Solid Earth Discussions

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Reply on RC1

Jorge Acevedo et al.

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-AC1, 2022

REVIEWER #1

This study presents results of an ambient seismic noise imaging in the northwest of Iberia. Based on the higher observed discrepancy between the Rayleigh and Love-wave group velocity and corresponding shear velocity models, a radial anisotropic model is also presented. The manuscript ends with discussion trying to interpret both the isotropic shear velocity model and the observed radial anisotropy in details and in relation with internal and external structures of the area. The manuscript is well written and structured in which careful prepossessing of data and selecting cross-correlations and dispersion measurements has been done. In general, publication of the manuscript is recommended after revision according to the following points and comments.

Major comments:

1.- It's not clear that from which inter-component the Rayleigh-wave dispersions were obtained. Only from the ZZ? or RR? or both? as shown in Figure 3. In Figure 4, the Rayleigh group velocities out of ZZ are shown. How different or similar are the Rayleigh group velocities out of the RR, and in comparison with the Love-wave velocities? Would it result in the similar Rayleigh Love wave discrepancy as the ZZ? In this study, ambient noise data from 3-components seismic stations was used. It seems the data quality of the horizontal components were good enough to reconstruct the Love-wave velocities (from TT). I am curious why the authors haven't used the great opportunity to benefit all of the possible inter-components containing the Rayleigh-wave, which are ZZ, RZ, ZR, and RR. Therefore, my recommendation (unless there's been a serious issue with the data!) is to use all of these four inter-components cross-correlations and to obtain the dispersions of Rayleigh-wave as a production of these four inter-components (e.g a production by logarithmic stacking in the period–group velocity domain introduced by Campillo et al., 1996; see for instance Zigone et al., 2015). This would provide more reliable group velocity of the Rayleigh-wave, and therefore to improve the reliability of the Rayleigh-Love discrepancy.

The Rayleigh-wave dispersion measurements that were used in this study were extracted exclusively from vertical-component (ZZ) cross-correlations. Rayleigh group velocities from radial components (RR) were also calculated, and their average dispersion curve seems quite similar to the ZZ average (Fig. R1):

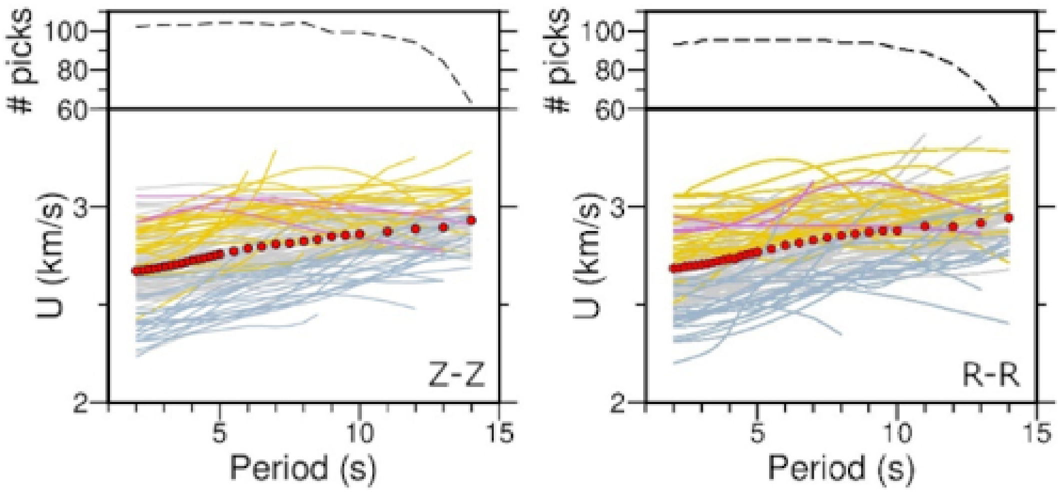

Figure R1. *Rayleigh-wave group velocity dispersion curves obtained from vertical (left panel, ZZ) and radial (right panel, RR) component cross-correlations. Top panels show the number of velocity determinations as a function of the period.*

However, we decided not to include dispersion measurements from RR cross-correlations in further processing steps due to three reasons:

- *RR derived velocities display higher uncertainty than ZZ velocities. The stability and error analysis of the dispersion curves in Fig. S2 (supplement) shows that RR measurements exhibit uncertainties up to 4%, while ZZ uncertainties are well below 1%. Consequently, this is also noticeable in Fig. R1. In that figure, it can be seen that the range of group velocities is wider for the R-R dispersion measurements, specially at longer periods (> 8s).*
- *The signal-to-noise ratio (SNR) of the ZZ cross-correlations is, on average, 3 times higher than the RR cross-correlations SNR.*
- *Some RR dispersion curves cannot be calculated due to low-quality empirical Green's functions or higher-mode contamination. This fact leads to a lower number of group velocity estimations (Fig.R2) and the loss of some important interstation paths.*

