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Interactive comment on "Crust and upper mantle structures of the Makran subduction zone in south-east Iran by seismic ambient noise tomography" *by* M. Abdetedal et al.

M. Abdetedal et al.

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Reply to the referee#1: We would like to express our appreciations to details, constructive comments given by the referee. We have studied all the comments carefully and made major corrections to the manuscript, and we hope that they met the referee's approval. We answered all the questions, comments and suggestions in details in attached pdf file and please also see the corresponding changes in the main-text. The discussion section has been modified based on the comments from referee #2 significantly and now is more focus on the original outcomes. Also some of the figures including Fig. 10 has been updated and we added symbols/text to outlines those main

anomalies used in the text for interpretation. Note that the original comments given by the referees are marked bold below. All changes and modifications are bold in the revised version of manuscript.

Comments given by the referee #1:

1. Please show station distribution and the sensor types. In Fig. 8, some stations are located out of the map.

Reply to referee #1:

We have modified Fig.1, inset map according to the comment given by the referee.

2. Please show plots of cross-correlation functions against the separation distance. Without the plots, I cannot determine the data quality.

Reply to referee #1:

We have shown cross-correlation functions against the separation distance for all station pairs in Fig. 2. The manuscript was modified as below, page 6 line 8-11.

All empirical Green's functions in the 10-50 s period band are plotted in Fig. 2 to evaluate the quality of the cross-correlation functions. Both positive and negative correlation lags show clear surface waves with an average apparent velocity of about 3 km/s.

3. In section 3, the authors discuss the directionality, but they did not discuss the effects for group velocity measurements. The authors should discuss them. For example, Harmon et al. (2010) discuss the basis of phase measurements.

Reply to referee #1:

Results of our analysis show that the sources of the microseism exhibit variability in time but significant amount of ambient noise e.g. more than at least 51% exist in all period studied for yearly data, see page 8 lines 9-27. To follow the comment, we added the sentence given below, see page 7 lines 2-13:

Therefore we need to find out whether ambient noise is well enough distributed in azimuth to return unbiased dispersion measurements for use in tomography. Based on phase speed measurements from a three-station method, Lin et al., 2008 present evidence that ambient noise is distributed sufficiently isotropically in the frequency band they consider (6– 40 s period), so that phase velocity measurements are returned largely unbiased. To quantify the effect of strongly anisotropic background noise source distribution, Yang and Ritzwoller, 2008 performed synthetic experiments and they found that in the presence of low level homogeneously distributed ambient noise, less than 0.5% of measured phase velocities is affected by much stronger ambient noise in an off-axis direction. According to Yang and Ritzwoller, 2008 we need to show that in all period ranges studied the useful amount of ambient noise signals in all azimuths are above 50%.

4. Spectrogram against periods and group velocity (e.g. Fig. 13 of Bensen et al. 2007) should be included.

Reply to referee #1:

We produce Fig. 6 according to the comment given by the referee. The main-text in the manuscript was modified as below (see page 9, lines 10-14):

Fig. 6 shows the procedure graphically using the waveform obtained from 12-month stacked cross-correlation between stations BNDS and GHIR. Fig. 6b illustrates the dispersion ridge tracked as a function of period to obtain group speed curve. Then the phase-matched filter is applied to clean the waveform. The cleaned waveform is redispersed , following Bensen et al., 2007 and it is shown as the dotted line in Fig. 6c.

5. P.11 lines 10 and 19: The authors argue "The non-linearity is not significant for group velocity measurements". However, I cannot understand the logic. In general, the nonlinearity becomes important when the lateral heterogeneities are strong. In particular, initial model dependency on the final model is problematic in many cases.

Reply to referee #1:

The main text was modified as below: (page 10 lines 5-26) We used the fast marching surface wave tomography (FMST), the iterative non-linear inversion package developed by Rawlinson, 2005 and Rawlinson and Sambridge, 2005. This method includes the forward calculation and inversion procedure. The inversion procedure is carried out with subspace method with the assumption of local linearity. It is based on a local linearization of the problem about the current model to seek the perturbation of the model parameters to match the group velocity measurements. The inversion step allows both smoothing and damping regularization to suppress the non-uniqueness of the solution. FMM is a grid-based numerical algorithm base on the eikonal equation which is formulated to locate the first arrival phase of surface waves rather than the group time. However, to describe the dissipation of the group energy an eikonal solver can be used if multi-pathing is not included. In this case the interfering waves cause the group energy to follow notably different paths. Therefore when the phase and group velocities have similar geographic pattern comparable results can be obtained (Arroucau et al. 2010; Saygin and Kennett 2010; Young et al. 2011; Saygin and Kennett 2012). Young et al. 2011, obtained similar group and phase velocity maps using FMM in south-eastern of Australia. The nonlinear relationship between the travel-time and the group velocity could be explained by applications of FMM and subspace inversions (Rawlinson 2005; Rawlinson and Sambridge 2005). However, the nonlinearity is not significant for group velocity measurements as compared to phase velocities because of great circle path approximation, therefore the results produced by the first iteration were considered as the optimal solutions (e.g., Shirzad et al., 2013).

