Answers to reviewer comments #1

Title: 3D crustal structure of the Ligurian Sea revealed by ambient noise tomography using ocean bottom seismometer data

Dear Reviewer,

We thank you for the fair, detailed and constructive review of our manuscript se-2021-55. Please find below our line-by-line-revision where we carefully respond to all your comments and suggestions and explain how we modified the manuscript.
The comments from the reviews are indicated in **black** and our replies in **red** colour.

Scientific questions and comments
The title suggests only ambient noise and OBS data are used. I suggest changing this to be more generic due to the extensive role of the teleseisms and also acknowledge the land-based station. The Alpine region is also interpreted so should be included with the Ligurian Sea.
We changed the title accordingly by adding the words "surface wave tomography" which includes both, the ambient noise and the teleseismic cross-correlations. We stick to "Ligurian Sea" because we see this as the focus of the manuscript. Also, we did not change "ocean bottom seismometer", because we want to emphasize the offshore part of the dataset. The land stations are clearly marked and acknowledged in the manuscript, but the vast majority of new conclusions of this paper is resulting from the use of OBS (ocean bottom seismometer) data.

Abstract

Line 14 – State what velocity models used
We updated this sentence to prevent misunderstandings on the work done. We did not invert velocity models, but performed surface wave tomographies.
Old: "We invert velocity models using an amphibious network [...]"
New: "*We perform ambient noise tomography, also taking into account teleseismic events, using an amphibious network [...]*"

Introduction – Add a review of the current knowledge of crustal thickness and sediment thickness. This then gives context to the interpretation later.
We added a short paragraph on the sediment thickness and a longer one about the knowledge of the crustal thickness.
New: "*The marine bedrock is covered by a sediment layer (e.g. Schettino and Turco, 2006) of varying thickness. It is less than 3 km thick near the Tuscany coast, increases towards the southwest, and reaches a thickness of up to 8 km offshore Marseille.*"

Line 40 – Be more specific than narrow and steep.
We specified: "*The continental margin is narrow (10-20 km) and steep at the Ligurian coast (Finetti et al., 2005) and broader (20-50 km) on the Corsican side (e.g. Rollet et al., 2002).*"

Methods

Lines 76-78 - 22 OBS/BBOBS are plotted in Figure 1 which I assume are the 22 with complete datasets. Given 28 were recovered, please comment on why the other 6 were not used. Potentially plot all the locations on Figure 1 but identify which were not used in this study and which were not recovered to see the full extent of the experiment.

We use all stations for which all three seismometer components and the hydrophone component worked correctly during the complete deployment. This is true for the 22 OBS shown. Showing stations that were not used (no data or not enough data was recorded) would, in our opinion, not increase the clarity of the figure. Therefore, the corresponding numbers in Section "Data" were updated.

Line 104 – Given ambient noise and ambient noise tomography have been obtained for studies going back to 2007, the statement saying there is "no established routine for ambient noise analysis" seems contradictory. For example, the RHUM-RUM, PI-LAB and VoiLA experiments all produced ANT results. Even on land, multiple processing techniques are applied. Suggest removing and focus on the fact OBS data require tilt and compliance removal as a crucial processing step and should be performed in future.

We changed "there is no established routine" to "it is not yet used regularly".

Lines 134-135 – Please explain why only select land-ocean pairs were used rather than all possible combinations. Did using the full dataset bias the directionality of the ray paths?

Of course, we use all land-ocean pairs. To clarify this, we deleted the sentence and rephrased the paragraph to clarify that we correlated all OBS-OBS and all OBS-land stairs. The selection of additional land-land pairs was made to increase the ray coverage in the Ligurian basin.