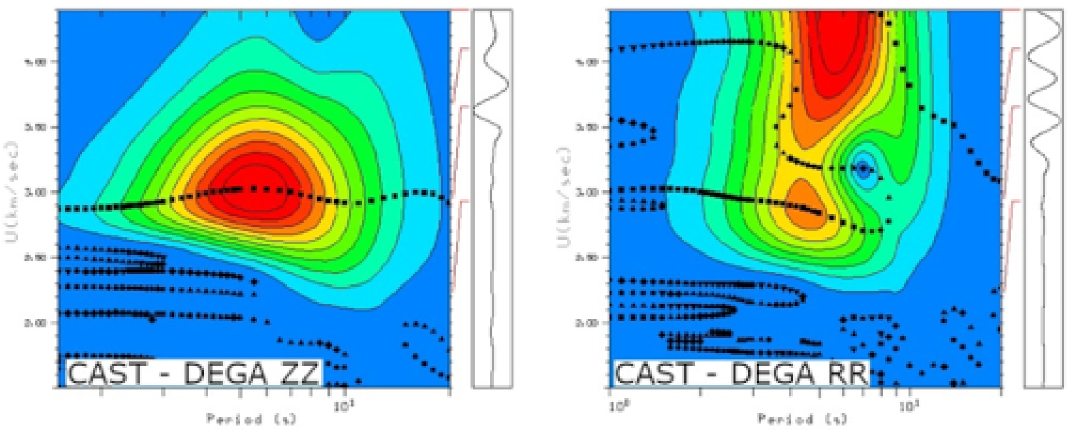

Figure R2. *Vertical (left panel) and radial (right panel) component MFA surfaces derived from the same inter-station path (CAST-DEGA). Note the lower quality of the radial MFA surface.*

Considering these three points, we believe that adding Rayleigh-wave group velocities from RR, RZ or ZR cross-correlations to the calculations is not likely to benefit or enhance the reliability of the results. On the contrary, it will mean to include higher uncertainty *data in our processing. As stated in Campillo et al., (1996), the logarithmic stacking of the ZZ, RZ, ZR and RR inter-components is useful when the quality of the seismic dataset makes difficult to define dispersion curves with confidence. In our case, we demonstrated that the ZZ dispersion measurements and the subsequent results are reliable. In fact, most of the radial anisotropy studies use Rayleigh dispersion measurements obtained only from ZZ cross-correlations (e.g., Shrizard et al., 2017, Dreiling et al., 2018; Movaghari et al., 2021; Alder et al., 2021).*

We have added a few lines in the manuscript (265-268) to clarify that Rayleigh wave dispersion measurements come from ZZ cross-correlations and briefly commenting the reasons why RR cross-correlations were discarded. We have also included RR examples to Fig. S2 in the supplemental material.

2.- The authors have mentioned the significance of the azimuthal anisotropy resulted in their previous study (Acevedo et al., 2020). Anisotropic structures in general might not be well explained neither by radial anisotropy alone nor by azimuthal anisotropy. Since results of azimuthal anisotropy (fast orientation and delay time) in the study area is present, and also in order to have a better picture of the anisotropic structures, both radial and azimuthal anisotropy should be discussed. More particularly, the distribution of fast-polarization orientation over depth (at each period) in comparison with Radial anisotropy could be discussed in terms of deformation history and regional tectonic regimes of the area associated with late Variscan shear zone and /or Alpine convergence. However, there is little discussion in this matter (line 434-435). I therefore suggest including the depth variation of the fast orientations superimposed on the radial anisotropy pattern at each depth (Figure 7) and add more discussion based on such comparison. The author could try to address questions, for instance, how the fast-polarization orientation varies from surface to depth while radial anisotropy is increasing? Is there a relation between radial and azimuthal anisotropy at the region where there is a change from negative to positive RA (contrasts observed under GTOMZ, CIZ and CZ; line 421-422)? In lines 463-465 as the authors discussed the cause of positive radial anisotropy by the horizontally sheared fabrics, here the depth pattern of fast orientations could help in better understanding such effect.

We agree that a more direct comparison between radial and azimuthal anisotropy will help to better understand the anisotropic structure because, as you have rightly pointed out, it might not be well explained neither by radial nor by azimuthal anisotropy alone. We have rewritten and extended the discussion on this matter (from lines 456 to 492), and we have added a new figure (Fig. 9) that compares directly the radial and the azimuthal anisotropy magnitude (Acevedo et al., 2020) in the CZ and shows how fast azimuthal directions vary with depth. In addition, a panel showing the variation of the seismic anisotropy with lithostatic pressure measured in rocks from NW Spain (Brown et al., 2009) is included.

