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Interactive comment on "Near surface structure of the North Anatolian Fault Zone from Rayleigh and Love wave tomography using ambient seismic noise" *by* George Taylor et al.

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General Comments

In this study, the authors present a shear velocity model of the North-Anatolian Fault Zone near 30.3°E longitude for the top 10km with an extent of 30x70km. The model was computed using a classical surface wave tomography approach with surface waves extracted from ambient noise cross correlations. It provides a detailed image of the studied region that matches observations from previous studies and seems to bring new insight into the local geological structure, including previously unavailable estimates of sedimentary basin depths. I am not familiar with the study region and there-

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fore will focus this review on the technical/methodological aspects.

The work has been done in a professional manner and is mostly well documented, although some aspects remain unclear. Many details about the method are given, but some essential ones are missing that would help the reader better understand the approach, reasoning behind it, and assess the confidence in the interpretation of the model.

I think this work presents interesting observations for the region and ambient noise in general, but needs more work on the methodological part to make it fully reproducible and comprehensible. Therefore, I suggest this work to be published after moderate to major revision.

Specific Comments

Major Points

1. The interaction of group-velocity measurement and phase-velocity measurement is a bit unclear. The group velocity measurements are described well, but how exactly they are used to constrain the phase velocities and how the phase velocities are measured is not.

2. The benefit of the 2-step approach to shear-velocity inversion (neighbourhood algorithm to find initial model for linearised inversion) is not explicitly demonstrated or referenced. The linearised inversion does decrease the misfit significantly using the found initial model, but how does it compare to a ‘guessed’ initial model? I assume the authors did some tests, encountered problems, or have previous experience with linearised inversions which lead to deciding on this procedure. I would appreciate more insight into the reasoning, because this approach seems to have potential to help with the choice of an initial model for linearised inversions in general.

3. The checker-board test provides only rudimentary insight into the resolution of the phase velocity maps, because the results of only one single velocity-distribution, which

is not argued for, are presented. See Lévêque et al., 1993 on how checkerboard tests can lead to misinterpretations, if not done carefully. Because the inversion algorithm used to construct the phase-velocity maps does not provide an inherent resolution estimation (to my understanding), I suggest to add additional checkerboard tests to better judge the ability to image features of different sizes and magnitudes. On a related note, can the authors share insight on how lateral resolution estimates may translate from phase-velocity maps to shear-velocity maps?

> Lévêque, J.-J., Rivera, L. & Wittlinger, G. On the use of the checker-board test to assess the resolution of tomographic inversions. Geophys. J. Int. 313–318 (1993).

4. What is the depth resolution of the shear velocity inversion? The linearised inversion scheme implemented in CPS (Hermann 2013) provides a resolution matrix as output. I suggest to add a figure of example resolution matrices to the Supplementary (maybe 3 matrices for the 3 previously shown nodes, or a mean resolution matrix of all nodes) to give the reader a better understanding of the validity of the author's interpretation of the model. This would be in addition to the "vertical resolution" insight gained from the depth sensitivity kernels as the resolution matrix better illustrates the interdependence of different depths, possibly giving insight into potential biases in the final model and interpretation thereof.

5. The authors investigate and interpret the azimuthal anisotropy found by comparing Love- and Rayleigh-wave maps. I am curious to see if the authors also investigated the potential bias in the group- and phase-velocity measurements themselves that may be introduced e.g., by an inhomogeneous noise source distribution in the study region. Are there other studies for the region investigating the noise source distribution and the effect this may have on velocity measurements?

6. Why did the authors chose to not invert the group velocities for group-velocity maps? The bulk of the work is already done by manually measuring all dispersion curves. The methodology would be very similar to the approach based on phase velocities and

using the group velocities may provide additional insight or help better constrain the imaged features.

7. The used colormap excels at pronouncing differences in the models (Figures 3, 4, 5, 6, 7, S9, and S10), but can lead to misinterpretation, because it is not perceptually uniform and introduces visual discontinuities where there are no discontinuities in the data. I suggest to use another colormap that is perceptually uniform. One collection can be found here: http://www.fabiocrameri.ch/colourmaps.php. Why non-uniform colormaps can be problematic, see e.g.,

> Michelle A. Borkin and Krzysztof Z. Gajos and Amanda Peters and Dimitrios Mitsouras and Simone Melchionna and Rybicki, Frank J. and Charles L. Feldman and Hanspeter Pfister. (2011) Evaluation of Artery Visualizations for Heart Disease Diagnosis, IEEE Transactions on Visualization and Computer Graphics 17, 12.

8. The axis labels on Figures 1a, 1b, 3, 4, 6, and 9 are incomplete/wrong. Especially figure 9 is hard to read and understand like this. Similarly, the colorbar labels on Figures 3 and 4 are missing punctuation marks. The colobar label on Figure 6 is unreadable.

Minor Points

Main Text

- Page 2, Line 19-20 / Figure 1: How high is high topography? A colorbar for topography would be helpful in the figure. This would also help to judge whether there may be possible bias in the measurements caused by station altitudes.

- Page 2, Line 23: Please give a reference for the observations of “Striations and down dip motion on faults”.

