7.- The same line, "bulk properties of the rocks" sounds like the bulk composition of the rocks. Please consider using antherword such as "elastic properties of the rocks".

The word has been changed, elastic properties is more accurate.

8.- Line 24, “caused by” -> “which we attribute to”

Corrected

9.- Line 25, ‘the internal deformation of rocks either during the Variscan orogeny or prior to it’ -> “the pre- or syn-orogenic deformation associated with the Variscan orogenesis”.

Corrected

10.- Line 31, “shear waves” -> “shear wave velocities”

Corrected

11.- Line 37, “whose importance varies with depth” -> “that dominate different depth levels”

The sentence has been changed

12.- Line 42, “depth” -> “depths”

Corrected

13.- Line 45, “the features that govern the” -> “the governing features”

Corrected

14.- Line 50, “been” -> “been identified”.

Corrected

15.- Line 51, “ancient orogenic belts (Wang et al., 2020)”. Although there are some disputes on the age of the initiation of Cordilleran orogenesis, it is certainly a Phanerozoic orogeny and is likely as old as the Variscan orogeny (Paleozoic). So I would not use the word “ancient”, which more properly refer to orogenesis in Precambrian.

Many thanks for the clarification, it is true that the term “ancient” suggests a Precambrian origin. We have removed the word.

16.- Line 63, “an orogenic system, in the West, to the external zone, to the East” -> “an orogenic system (west) to the external zone (east)”.

Corrected

17.- Line 64, “in the area” -> “in this area”.

Corrected

18.- Line 67, “helped to broaden” -> “broadened”.

Corrected

19.- Line 68, “In order to increase the resolution in the structure of the crust” -> “To improve imaging resolution at crustal depths”.

Corrected

20.- Line 74, “for the unraveling of” -> “for unraveling”

Corrected

21.- Line 177, “that are mostly made of” -> “that they are mostly made of”.

Corrected

22.- Line 186, “It is only in the first of the domains defined, in the CZ, that it has been reported a …”, this can be simplified, “Earlier studies have been reported in the CZ domain a”

Corrected

23.- Line 194, ’11 stations’ -> “Among them, 11 stations”.

The entire section 3.1 (Seismic data) has been rearranged to make it clearer. This suggestion was useful to explicit that the GEOSN was composed by 11 portable stations, but we also have access to the data acquired by two permanent stations in the area. We have tried to explain this fact in a better way in the new text (please see Reviewer #1, point 11 for more details).

24.- Line 208, “26% of overlap” -> “26% overlap”.

Corrected

25.- Line 214 “with corner frequencies between 0.01-2.0 Hz”-> “with corner frequencies of 0.01 and 2.0 Hz”.

Corrected

26.- Line 292 “keep delineating a large high” -> “delineate a consistent large high”

Corrected

27.- Line 315, “higher depths” -> “greater depths”

Corrected

28.- Line 354, “element of our models, both the surface- and the shear-wave ones, is” -> “element in both group and shear velocity models is”

Corrected

29.- Line 390, highlight the velocity contour of 3.1 km/sec using a thick line or another color.

The 3.1 km/s velocity contour has been highlighted in grey in Fig. 8a. The figure caption has been changed accordingly.

30.- Line 455, citation format issue, remove the extra comma, “Chen et al. (2009) and Guo et al. (2012)”

Corrected

REFERENCES

Lynner, C., Beck, S. L., Zandt, G., Porritt, R. W., Lin, F. C. and Eilon, Z. C.: Midcrustal deformation in the Central Andes constrained by radial anisotropy, J. Geophys. Res.-Sol. Ea., 123(6), 4798-4813, https://doi.org/10.1029/2017JB014936, 2018.

Wang, J., Gu, Y. J. and Chen, Y.: Shear velocity and radial anisotropy beneath southwestern Canada: Evidence for crustal extension and thick□skinned tectonics, J. Geophys. Res. - Sol. Ea., 125(2), e2019JB018310, https://doi.org/10.1029/2019JB018310, 2020.