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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.

George Taylor et al.

george.taylor@helsinki.fi

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Dear Sven Schippkus,

We would like to thank you for taking the time to produce a detailed review of our manuscript. We have taken each of your comments into consideration, and you can find our responses below. Any page and line numbers refer to the clean, updated manuscript. I will shortly upload the 'tracked changes' version of the manuscript, once it has been checked and finalised.

Major points

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.

We realise that the current description of how the phase velocities are determined is unclear. A description can be found in the documentation on the *do_mft* program that we use to determine phase velocity (Herrmann et al., 2013). In particular, phase velocities are calculated using the analytic signal of a narrowly bandpassed surface wave using the equation:

$$c = \frac{\omega_0 r}{-\Phi + \frac{\pi}{4} + \frac{\omega_0 r}{U_0} + N2\pi} \tag{1}$$

where ϕ is the instantaneous phase of the surface wave, ω_0 is the centre frequency of the filter, r is the inter-station distance and U_0 is the group velocity. N is some integer. Thus, once the group velocity curve is known, the corresponding phase velocity curve(s) can be calculated. The ambiguity in the phase velocity curves arises from the factor $N2\pi$ in this equation. This where the a priori earth model is used. *do_mft* uses eq. 1 to calculate a suite of phase velocity curves for various values of N, and the curve that most closely matches the synthetic phase velocity curve of the a priori Earth model is considered to be the one that corresponds to the correct value of N. This phase velocity curve is picked manually in our case.

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.

The main benefit of using the neighbourhood algorithm in this case is to provide a much broader overview of the acceptable parameter space of the inverted S-wave velocity models, rather than just presenting one 'best-fitting' model from a linearised inversion. In addition, presenting the results of the neighbourhood algorithm such as in the current Fig. 5 also allows the reader to form an intuitive (if only qualitative) understanding of potential uncertainties in the S-wave velocity model. However, limitations in the number of free parameters that can be efficiently inverted for using the neighbourhood algorithm ($\sim$30) can cause problems. This is best demonstrated by the most northern model node in Fig. 5, which displays a larger range of acceptable models and a considerably higher misfit than the other two nodes presented. This may in part be due to the rather coarse parameterisation imposed on the problem by the neighbourhood algorithm, which is unable to provide a satisfactory fit to the dispersion data collected within the sedimentary basins that are located in that part of the model.

The 2-step inversion process has been applied in fault zone imaging before (Hillers and Campillo, 2018), and is an attempt to compromise between these two problems. We want to provide the reader with an overview of the acceptable parameter space and an intuitive sense of the uncertainty in different parts of our model through the neighbourhood algorithm, as well as presenting a model that provides the best fit to the data. As the reviewer has pointed out, the improvement of fit following the linearised inversion is substantial, and as the neighbourhood algorithm results are already to hand, they may as well be used as the initial model to guide the linearised inversion, rather than using a guessed model, or the Karahan model (which is very coarsely parametrised).

We have updated the manuscript to include the full justification of our approach as outlined above. This information can now be found on page 8, lines 27 – page 9 lines 1 – 2.

> Hillers and Campillo, 2018. Fault zone imaging from correlations of aftershock waveforms. *Pure. Appl. Geophys.* 175.

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 Leveque 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?

We agree with the reviewer's concerns regarding the issues with checkerboard tests. However, as the reviewer stated, the inversion algorithm we used for the phase velocity tomography does not include an inherent resolution estimate. In such cases, checkerboard tests remain the standard tool for estimating horizontal resolution in tomography studies.

We do not believe that including further checkerboard tests would provide the reader with any more useful information on lateral resolution in this case. Displaying a fixed checkerboard wavelength at different wave frequencies, as we currently do, covers the same physical parameter space as showing different checkerboard wavelengths at a fixed frequency, and makes adding further checkerboard wavelengths redundant.

Nonetheless, the reviewers point is valid, and we do wish to provide evidence of the robustness of our tomography to recovering anomalies of different amplitudes and shapes. We have extended the supplementary information to include two new resolution tests for our ray path distribution at 4 s period: we show the recovery of two spikes (of different amplitude to the checkerboard) in Fig. S12, and in Fig. S13 we show the ability of our tomography to recover a randomly generated velocity model that contains anomalies of differing wavelengths. We hope that these further tests provide greater confidence in the horizontal resolution of our phase velocity tomography. Furthermore, we now provide the details of the lateral interpolation performed when constructing the final S-wave velocity model on page 12, line 7 - 9.