Old version: "We use the tilt- and compliance-corrected daily files to estimate cross-correlation functions (CCF) for every vertical component OBS-OBS station pair (Bensen et al., 2007). These CCFs are complemented by CCFs for selected OBS-land station pairs and land-land pairs to obtain cross-correlations in other ray directions than those of the OBS-land pairs (Fig. 2). Additionally, we calculate CCFS for all land-land pairs for the land stations A317A, ARBF, and DIX (see Fig. 1) and CCFs for all combinations of 20 selected land stations (namely AJAC, BLAF, BOB, BSTF, CALF, CARD, EILF, ENR, GBOS, IMI, ISO, MSSA, PCP, PLMA, ROTM, SAOF SMPL, TRBF, TURF, and VLC) to increase the resolution onshore. "

New: "*We use the tilt- and compliance-corrected daily files to estimate cross-correlation functions (CCF) for every vertical component OBS-OBS and OBS-land station pair (Bensen et al., 2007). Additionally, we calculate CCFS for all land-land pairs for the land stations A317A, ARBF, and DIX (see Fig. 1) and CCFs for all combinations of 20 selected land stations (namely AJAC, BLAF, BOB, BSTF, CALF, CARD, EILF, ENR, GBOS, IMI, ISO, MSSA, PCP, PLMA, ROTM, SAOF SMPL, TRBF, TURF, and VLC) to increase the ray coverage onshore (Fig. 2).*"

Changing Figure 2 to a hit count for the ray paths would show this visually.

We updated Figure 2 accordingly.

Line 189 – Given there is no overlap between the ambient noise and teleseisms, how confident can you be that the dispersion curves are complementary? Particularly for the ambient noise OBS-OBS pairs, they don't look complimentary in Figure 5a (b and c are good). Studies have also suggested that a $\pi/4$ phase shift is required when combining ambient noise and teleseisms (e.g., Boschi et al., 2013; Yao et al., 2006; Tsai, 2009; Tsai, 2010). Do you require this here?

The OBS-OBS dispersions not looking that complimentary might be caused by the comparably low number of pairs; a great portion of the dispersion curves do look complementary, but 'outliers' are more visible due to the low number of curves.

The mentioned phase-shift applies to investigations of phase velocity (e.g. the mentioned Boschi et al, 2013). Since we derive group-velocities from cross-correlation functions in the time-domain, we do not require a phase shift for teleseismic cross-correlations.

Please comment on the criteria used in order to keep a station-station cross correlation pair. Was there a minimum signal to noise ratio required, or a certain number of days in a stack required?

We manually picked the dispersion curves. Station pairs, where no correlation maximum was detectable in the MFT plot, were excluded. To clarify, we added a sentence in line 194: "*Station pairs showing no detectable maximum were excluded.*"

Line 207 – What is the spacing of the new refined grid. added
 Line 208 – Why remove 11, 13 and 14s period?

We did not "remove" 11 s, 13 s, and 14 s period, but decided to follow common practice and apply less sampling at increasing periods, as we also do for periods > 15 s. We did not modify the manuscript.

 Line 219 – What size were the checkers and for which periods? Minimum and maximum period for both methods should be shown to give an idea of the resolution across all periods.

We added the periods and sizes to the caption of Figure 7 (checkerboard tests). We also added more checkerboard tests to the supplementary material (Fig. S4-S7).

Line 226 – Specify exact depth range for PREM (>4km depth?) and the same for Dannowski et al. 2020 (Surface – 4 km depth?).

We added the missing depth range.

Old: "[...] we set up a starting model with fixed layers (Table S4) that is based on PREM (Dziewonski and Anderson, 1981) at depth and on v_P-velocities from Dannowski et al. (2020) for the shallow layers."

New: "*[...] we set up a starting model with fixed layers (Table S4) based on the v_P-velocities from Dannowski et al. (2020) for up to 16 km depth and on PREM (Dziewonski and Anderson, 1981) below.*"

The authors suggest shallow water depth and seasonal variations for the OBS may have negatively impacted the quality of the CCFs. Did the authors correct for the seasonal component or was this not possible given the short deployment? Did you look at the noise quality and whether there is a clear variation between the quality of deeper OBS and those shallower?

We looked into the data and observed more days with high noise levels during the later autumn and winter months. The quantitative evaluation of the influence of the seasonal signal on the CCFs would be out of the scope of this paper. Therefore, we changed the sentence "and the highly variable weather conditions of our research area" to "*and probably also the highly variable weather conditions of our research area*".

A seasonal effect was not removed, due to the deployment time of only 8 months. The seasonal component can be removed more easily for data sets that are longer than a year.