6. Fig. 10: About the sensitivity kernels.

(a) The kernels have no sensitivity at 0 km. I guess the plot is incorrect because typical examples (e.g. Lebedev et al., 2013) show positive values. (b) How did the authors calculate the kernels? In particular, the S wave velocity model used in the calculation should be shown. (c) Sensitivity kernels to P-wave velocity and density are also significant.

Reply to referee #1:

(a) We express our apology for this mistake, we modified the sensitivity kernels in the Fig. 12. by modifying the initial model. The first layer velocity was set to 0 in the initial model that was corrected.

(b) We produce Fig. 12a according to the comments given by the referee. The main-text in the manuscript was modified as below (see page 11, lines 7-9):

In order to guide the interpretation, the sensitivity kernels for different periods were also calculated from AK135 velocity model (Fig. 12a).

(c) Following the reviewer's comment the main-text in the manuscript was modified as below (see page 11, lines 9-14):

In addition to shear wave speed (Vs) we require P-wave speed (Vp) and density () as well for inversion. We use the average continental Vp/Vs ratios of 1.75, from Yamini Fard et al., 2007. Furthermore, surface waves are less sensitive to Vp than Vs except in the uppermost crust. Density () is assigned similarly using parameters of background model (AK135). Sensitivity kernels presented in Fig. 12b.

7. With the sensitivity kernels, the authors could infer 3-D S-wave structures. At least, the authors should present local 1-D structures at typical points.

Reply to referee #1:

We agree with the statement given by the referee to include Vs velocity models of the study area, but it is very much beyond the scope of the current paper. Note that determining Vs velocity model using Rayleigh-waves and Love-waves fundamental modes resulted from ANT and thus constraining the radial anisotropy under the Makran region is an ongoing project by the authors that will be published in a separate publication.

8. Because the number of station pairs is not so large, plot of group velocity anomalies

along the paths is informative.

Reply to referee #1:

If I correctly understood referee means in those part with low ray path coverage the results are not reliable. To minimize the inversion instabilities we use regularization which refers to constraints placed explicitly on the estimated model during inversion. These constraints appear in the 'penalty function' that is explicitly minimized in the inversion. The strength of regularization or damping is usually varies with information regarding data quantity, quality, and distribution and relating to the reliability of the reference model or other prior information. The regularization scheme that we have used was smoothing. If data are homogeneously distributed, but with poor data coverage, smoothing blends the estimated model into a background reference in regions of low data density. The smoothing parameter merge the estimated model smoothly and continuously into the isotropic reference state in regions of poor data coverage. In regions of good coverage, this term has no effect. Furthermore according to checkerboard maps in the study area there is a good ray path coverage. Otherwise, if we miss any point to address the referee's comment, please give us more explanation about your comment. Please note that selected dispersion curves are also plotted in Figs. 7 and 8.

9. In section 6, the authors compared the group velocity maps with moho variations. For a quantitative comparison, the authors should compare the same physical parameters. For example, group velocity maps can be estimated from the model of Shad Manaman et al. (2011).

Reply to referee #1:

The results on Moho depth by Shad Manaman et al., (2011) is very significant, because it is one of the few high resolution Moho depth map in the study area. We used Moho depth obtained by Shad Manaman et al., (2011), in order to help us more in the interpretation and we did not compare it with any tomographic results, however according to comment by referee we modified the “Discussion” section and those parts comparing our results with Shad’s Moho map have been removed.

Minor comments

1. I feel the introduction about the tectonics (in particular related to seismicity) is lengthy.

Reply to referee #1:

The introduction of the manuscript have been modified in accordance with the comments from the Editor to describe tectonic of Iran with more details.

“The description on the tectonics of this region is still insufficient for readers especially unfamiliar with SE Iran.”

However we removed a paragraph and also moved some parts to other section according to comment by second referee. Nevertheless if referee insists we can split out the introduction into two separate sections, if editor and also other referee agree, in the next revision.

2. P.5 line 10, "seismic noise is diffuse”. I guess that “seismic noise wavefield is diffuse"

Reply to referee #1:

The main text was modified according to the comment. (See page 4, line 17).