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.

In the supplementary material (Fig. S9), we have added the requested resolution kernels for the 3 model nodes displayed in Fig. 5 of the main text.

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?

The analysis performed in Fig. 8 is based upon the raw phase velocity measurements, with the Rayleigh wave (main text) and Love wave (supplementary) phase velocity measurements treated separately. The raw phase velocity measurements are represented as black dots in the figures. The raw data is binned and averaged in 10 degree azimuth bins (red dots) in order to clean up the scatter in the measurements so that a first order analysis of the azimuthal variation of the phase velocities can be performed.

Whilst we do not know of any study that specifically investigates the noise source distribution in this region, it is likely that the noise is predominantly aligned perpendicular to the coastlines of the Black Sea and the Mediterranean. Evidence of this is visible in Figs. S17 and S18, giving a first order overview of the azimuthal distribution of our phase velocity measurements, which are dominated by north-south oriented paths. The main effect of the presence of an anisotropic noise source distribution is to increase the uncertainty of our measurements taken from azimuths that are not aligned in the dominant direction (e.g. east-west), as fewer total measurements are available. This effect is visible in Fig. 8 for example: the binned data at azimuths $\sim$ 90 degrees in general have larger uncertainty bars than the measurements at 0 or 180 degrees.

We have altered the manuscript to include more information on this point and clear up any confusion as to how the phase velocity measurements in these figures are being presented, and to include the necessary caveats that an anisotropic noise distribution increases the uncertainty when trying to analyse the azimuthal variation of phase velocities. These alterations can be found on page 13 line 20 – page 14 line 2.

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.he manuscript has been updated with all of this information in order to better explain this process to the reader, and can be found on page 6 lines 16 – 23.

The fast marching method is a wavefront tracking approach that uses an eikonal solver at its core to compute the wavefield through the velocity model at each iteration. As such, it is strictly only valid for tracking phase velocity wavefronts, rather than group velocity. This is due to the fact that the direction of the actual wave vector (phase velocity) and the ray path of the group velocity can be very different in an anisotropic medium (Tanimoto 1987). In practice, one can get away with using an eikonal solver on group velocities if the medium is smooth and only weakly anisotropic. In our case, we judge this to be a poor assumption in the current study area, due to the complex structure of the large fault system and the clear presence of azimuthal anisotropy in our phase velocity measurements. Group velocity measurements also tend to be associated with larger uncertainties than phase velocities (Lin et al., 2013).

In fact, in an early version of this manuscript submitted to another journal, we did invert group velocity measurements, but we were asked to instead target phase velocities by the reviewers there, for the reasons stated above. As we have received such conflicting advice on this point from several reviewers, we believe that the inversion of group velocities is on shaky ground in this study, and there appears to be no consensus within the community as to whether such a treatment is acceptable. For these reasons, we prefer to focus only on the phase velocities here.

> Tanimoto, 1987. Surface-wave ray tracing equations and Fermat's principle in an anisotropic earth. *Geophys. J. R. Astr. Soc.* 88.

> Lin et al., 2013. Surface wave tomography of the western United States from ambient seismic noise: Rayleigh and Love wave phase velocity maps. *Geophys. J. Int.* 173.

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 collec tion can be found here: http://www.fabiocrameri.ch/colourmaps.php. Why non-uniform colormaps can be problematic

We have updated the colour maps of both our phase velocity and S-wave velocity images, using the resource suggested by the reviewer here. As alluded to by the reviewer, the new colour map does a poor job of emphasising the structure at long periods in the phase velocity tomography. As such, we have updated Figs. 3 and 4 to show phase velocity up to 5.0 s period. We discuss the reduction in horizontal resolution, and the horizontal averaging of structure, at longer periods due to the increased wavelength of

the surface waves on page 14, lines 12 – 15.

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.

