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Solid Earth
Discussions

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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Dear Anon,

Thank you for taking the time to review our manuscript. We are glad that you find it interesting and impactful. Below I give our responses to each of the comments on the manuscript. Any page and line references refer to the clean, updated manuscript. I will upload a 'tracked changes' version as a supplement shortly, once the manuscript has been finalised.

Although they emphasize the importance of good and reliable knowledge on crustal

structures along the continental shear deformation zones at the very beginning in the introduction, and since this is one of the primary task for taking all such efforts in the region, I am very upset why they avoid to interpret their results mainly around this target which could be vitally important for future studies that aim at a decent seismic scenario for the region.

There have been numbers of recent geophysical model and observations in a region including the study area and further west dealing with the branch of the NAFZ beneath the Sea of Marmara. However, introduction significantly lacks of a compilation of previous studies and their findings including the DANA experiment.

We appreciate that we may have missed references to studies that would be appropriate to cite and discuss in the context of this work. However, here the reviewer has not given us any specific examples of what essential references might be missing, which makes this comment difficult to respond to.

We believe that we have made extensive referencing to the relevant literature throughout both the introduction and discussion sections of the manuscript. We have made sure to include classic studies of the geological structure of the Izmit region from authors such as: Sengor, Yilmaz, Okay, Barka, Akbayram, Tank, Kahraman, Altuncu Poyraz and Komazawa. We also include a reference for all previous studies using the DANA network, and discuss them where they are relevant. In this revision, we further include a citation to Papaleo et al. (2018), which has been published since the original writing of this manuscript.

In the present work, inversion results for shear wave velocity for deeper sections at 3.5 and 5.5 km do not provide profound velocity contrasts among three tectonic zones, namely, Istanbul, Armutlu-Almacik, and Sakarya Zones (see Fig. 6) whereas using the same network and teleseismic P-and S arrivals Papaleo et al. (2017, 2018) were showing clear separation reflected as relatively high wave speeds beneath Istanbul Zone to the north, and low beneath Sakarya Zone to the south that is mostly likely due to the lithological differences down to, at least, the depth of 20 km.

Only for the first 1.5 depth range, resolution is sufficient to resolve shear zones along the northern branch. There down to the depth of 1.5 km major difference is claimed by the authors to be associated with low S-wave velocity to the north of the NAFZ, associated with faulted marine clastic sediments near Izmit (Akbayram et al., 2016) and with the Adapazari sedimentary basin. I think a detailed introduction with more geological constraint as well as other geophysical data to support this and further velocity variations at this depth range is missing. Such introduction is crucial since below this range velocity variation does not show high resolution details.

The best horizontal and vertical resolution claimed by the teleseismic tomography of Papaleo et al. (2017, 2018) is 15 km, which greatly exceeds even the maximum depth extent covered by this surface wave study (10 km). Furthermore, as teleseismic tomography studies, Papaleo et al. (2017, 2018) severely lack resolution for near surface structures, notably being unable to detect even the sedimentary basins, due to their lack of crossing ray paths at shallow depths. As such, the depth ranges of the current study and Papaleo et al. (2017, 2018) do not even overlap, and we do not find it surprising that exact comparisons are difficult to draw between the two studies.

We compare the results of our investigation to those of Papaleo et al. (2017, 2018) in the manuscript on page 14 lines 23 – 32, noting that despite the major differences in depth range and resolution between the two studies, some features (such as the high velocity Armutlu Block) are common to both models. We are also open about the fact that our horizontal resolution decreases as a function of depth, due to the increasing period of surface waves used to provide the constraints on the deeper sections of our model. This feature is common to all surface wave tomography studies, and is discussed on page 14, lines 12 – 21. We have also now included full resolution kernels (Fig. S9) in the supplementary material for several nodes in our model at reviewer request, so that this information is available to the reader in a quantitative sense.

It seems there is an effect of N-S elongated azimuth of station pairs on resolved images. This effect can be investigated using sensitivity analysis, i.e., checkerboard test results. I am aware that authors have already added materials in Supplementary but I believe it is much better if given within the Sensitivity Analysis section of the main text. In this way, later they can use this by putting quantitative arguments when they describe the results (reliability of various features which will be potentially examined in the Discussion). I would like to see the ray-paths of periods and their checkerboard results in supplementary file to be able to see the influence of dominance of N-S orientation of stationpairs in your data set.

We appreciate that sensitivity analysis is an important part of appraising the results of a tomographic study. However, we prefer to keep this information in the supplementary material, rather than the main manuscript, and we note that the second reviewer of this paper appears to hold the same opinion.

The revised supplementary material now contains an expanded analysis of horizontal resolution, including spike tests (Fig. S12) and we also demonstrate the recovery of a known random velocity field (Fig. S13). We also include depth resolution kernels in Fig. S9. We hope that these further demonstrations of model resolution will satisfy the reviewers concerns on this point.

According to my recollection, in some studies dealing with ambient noise inversions in the literature, group velocities and related time information are used for further inversion process. Here authors are using phase velocities. Perhaps this has to be addressed in the text.

We refer to our response to the comment by Sven Schippkus regarding group velocity for this point. To restate here: we do not believe that a group velocity tomography is theoretically justified given that we use an eikonal solver, and we have received a wide range of conflicting advice from reviewers of the manuscript on this point.

Figure 2 is interesting. One of the first things that is prominent on this figure is the zero-offset energy. What might be the major source for that? Needs to be clarified.