Adding more detail for the checkerboard tests and further resolution tests at the end of the data resolution section is required. Moving some of the checkerboard details from the results into this section would also improve clarity. Add synthetic resolution tests to see if the basin structure and sediment thickness can be recovered. Depth resolution tests are required such as spike tests and checkerboard tests.

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We added details on the checkerboard tests here. We increased the number of tests and varied the grid size. Also, we calculated a restoration test for v_G- and v_S-maps to increase the understanding of the resolvable areas.

Results

The results section and interpretation are confusing. Currently the results are focussed on some error analysis and an overview and interpretation of the group velocities. The interpretations section then does the same for the shear velocities. I suggest changing this section to include the results from the 1-D dispersion curves, group velocities and finally shear velocities and move all interpretation to the next section.

In this section state what periods you are going to interpret for the group velocity maps (e.g. 5– 40s) and also for the shear velocity maps. It would also be useful near the start of the results to give a broad overview of the range in velocity for the region and for the minimum and maximum period/depth.

Thank you for this feedback. We carefully reorganized the results and interpretation sections. Please see the track changes version for the in-depth revisions of both sections.

The authors suggest the resolvable area has been chosen based on the checkerboard tests. However, for 40 s period the recovered checkerboard is much smaller than the area shown. Line 264 suggests one polygon is drawn for all periods, however this area seems only relevant for the shortest periods (5-8s) and is not appropriate for any of the teleseismic periods (>20s). I suggest the authors produces further resolution tests at a larger range of periods including the minimum and maximum periods used. Also state the size of the checkers (it looks like +- 1 degree) and show other tests with increased checkers for longer periods, as currently 40 s is not well resolved. With the current tests I cannot tell whether there is resolution for teleseisms at the shorter periods (e.g. 20s). This should also move to the previous section.

We increased the number of resolution tests and information in the manuscript. We updated the checkerboard tests (CB) in section 4.1 (now 5 s, 15 s, 20 s, 90 s). Checkerboards for all other periods are calculated as well and are shown in the supplement. We added CBs with a finer grid size (0.4°x0.4° in addition to 0.8°x0.8°) to give more information about our resolution for small scale anomalies. Additionally, we now include a restoration test based on synthetic data (Figures in the Supplement). We also added a section on resolution tests to the previous section on data quality (3.4).

Line 280 – Please comment on the crustal thickness of the area in the introduction and give the reader context on what depths the 20 and 40 s period are sensitive to.

We added a paragraph on the crustal thickness to the introduction. The discussion on sensitivity was added and moved to the discussion.

New (Section 5.2): "*This hints at less heterogeneities in the mantle, but might also be caused by the decreasing sensitivity of group velocities with increasing period (e.g. Adimah & Padhy, 2020). For a period of 5 s, the group velocity is sensitive to a narrow depth range that peaks at ~5 km. For 20 s, the overall sensitivity is lower and has a much broader range of approximately 10-25 km depth.*"

Line 289 – Again give the reader information about the crustal thickness for the area. This is also a bold statement. How have you ruled out compositional variations, temperature and grain size? This is also interpretation so should probably go later in the manuscript.

This sentence was deleted while updating the manuscript.

Variations in the group and shear velocity are smaller than the checkerboard tests and alternate from slow to high velocity laterally. This may suggest the grid is too fine scaled and producing artefacts. Checkerboards at a finer scale would help determine if these are real features.

Finer checkerboards were added. We tested different grid sizes and used the one with the lowest overall RMS. Artefacts do appear in the north of our research area (fast area "fingers") and between Corsica and the Italian coast. Both are caused by the ray coverage. The "fingers" offshore Nice are discussed in Sect. 5.2.

Interpretation

Lines 310-311 – This sentence currently reads as though the Group and shear velocities show different things. I don't think you mean this.

Indeed, we do not mean that. We rephrased this sentence to increase clarity.

Old: "*We calculated 1D depth inversions for v_S based on the group velocity maps described above. In the following, we discuss four distinct regions that show different characteristics in the shear velocity maps.*"

New: "*In the following, we discuss three regions that show differing characteristics in the velocity maps: [...]*"

Line 312 – Does the average RMS vary with depth? Given much of the discussion is focussed on Moho depths it would be useful to see how RMS varies with depth with it likely to be better at shallow depths and worse at greater depths aligned with the Moho. Showing the resolution matrix would be useful to assess the resolution with depth.