We apologise for the technical issues, but this appears to be an issue with the download of the pdf from the Solid Earth discussions site. It seems to be at least in part a function of browser / operating system. Using Chrome on MacOS, I experience the same issues as the reviewer, but with Firefox on a Linux machine, I do not have issues with the pdf or any of the figures. The raw figure files do not have any of these issues. We will ensure that the final figures are in a suitable format to hopefully avoid these problems, and that the final version of the manuscript works properly.

Minor points

- 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.

A colourbar for the topography has been added to Fig. 1 as requested.

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

The reference to Dogan et al., (2014) has been added to this sentence on page 2 line 23.

- 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.

We would like to thank the reviewer for pointing out this publication to us. We were also puzzled by the potential causes of the arrivals at +/- 10 s, and attributed them to either being body wave reflections contained within the noise, or an artefact of the focal spot. We are happy to also include the reviewers reasoning and this citation in a discussion of the multiplets that can now be found on page 5, lines 7 – 9.

- 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?

This is a result of the manual picking of phase velocity curves used in this study. It is very easy for us to discard an entire period – velocity map on the basis that none of the data it contains are trustworthy, due to the fact that even the shortest period (1.5 s) does not fulfill the wavelength criteria. At intermediate inter-station distance, the situation arises where some of the short period data may be trustworthy, and we are compelled to try to visually extract the useful data from these period – velocity maps, and must exclude only the longer period data for which the wavelength criteria is still not fulfilled.

We have updated page 6, lines 6 – 11 to better describe this process.

- Page 6, Lines 14-16: This part needs more details to be fully reproducible. I under stand 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?

We hope our response to the reviewers first point regarding the details of the phase

velocity picking also addresses this point.

- 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.

We hope that the updated description of phase velocity calculation helps to clarify this section. The procedure is as follows: Phase velocity curves between stations -> travel time between stations -> phase velocity as a function of position, as is stated in the current text.

- 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?

We calculate 20050 models for each node. So the total sum is 20050 * number$_{o}f_{n}odes.Wehaveupdatedthetextonpage8, line15 - -16toexplicitlystatethis.$

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

We have switched these two sentences as suggested by the reviewer.

- 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?

We hope that our response to the reviewers second major point covers this comment as well. We have elaborated on our reasoning for the neighbourhood algorithm. As a side note, "convergence" is not a good description of what the neighbourhood algorithm attempts to achieve. As we state above, the neighbourhood algorithm seeks to obtain a suite of solutions that would acceptably fit the data, rather than “converging” to a best solution. This is facilitated within the neighbourhood algorithm by an allowance for each step in the inversion to “move to” a newly generated model, even if new model provides a worse fit to the data than the previous model. This prevents the inversion from displaying results that may only be taken from a local minimum in data misfit. As such, it is not necessarily expected that the two inversion procedures will produce to the same solution. In essence, the neighbourhood algorithm defines “the solution” in different way to a linearised approach. This is also compounded by the fact that, as stated above, the linearised inversion has a much finer velocity parameterisation than the neighbourhood algorithm.

- 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.

Again, we apologise for the technical issues. When the axes labels are visible, we believe this section is clear enough.

Supplementary

We have updated to text S4 to state that the damping parameters were chosen through subjective judgement.

- 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 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?

The reviewer is correct on this point, as far as our thinking on this issue goes. We attribute the differences between Rayleigh and Love wave ray paths to differences in the noise source distribution of the respective waves. On the note of their being less paths available at 2 s: this is due to noisier measurements that are made at shorter periods. The extra noise is caused by the increased scattering of the short period waves off small heterogeneities in the upper crust, as well as difficulties in identifying a fundamental mode signal, as the reviewer suggested here.

We have updated Text S11 to include a short discussion of the above points.

Technical corrections

- 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

The errors with the axes labels are likely due to the problems associated with the download from the website on certain browsers and operating systems. We have added the colour bar for the topography as requested. We prefer to keep the description of station names in the manuscript, as the data are in the public domain, and an interested reader may wish to track it down and use it upon seeing the manuscript.

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

Unfortunately only low resolution .jpg files are available for these figures. We have reduced the size of the figures in an attempt to improve the quality of the images.

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

We have updated the figure caption.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-100, 2018.