The new discussion tries to answer the interesting questions raised but there are some limitations that are inherent to the techniques used in Acevedo et al., (2020). On the one hand, seismic anisotropy derived from shear wave splitting represents the average anisotropy in the layer causing it, but it is not constrained at depth. On the other hand, ambient noise-based azimuthal anisotropy measurements are dependent on their period, and they can be associated to their approximate depth by analizing the sensitivity kernels, but they are only representative of the CZ region. That is why we can only compare directly radial and azimuthal anisotropy in the CZ. Nevertheless, some striking features can be observed. The minimum magnitude of both the radial and the azimuthal anisotropy coincides with the theoretical depth at which cracks are closed by the lithostatic pressure (we have used the laboratory data in Brown et al., (2009) because those rocks are from near the study area, but in fact, the effect of crack closure is commonly observed in *crystalline rocks at increasing confining pressures). This also coincides with the depth of the transition between the Alpine-reworked Variscan cover and the pre-Variscan basement. The shift is also reflected in the pattern at depth of the fast-azimuthal directions, and it has important implications in terms of the processes that control the anisotropy. These observations are now described in the discussion section.*

Other comments:

Introduction:

3.- Line 71; "The ANI reflects the variation of the seismic velocities of the bulk rock …" seems not a correct statement. Other passive seismic imaging techniques that use earthquake data can also provide images of velocity variation of subsurface structures.

The sentence has been changed to clarify that other passive seismic imaging techniques can provide information about the variation of seismic wave velocities.

4.- Line 72; "…, which is controlled by their elastic parameters". How about temperature? Please modify or ignore this statement.

The incorrect statement has been removed.

5.- Line 73-75; Earthquake-based tomography can also provide images of upper crustal structure. A main advantage of ANI is data availability, particularly in regions with low seismicity -where not enough regional/local earthquake occurring- and also as an alternative to high cost active survey.

Lines 79 and 82-83 have been modified to explicit the mentioned advantages of the ANI method.

6.- Please provide an earlier study as reference in line 27-28 (e.g. Silver, 1996). The same in line 29; here you could use e.g. Mainprice et al., 2000.

Both studies have been added to the manuscript.

7.- Line 30; please remove Shapiro et al., 2004 as it's not a proper reference for anisotropy.

The reference has been removed

8.- Line 61; please provide reference for "Overall, the part of the Variscan belt that crops out in the Cantabrian Mountains (CM) represents one of the most complete sections of this orogen in Europe, …"

The assertion was extracted from a chapter of the book: Spanish Geological Frameworks and Geosites: An approach to Spanish Geological Heritage of International Relevance, edited by García-Cortés, A. The corresponding reference has been added to the manuscript (line 66).

Geological setting:

9.- Line 135; "Overall, the part of the Variscan Belt that crops out in the CM represents one of the most complete sections of this orogen in Europe …" This is a repeated sentence. Please remove it.

The repeated sentence has been removed.

10.- Section 2.3 seems a bit long and include a lot of geology. Particularly, explaining the four distinguished domains from line 156 till line 179; Are all of these are necessary for the discussion? Could you make it shorter?

A few lines have been removed from section 2.3 to make it more concise. However, we think that it is a brief geological summary that helps to interpret the results from the tomographic maps and the radial anisotropy.

Data and Method:

11.- It's not clear how many stations in total were used. 13 temporary (portable) but two of them were permanent?! In case they were all temporary, you may remove the word "permanent" in line 196.

The GEOCSN network was composed of 11 portable stations, but we also had access to the data that was simultaneously acquired by two permanent stations of the Spanish Seismological Network (SSN). That is the reason why we said that the network had 13 stations, 11 temporary stations plus 2 permanent stations. Please see next point for more details.

12.- Have you used all stations between June 2019 and February 2020? I suggest making the Sec 3.1 more clear and including information about how many stations in total, from which network in details, and in which the time period the continues seismic recordings were used.

The GEOCSN network was active between June 2019 and February 2020. As we explained in the previous point, it was formed by 11 portable stations, and it was complemented with data from two permanent stations of the SSN. The other experiment that provided data for this study was the IberArray seismic network. It was active between 2011 and 2013, but we have processed only 12 months of data from that network (year 2012) because the temporary stations had different installation and removal dates, but they were all active during the year 2012. In total, we used data from 7 portable stations of this network. Like in the case of the GEOCSN network, the IberArray dataset was augmented with seismic data from one station of the SSN. We have rearranged Section 3.1 and we have added some explanations to make these points clearer. We have also included the position of the seismic stations in Fig. 1 (see point 20).