3. P.5 line 16-18: Please cite references properly. For examples, Shapiro et al. (2005) inferred group velocity map at a local scale, and Yan et al. (2007) also inferred group velocity maps at a regional one. Nishida et al. (2008) also analyzed, for example, Love waves, including the crustal overtones.

Reply to referee #1:

In order to follow reviewer’s comment the main-text was modified as below, see page 4 lines 20-30. Recent studies show that the use of ambient noise to extract surface

wave empirical Green’s functions (EGFs) to infer Rayleigh and Love waves can provide important information about the 3D shear wave velocity structure in the upper mantle both on a global and regional scales. Shapiro and Campillo, 2004, inferred Rayleigh wave group velocity map from ambient noise data recorded in station-pair separated by distances from about one hundred to more than two thousand kilometres. Cho et al., 2007 and Yan et al., 2007, also inferred group velocity maps at a regional scale. Shapiro et al., 2005 and Sabra et al., 2005, extracted time-domain Green’s function from ambient seismic noise at a local scale. Lin et al., 2008, presented seismic ambient noise tomography for love wave. Nishida et al., 2008 also analysed Love waves to study seismic ambient noise tomography, including the crustal overtones.

4. P.5 line 26: were => was

Reply to referee #1:

The correction was done.

5. P.7 line 16: The authors cited Stehly et al. 2006. I think they should cite a paper by Longuet-Higgens should, which is the original paper on the nonlinear interaction.

Reply to referee #1:

To follow the comment, we added the paper by Longuet-Higgens.

6. Fig.2 (left): One octave-band frequency filter (e.g. 10-20 s, 20-40 s, 25-50 s) is better for visual inspection in the time domain.

Reply to referee #1:

We have modified Fig. 3 according to the comment given by the referee.

7. P.11: An explanation of FMST is repetitive.

Reply to referee #1:

We have modified the main-text as below (see page 10 lines 5-12):

We used the fast marching surface wave tomography (FMST), the iterative non-linear inversion package developed by Rawlinson, 2005 and Rawlinson and Sambridge, 2005. This method includes the forward calculation and inversion procedure. The inversion procedure is carried out with subspace method with the assumption of local linearity. It is based on a local linearization of the problem about the current model to seek the perturbation of the model parameters to match the group velocity measurements. The inversion step allows both smoothing and damping regularization to suppress the non-uniqueness of the solution.

8. P.18 line 18: "Our crust and upper mantle velocity maps" => "Our group velocity maps at periods. . ."

Reply to referee #1:

We have modified the text.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/6/C237/2014/sed-6-C237-2014-supplement.pdf

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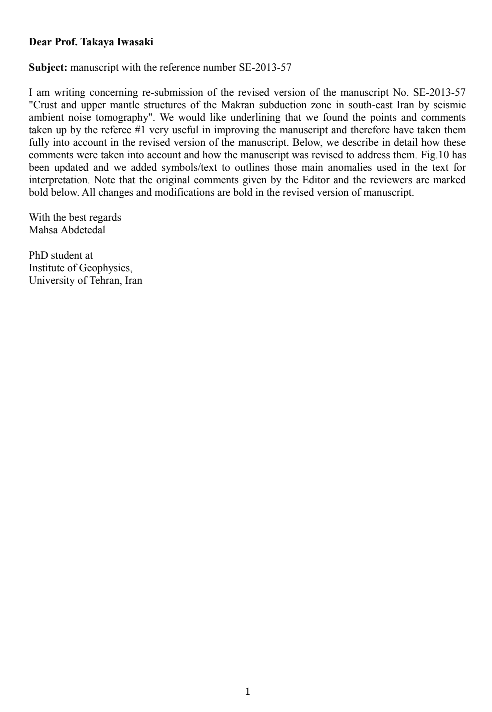
Interactive comment on Solid Earth Discuss., 6, 1, 2014.



Dear Prof. Takaya Iwasaki

Subject: manuscript with the reference number SE-2013-57

I am writing concerning re-submission of the revised version of the manuscript No. SE-2013-57 "Crust and upper mantle structures of the Makran subduction zone in south-east Iran by seismic ambient noise tomography". We would like underlining that we found the points and comments taken up by the referee #1 very useful in improving the manuscript and therefore have taken them fully into account in the revised version of the manuscript. Below, we describe in detail how these comments were taken into account and how the manuscript was revised to address them. Fig.10 has been updated and we added symbols/text to outlines those main anomalies used in the text for interpretation. Note that the original comments given by the Editor and the reviewers are marked bold below. All changes and modifications are bold in the revised version of manuscript.

With the best regards
Mahsa Abdetedal

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Fig. 1. reply-letter