We discuss potential sources of the zero offset energy on page 4, line 27 – page 5, line 3.

Azimuthal anisotropy Large scatter azimuthal variations of phase velocities (see Fig. 8 S13) under the presence of N-S dominating azimuth of station-pairs. Thus long period behavior of directional dependent phase velocities is doubtful. And thus, a frequency varying fast velocity directions (with increasing uncertainties as period increases) is also not too convincing.

This work examines anisotropy issue with a superficial discussion regarding early constraints on seismic anisotropy in the region. Authors appear to take the discussion regarding seismic anisotropy only using a single SKS splitting study (Biryol et al., 2010), which has been informative for upper mantle anisotropy. However, there are a few earlier studies performed along the NAFZ (central and western NAFZ) with direct observation of crustal anisotropy. No specific discussion in the light of earlier works revealing upper crustal anisotropic structure mainly based on shear wave splitting structure (e.g. Peng and Ben-Zion, 2004-2005; Hurd and Bohnhoff, 2012) or entire crust from RFs analyses (Vinnink et al., 2015; Licciardi et al. 2018). The question on what part(s) of the area may indicate structure-induced, and what part(s) stress-induced anisotropy is still ambiguous. Moreover, a single model for such a complicated tectonic setting with significant lateral heterogeneities cannot be represented a single-smooth depth-varying model with very consistent SKS orientations (see e.g. Peng and Ben-Zion, 2004-2005; Hurd and Bohnhoff, 2012; Vinnink et al., 2016). At least early shear wave splitting and RFs data suggests the opposite what the current work says.

We thank the reviewer for providing these references to interesting prior studies on the azimuthal anisotropy of the Izmit region. We had not been able to locate them ourselves, and we have incorporated each into the discussion of our anisotropy results in section 4.2, page 16, line 26 – page 17, line 5. However, we do not agree that these

studies suggest the opposite of our results. In fact, Peng and Ben-Zion (2004, 2005) clearly detect a cluster of fast directions oriented between 45 and 90 degrees from north in the top 3 km of the crust. This range of fast directions matches exactly to the Rayleigh wave fast directions that we measure (Fig. 9). Peng and Ben-Zion (2004, 2005) also note that the fast direction often aligns parallel with the strike of the North Anatolian Fault, and we argue the exact same point for our short period measurements on page 17, line 6.

Furthermore, Hurd and Bohnhoff (2012) analyse only one station that overlaps with the current study area: CAY. Their analysis of shear wave splitting at CAY shows fast directions that are aligned between 30 – 90 degrees from north, with most observations clustered at 45 degrees. This again overlaps exactly with our range of measurements in Figs 8 and 9. Whilst Vinnik et al. (2016) targets anisotropy in the upper mantle, they also detect a dominant fast direction of 60 degrees from north between 30 km depth and the surface. We believe these results actually lend great weight to our first order observations of azimuthal anisotropy.

We feel that our description of the anisotropy results may be leading to some confusion here, especially as Fig. 9 was not properly described in the initial submission of this manuscript. We have now updated section 3.6, page 13, lines 2 – 10 to better describe the anisotropy results and to better integrate the information contained in Fig. 9

Another thing I could not figure out is that authors do not provide any clue regarding radial anisotropy? If they are already able to invert both love and Rayleigh wave wouldn't it be possible to visualize radial and tangential shear wave speed variations at various depth?

It is difficult for this study to accurately measure the presence of radial anisotropy as a function of position, due to the differing levels of damping applied to the Love and Rayleigh wave phase velocity tomographies (Fig. S4). The Love wave data require a higher level of damping than the Rayleigh wave data. This differing level of damping introduces biases between the two velocity data sets that are impossible to resolve from radial anisotropy. In an early version of this manuscript submitted to another journal, we made an attempt to quantify radial anisotropy as the reviewer has requested here. This approach was met with harsh criticism for the reasons outlined above, and as such we do not believe we can make any reliable estimation of radial anisotropy from the results we present in this study.

More importantly, I am seriously wonder why they have not gone for a detailed harmonic analysis that can provide depth variation of fast polarization azimuths on a finer spatial resolution using on available data set.

We do not believe that this data set is suitable for harmonic decomposition, given the already limited ray path distribution (Fig. S17 and S18). Further decomposing the data set is likely to exacerbate the issue with the north-south dominated ray distribution, leading to unreliable estimates of azimuthal anisotropy. We believe that our simpler, first order approach to analyse the data set as a whole is on much safer ground. If the reviewer is unconvinced by our analysis of the broad regional pattern of anisotropy (as indicated by a previous comment above), then we doubt a more detailed regional decomposition would convince them further!

I would omit this part unless it is supported with a more convincing and detailed analysis of the data set.

We strongly believe that our simple analysis of raw phase velocity measurements is a reliable first order measurement of azimuthal anisotropy. This is clearly demonstrated by the fact that our results are in very close agreement with all of the previous shear wave splitting studies that we have been pointed to by this reviewer. As such, we would strongly argue for its inclusion in this manuscript.

Figures For Figs. 1, 3, 4, and 6, values of latitude and longitude is strange.

This may be due to the fact that Fig. 1 actually displays a larger geographical area

than the subsequent figures. We have checked carefully, and the latitude and longitude values on each figure are correct.

Two references of Sengor (Sengor and Yilmaz, 1981; ÂÿSengor et al., 2005) are not listed in the alphabetical order.

This is probably due to the bibliography style file not recognising the Turkish “S” character. This issue has been fixed.

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