13.- How you assessed any seasonal effect in the cross-correlation functions? Perhaps you could assess it in a way in Figure S1, and to add a sentence about it to the text.

Seasonal effects are the result of the variation in the position and intensity of the noise sources. Theoretically, noise sources must be fully equipartitioned to obtain reliable dispersion measurements, but this requirement is not usually fulfilled. The f-k analysis performed in the study area by Olivar-Castaño et al., (2020) demonstrated that, although the most intense seismic sources are located to the N and the NW of the area, a sufficient level of noise arises from all azimuths. Nevertheless, the stacking of noise-cross correlations over long time spans, as we do in this study, diminishes the impact of potential seasonal effects in the measurements (Corela et al., 2017). Anyway, the non-stationary/non-isotropic nature of the seismic noise wavefield does not alter substantially the measured velocities of surface waves and is not a critical issue (e.g., Froment et al., 2010). A brief paragraph on this matter was added to Section 3.2 (lines 269-273).

14.- Please provide more example of inter-station dispersion curve (as in Figure 2d) in Supplementary material.

A new figure showing more inter-station dispersion curves has been included in the

Supplemental material (Fig. S1).

15.- I could not quite understand "… defining a constant S-wave velocity…" in line 272. Please make in a clearer way.

The statement has been changed to make it clearer. We tried to express that we used and initial model with a constant S-wave velocity of 3.35 km/s.

Discussion:

16.- In line 353, "…both the surface and shear-wave ones…", do you mean both the surface-wave group and shear velocities?

That is right, the sentence has been corrected.

17.- What does the "RA style" refer to? in line 404 and in a couple of other lines. It's confusing.

The anisotropy style referred to its sign: positive or negative. We now use the term "sign" instead of "style" in the text.

18.- How did you obtain the angels 60-90 and 0-30 degree for dipping features (line 406)? Are these numbers estimated from the shape of the positive and negative anomalies in the cross-section (Figure 8c)? If so, I recommend avoiding giving quantity of the dipping structures because such image can differ by changing smoothing parameters of the inversion or even in plotting.

The mentioned angles are not a result from our study. In fact, those values were firstly proposed by Xie et al. (2013) and they were obtained from laboratory tests in rock samples. To our knowledge, they are still valid, and they have been featured in recent radial anisotropy studies (e.g. Dreilling et al., 2018).

Conclusion

19.- Line 472-474; "The observed discrepancy between the measurements from Rayleigh and Love waves impeded the possibility of performing a joint inversion …" not really clear what you mean. It is possible to perform joint inversion using the dispersion measurements from Rayleigh and Love to derive both isotropic and anisotropic Vs models. It's fine that joint inversion was not the purpose of this study. But it would not be the reason that why you have used Rayleigh and Love-based separately derived Vs model to infer Radial anisotropy. Please rephrase the sentence or might move it to the end of the conclusion and as suggestion for a later study.

The sentence has been removed, because it is completely right that it is possible to perform a joint inversion.

Figures

Figure2:

20.- Even though the Fig2a with its colors related to Fig2e and 2f is quite helpful, it is too small for representing the station location and geometry. Please provide a larger figure showing all the station's location with different symbol color for each network. Such a figure could be added to Figure 1.

Seismic stations have been represented in Fig. 1. Symbol colors indicate the seismic *network at which they belong, and stations names are depicted.*

Figure3:

21.- With regard to my comment No.1, the ZR and RZ Cross-correlations could to be shown in fig.3.

Please see point number 1.

Suggested references:

- Campillo, M., Singh, S., Shapiro, N., Pacheco, J., and Herrmann, R.: Crustal structure South of Mexican Volcanic belt based on group velocity dispersion, Geofis. Int., 35, 361–370, https://doi.org/10.1016/j.crte.2011.07.007, 1996.
- Mainprice, D., G. Barruol, and W. Ben Ismail (2000), The seismic anisotropy of the Earth's mantle: From single crystal to polycrystal, in Composition, Structure and Dynamics of the Lithosphere Asthenosphere System, Geophys. Monogr. Ser., vol. 117, edited by S. Karato, A. Forte, R. Liebermann, G. Masters, and L. Stixrude, p. 237, AGU.
- Silver, P. (1996), Seismic anisotropy beneath the continents: probing the depths of geology, Annu. Rev. Earth Planet. Sci., 24, 385, 432.
- Zigone, D., Ben-Zion, Y., Campillo, M., and Roux, P.: Seismic Tomography of the Southern California Plate Boundary Region from Noise-Based Rayleigh and Love Waves, Pure Appl. Geophys., 172, 1007–1032.

Most of the suggested references are now cited in the manuscript.