The inversion outputs one RMS value foreach inverted 1D profile. We now show the RMS values of the 1D inversion in mapview (Figure 8, panel i).

Lines 314-317 – Move to results section. Done.

Line 318 – Too strong to say it is definitely the geology at this point. I agree it is most likely variations in the geology and composition but you need to prove it. Suggest your hypothesis then build up the evidence with where the sediments are located and the more mafic compositions. Can you rule out temperature effects, grain size and fluids for your study?

We are well aware that changes in temperature, grain size and fluids affect the velocity. Nevertheless, the variations in geology include changes of these parameters and we did not try to estimate the effect of each one individually. In order to clarify:

New: "*In up to 9 km depth (Fig. 8a-c), we observe laterally varying shear-wave velocities on land that we assume to be caused by variations in the geology. At the Rhône delta (Fig. 1), where the sediment cover is up to 12 km thick (Pichon et al., 2010), we observe $v_S \cong 2.7$ km s^{-1} in the 4-6 km layer (Fig. 8b) and $v_S \cong 3$ km s^{-1} in 6-9 km depth (Fig. 8c). Similarly, the Po plain has an average sedimentary cover of 7-8 km (Molinari et al., 2015a) with a shear-wave velocity increasing from $v_S \cong 2.5$ km s^{-1} to $v_S \cong 3.1$ km s^{-1} at 4-9 km depth. In contrast to the sediment basins, we observe higher $v_S \cong 3-3.5$ km s^{-1} (4-9 km depth) beneath the Alpine belt, composed of crystalline and metamorphic rocks (e.g. Molinari et al., 2015b). This S-wave variation is, most probably, caused by the different geology of the Alpine belt and the sedimentary basins.*"

Line 346 – Please provide a reference and sentence summarising the work of geological studies to back up your assumption of sediments. Remove the word assumption.

We rephrased the sentence:

Old: "*We assume that sediments mainly dominate the S-wave velocity for the shallow layers.*"

New: "*At shallow depths (Fig. 8a-b), the S-wave velocity is mainly dominated by sediments.*"
A reference and summary of geological studies is already given in the manuscript, following the rephrased sentence.

Line 353 – Be clear whether these magmatic intrusions are recent or ancient. If they are recent and still contain melt then shear velocity will decrease, whereas if they are solidified then as you say, velocity will increase. I am also surprised that the velocities are not higher if they were solidified mafic intrusions. What is the rock type associated with the magmatic intrusions?
We added a short paragraph on the volcanism in the Ligurian Basin to the introduction. The youngest volcanism north of Corsica and in the Gulf of Genova dates back to 12-11 Ma (Rollet et al., 2002).

Line 355-368 – This paragraph is an over interpretation. Surface waves are sensitive to a broad range of depths particularly with increasing depth and are unlikely to characterise a distinct Moho. All of the shear velocity maps are shown over a depth range of 3km (e.g. 9-12 and 12-15 km). Without resolution tests for depth showing that an exact 1km layer is resolved, the authors cannot comment on observing the Moho. I suggest further resolution tests to aid this, combined with methods that are direct measures of discontinuities such as receiver function analysis. Cross sections for the study area are also required.
We agree that we can not resolve a discontinuity with the method. We rephrased the section to make it clear that we rely on Moho depth estimates based on seismic data. Given the depth along seismic lines, we then use our data to make assumptions on the 3D structure.

Line 359 – Where have you taken these velocity values from? Are they the range for the resolved area of mantle or for 1 point beneath the centre of the basin? More details required.
For clarification, we added: "*[…] at the southwestern end of their profile*".

Line 413-424 – You state that the Alpine front should be visible but also say it is only 50 km wide. Given checkerboards tests show shallow features are resolvable at 1 degree is it possible to resolve this feature?
We rewrote this section and also added checkerboard tests with a finer grid size.

Conclusions
The conclusions and abstract are clear and summarise the key points of the study very well.
Thank you very much for your constructive and positive comments.

