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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.

Anonymous Referee #1

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The authors conducted ambient noise tomography beneath southeast Iran. For the publication, details of data analysis are necessary. Specifically, the following issues need to be addressed.

1 Major comments

1. Please show station distribution and the sensor types. In Fig. 8, some stations are located out of the map.
2. Please show plots of cross-correlation functions against the separation distance. Without the plots, I cannot determine the data quality.
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.
4. Spectrogram against periods and group velocity (e.g. Fig. 13 of Bensen et al. 2007) should be included.
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.
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.
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.

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

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).

2 Minor comments

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

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

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.

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

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.

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.

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

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

3 Reference

- Harmon, N., Rychert, C., & Gerstoft, P. 2010, Distribution of noise sources for seismic interferometry, Geophys. J. Int., 183, 1470

- Lebedev, S., A., Joanne, and M. Thomas (2013), Mapping the Moho with seismic surface waves: A review, resolution analysis, and recommended inversion strategies, Tectonophysics, 609, p. 377-394.

- Longuet-Higgens, M. S. (1950), A theory of the origin of microseisms, Philos. Trans. R. Soc. London, 243, 1-35.

- Nishida, K., H. Kawakatsu, and K. Obara (2008), Three-dimensional crustal S wave velocity structure in Japan using microseismic data recorded by Hi-net tiltmeters, J. Geophys. Res., 113, B10302, doi:10.1029/2007JB005395.

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