- Page 4, Lines 26-33 and Figure 2: The authors mention several possible explanations for the near-0 arrivals that are dominant on the ZZ-component, which indeed is commonly observed. Do these also explain the multiplets of horizontal lines around -10s and +10s in the ZZ-panel or do these require another interpretation? Similar features have been observed in Lehujeur et al., 2018 that have been found to be instrument artefacts caused by the used digitisers.

> Lehujeur M., J. Vergne, J. Schmittbuhl, D. Zigone, A. Le Chenadec & EstOF Team (2018). Reservoir imaging using ambient noise correlation from a dense seismic network. J. Geophys. Res., https://doi.org/10.1029/2018JB015440.

- Page 6, Lines 5-8: The 13.5km inter-station distance threshold makes sense for measurements at 1.5s period. In this study, periods up to 10s are used, which would then lead to all station pairs with less 30km (assuming c=3km/s) inter-station distance at 10s. Instead, station-pairs are excluded based on visual inspection for all periods > 1.5s. Why did the authors choose a different approach for 1.5s and all other periods?

- Page 6, Lines 14-16: This part needs more details to be fully reproducible. I understand that the theoretically computed phase-velocity dispersion curves (from group-velocity curves) are used to constrain the phase-velocity measurements. How are the computed phase-velocity curves constructed? How are the phase velocities measured, i.e., what part of the waveform is used (e.g., zero-crossings, instantaneous phase, . . .)? How exactly does the theoretical phase-velocity curve constrain the measured phase velocities? Are the phase velocities picked manually or automatically?

- Page 6, Line 20-22: Mainly because it is not well described how exactly phase velocities are picked, this part is a bit confusing. Depending on the measurement procedure, it may be going in circles as follows: 1) Measure phase travel-time (if measuring e.g., zero-crossings) 2) Compute phase-velocity estimated from inter-station distance and great circle propagation 3) Convert to travel times. Please clarify this section.

- Page 7, Line 29: “A total of 20050 . . . for each node. . . “. Does this mean 20050 times the number of nodes (however many that may be) or 20050 distributed over all nodes?

- Page 7, Line 30: Eq. (2) is referenced before being introduced (in the very next sentence). Maybe change the order of these two sentences.

- Page 9, Lines 7-9: Did the authors try using the regional Karahan et al., (2001) model (see Page 6, Lines 16-17) as the initial model for the linearised inversion? Please elaborate on the benefit of using the initial model obtained from the neighbourhood algorithm. Why does the neighbourhood algorithm not converge to the same solution as the linearised inversion and how different are the final models retrieved from neighbourhood and linearised inversion?

- Page 14, Lines 15-19 / Figure 9: This section is a bit hard to follow, mainly because the axis labels in Figure 9 are unreadable.

Supplementary

- Text S4, Figure S4: This looks similar to a standard L-curve analysis, where a regularization parameter is usually chosen near the maximum curvature of the model-variance-vs.-parameter curve (e.g., Hansen & O'Leary, 1993). This is not mentioned explicitly, though. Is this choice of dampening based on maximum curvature or on subjective judgement alone?

> Hansen, P. C. & O'Leary, D. P., 1993. The Use of the L-Curve in the Regularization of Discrete Ill-Posed Problems, SIAM Journal on Scientific Computing, 14(6), 1487–1503.

- Text S10 and Figures S14, S15: There seems to be a slight bias of available interstation-azimuths that can not be explained by the N/S-dominant station distribution alone. The Rayleigh wave distributions (Fig. S14) are dominated by slightly NNE/SSW-rays (especially at 6s and 8s), while the Love wave distributions (Fig. S15) are dominated by slightly NNW/SSE-rays (well visible at all periods). Do the authors have an explanation for this difference? Maybe this is a sign for bias introduced during the visual inspection and selection of dispersion curves (main text: page 6, line 9) that could be caused by a potential difference in noise source distribution for Love and Rayleigh waves (See e.g., Riahi et al., 2013).

On that note, I suggest to add to Text S10 why there are generally less paths available

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for 2.0s than 4.0s, as this is not explained by the (in the main text) wavelength-based exclusion alone and a consequence of the visual inspection, I assume. Do you find dominance of higher modes at shorter periods that lead to these periods being preferentially excluded?

> Riahi, N., G. Bokelmann, P. Sala, and E. H. Saenger (2013), Time-lapse analysis of ambient surface wave anisotropy: A three-component array study above an underground gas storage, J. Geophys. Res. Solid Earth, 118, 5339–5351, doi:10.1002/jgrb.50375.

Technical Corrections

Main Text

- Figure 1: Axis labels for both maps are unreadable. I suggest to add a colorbar for topography. I did not find the station names being used in the text, therefore the authors could remove the explanation.

- Figure 3: Axis labels for all maps are unreadable. Colorbar labels have no punctuation marks and colorbar has no title (phase velocity).

- Figure 4: Same as Figure 3.

- Figure 6: Same as Figure 3 & 4. Additionally, the colorbar labels are incomplete.

- Figure 9: Same as Figure 3, 4, 6

Supplementary

- Figures S1, S2: Both figures are a bit low quality.

- Figure S13: “The blue line is the best fitting curve the raw data.” -> “The blue line is the curve best fitting the raw data.”

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