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

Felix Noah Wolf[1], Dietrich Lange[1], Anke Dannowski[1], Martin Thorwart[2], Wayne Crawford[3], Lars
Wiesenberg[2], Ingo Grevemeyer[1], Heidrun Kopp[1,2], and the AlpArray Working Group[+]

Overall this is a high quality manuscript that provides information on additional pre-processing requirements for OBS data, and performs a group and shear velocity inversion using ambient noise data and teleseisms. The authors use a new OBS dataset (LOBSTER) that is located in relatively shallow water compared to previous studies (~1000 – 2000 m depth) and find the fundamental mode is not always the most prominent signal requiring a more involved processing method. The resulting maps are used to interpret the lithospheric structure beneath the Ligurian sea and Alpine Region with a particular focus on Moho depth and sediment thickness variations and use the velocity variations to determine no evidence for mantle serpentinization.

Although the paper is reasonably well written (particularly the methods section), there are areas where more information and clarity are needed, particularly for the resolution and interpretation sections. Further resolution tests are needed to determine whether the features discussed are resolvable both laterally and in depth. This is particularly important for the teleseismic dataset and periods >20s. Depth sensitivity and resolution are not discussed. The results and interpretation raise some interesting discussion points on the variations in the lithospheric structure with a focus on sediment thickness changes and Moho depth variations, however both sections are confusing to follow and the interpretation often needs more explanation. The main problem is the use of surface waves as a direct measure for Moho depth. Surface waves should only be a proxy for discontinuities due to their broad depth sensitivity. Combining these indirect measures with more direct measure for Moho depth such as receiver functions would aid the authors interpretation and be a more compelling argument, coupled with a rigorous depth resolution analysis.

Below I delineate the issues:

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.

Abstract
Line 14 – State what velocity models used

Introduction – Add a review of the current knowledge of crustal thickness and sediment thickness. This then gives context to the interpretation later.
Line 40 – Be more specific than narrow and steep.

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.

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.

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

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?

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?

Line 207 – What is the spacing of the new refined grid.

Line 208 – Why remove 11, 13 and 14s period?

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.

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

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 this not possible given the short deployment? Did you look at in the noise quality and whether there is a clear variation between the quality of deeper OBS and those shallower?

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.

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.

The authors suggest the resolvable area has been chosen based on the checkerboard tests. However for 40s 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 40s 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.

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.

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.

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.

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.

Line 312 – Does the average RMS vary with depth? Given much of the discussion is focussed on Moho depths if 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.

Lines 314-317 – Move to results section.

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?

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.

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?

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.

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.

Line 413-424 – You state that the Alpine front should be visible but also say it is only 50km wide. Given checkerboards tests show shallow features are resolvable at 1 degree is it possible to resolve this feature?

Conclusions

The conclusions and abstract are clear and summarise the key points of the study very well.

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.

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.

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.

Figure 4 – Looks ok

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

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.

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.

Figure 8 – Enlarge the font size for depth.

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.

Technical Corrections
Line 25 – Change “We find no hint on mantle serpentinization …” to “We do not observe mantle serpentinization…”
Line 33 – Remove the use of “on.”
Line 41 – 42 – This sentence is not clear. Perhaps commas would help?
Line 94 – Add e.g. before references. There are many papers that have developed the ANT technique.
Line 203 – Add reference to figure 7 (checkerboard tests).
Line 286 – Remove “an”
Line 295 – Section 4.2 I think should be 5.2
Line 286 – Remove “an” in “with ~~an~~ increasing period”
Line 328 – Change “fits” to “is comparable”
Line 348 – Reference required for the sediment thicknesses