Figures
Figure 1 – Overall a clear figure. I suggest colour coding or using different symbols for the various networks in order to distinguish them. Enlarge the inset map to a scale where the red square is more visible. It might be worth using a different colour box for the tectonic features to differentiate from countries, or enlarge the text for the countries. The same is true for the instruments. Also explain why only some instruments are highlighted. 22 OBS/BBOBS are plotted which I assume are the instruments with complete datasets.
We added the OBS stations that were not used to the map, color coded in red. Also, we enlarged the text for the countries and stations and chose a smaller area for the inlay map, so the red square is more visible. All OBS are named, but on land we only named the three stations, for which we calculated all land- land cross-correlations (A317A, ARBF, DIX), as well as the station that is part of the exemplary OBS-land pair in Fig. 5 (BHB).

We decided not to use different symbols for the different land station networks. In our opinion, this would not increase the clarity of the figure. The network code, name, and location of all stations are listed in Table S1.

Figure 2 – Potentially colour the ray paths according to hit count. This would also help determine if only using select OBS-Land combinations is appropriate. Label images as ambient noise and teleseisms respectively.
We followed this suggestion and updated Figure 2, now showing hit counts. The caption already states that 8 s and 20 s show the ray coverage of ambient noise cross-correlation and teleseismic cross-correlation, respectively.

Figure 3 – It would be useful to see a land-land ambient noise pair. Perhaps if space is an issue, include in the supplementary material a good and bad version for each cross correlated pair for each combination of stations (e.g. land-land, OBS-OBS etc). The caption suggests one pair is not included which I think is b) but if c) was also discussed, this would be clearer.
We updated the caption to make clear that (b) was excluded. Also, we added a land-land station pair. We did not include a bad station pair because we think it does not improve the clarity and focus of the manuscript.

Figure 4 – Looks ok

Figure 5 – Space needed in group velocity but otherwise looks good. Corrected.

Figure 6 – Draw a line around areas with no ray coverage and low resolution to make clearer. In caption say purple triangles are stations. Enlarge the font size for the depth.
Updated accordingly.

Figure 7 – Please include checkerboard tests for the shortest and longest periods used for both ambient noise (5 -15s) and the teleseisms (20 - 90s or 20 – 40s if these are the only interpreted images) to give a clear indication of the extent of resolvable features. If space is an issue, add to the supplementary material. What size are the anomalies? Please state. Enlarge the font size for the depth. Also include checkerboards for different scales.
We changed Figure 7. Now, we show checkerboards for 5 s, 15 s, 20 s and 90 s. Checkerboards for all other periods are added to the supplementary material (Fig. S4-S7). We now show one checkerboard with 0.2°x0.2° grid size as well as one with 0.8°x0.8° grid size for each period. Fonts are enlarged.

Figure 8 – Enlarge the font size for depth.
We updated Fig. S3 accordingly. We also added a figure showing the root mean square (RMS) value for every grid cell for the shear-wave-velocity inversion.

Data Availability – While the raw data is accounted for, the final model is not included or any outputs for the processed cross correlation functions. I recommend uploading at least the final model with errors as a supplementary material in line with the journals policies.
We now provide a *.zip-folder containing the in- and output of the group velocity tomography, the code we use to create 1D shear-wave velocity inversion input files from that, and the resulting output as *.xyz files (including plot-script).

Technical Corrections

Line 25 – Change "We find no hint on mantle serpentinization …" to "We do not observe mantle serpentinization…" We changed this.

Line 33 – Remove the use of "on." removed

Line 41 – 42 – This sentence is not clear. Perhaps commas would help? We added commas to structure the sentence.

Line 94 – Add e.g. before references. There are many papers that have developed the ANT technique. We added "e.g.".

Line 203 – Add reference to figure 7 (checkerboard tests). added

Line 286 – Remove "an" removed

Line 295 – Section 4.2 I think should be 5.2 Well spotted, thanks! We changed that.

Line 286 – Remove "an" in "with an increasing period" removed

Line 328 – Change "fits" to "is comparable" changed

Line 348 – Reference required for the sediment thicknesses reference added