Solid Earth Discuss., 6, C247–C256, 2014
www.solid-earth-discuss.net/6/C247/2014/

Solid Earth
Discussions
Open Access

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.

mahsa.etedal@gmail.com

Received and published: 10 April 2014

Reply to the referee#2: We would like to express our appreciations to very careful review of our paper, and for the comments, corrections and suggestions that ensued. A major revision of the paper has been carried out to take all of them into account, and we hope that they met the referee's approval. We answered all the questions, comments and suggestions in details below 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. Some 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 #2:

1. Figure 1 is hard to understand the geology and tectonics of the study area. For example, what are “WM” and “EM”? I recommend replacing the topographic map in Fig.1(a) with the geological map in which tectonic blocks discussed in this paper are clearly shown.

Reply to Referee #2:

Following the comment we add sentence below based on Byrne et al., 1992, in Fig. 1, caption

WM: Western Makran (near 59.5’E), EM: Eastern Makran (near 63.5’E),

A geological map indicates geological features, rock unit or geologic strata exposed at the surface, however, our main aim in this article is to study the deeper part e.g. crust and upper mantle structures/transitions. Therefore, we used topographic map because it depicts the tectonic features besides showing Moho map which can help to study the changes in surface topography. However, if the referee insists we can replace the topographic map with geological map in next revision.

2. In lines 6-9 in Page 4, the authors describe the change in seismic activity across the Sistan Suture Zone. In Fig.1 (b), however, we see no significant difference in seismicity. The authors should designate an area discussed in this part of the text using, for example, arrows and add more clear explanation.

Reply to Referee #2:

In Fig. 1b we intend to show the geometry of slab by plotting the earthquakes larger than Mw 4.0 from global CMT catalogue. Plotting all local and small earthquakes make

SED
6, C247–C256, 2014

the map very busy and useless for our main aim in this article. However, the plot of seismicity is modified in the current version of the manuscript to highlight the change in seismic activity across the Sistan Suture Zone. We modified Figs.10 and 11 of the revised version of the manuscript. We plotted earthquake locations on tomographic map which might be helpful to indicate the seismicity of the Sistan Suture Zone.

"The 1994 Sefidabeh earthquakes in eastern Iran: blind thrusting and bedding-plane slip on a growing anticline, and active tectonics of the Sistan suture zone" by Berberian et al. 2000, was added which would give the reader more information.

The main text was also modified as below (page 3 lines 13-19):

Across the Sistan Suture Zone this seismicity pattern changes to low seismicity condition compared to the Zagros region. Recent and active deformation in Sistan is dominated by right-lateral strike-slip and thrust faults, related to the indentation of Iran by the Arabian shield (Berberian et al., 2000). Large earthquakes with surface rupture are known further north in the Sistan Suture Zone including the Mw 7.2 Zirkuh earthquake of 1997 (Berberian et al., 1999).

3. In Page 5, the authors described that they used ambient noise tomography. Why is this method superior to the other methods including seismic tomography used in the previous studies. The authors also should describe more clearly their own aims in this paper from geological/tectonic view points on the studied area.

Reply to Referee #2:

The manuscript was modified as below, page 4 line 7-14:

The purpose of this study is to produce Rayleigh wave group velocity maps with higher resolution than previous surface wave maps that have been produced in the Makran region in order to study the crust and the upper mantle structures/transitions in the Makran subduction zone. Compared to our understanding of active tectonics, much less is known about the Makran subduction zone and the seismic behaviour of the

SED
6, C247–C256, 2014

Makran subduction zone has remained largely unknown. Also there are basic limitations to earthquake-based surface wave tomography because of seismicity and poor station coverage.

4. Some technical descriptions in “Introduction” (lines 20-29 in Page 5) should be moved to Sec. 2 or Sec. 3.

Reply to Referee #2:

To follow the comment, we moved the sentences to Sec. 2 see page 5 lines 2-10

5. In page 11, the authors adopted the results from the first iteration. If this is the best, the authors should provide an example in which the solutions at the 1st iteration are the best as compared with those from other (later) iteration steps.

Reply to Referee #2:

We modified the text as below (page 10, line 12-26):

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).

SED

6, C247–C256, 2014

6. Tomographic results are difficult to understand. The authors should clearly indicate discussed areas/patterns by arrows or appropriate symbols.

Reply to Referee #2:

To follow the comment, we modified Fig. 10.

7. In lines 19-23,page 13, the authors describe a transition from low to high velocity. In Fig.8, however, a boundary between the low velocity and high velocity blocks seems to extend in west-east direction, and does not coincide with the Sistan Suture Zone. So I say again that the authors show more clearly areas they intend to explain. Where is the “transition”? From the present explanation, I do not understand that this suture zone is a segment boundary.

Reply to Referee #2:

According to other comments, we tried to explain the Sistan Suture Zone in a clear form, however we agree with the statement given by the referee that west-eastern boundary between the low velocity and high velocity blocks does not coincide with north-south direction of the Sistan Suture Zone. We modified the main text according to the comment as below (page 12, line 20-24).The “Discussion” section has been updated and we know focus on the main outcomes of the results.

In central Makran between the Sistan Suture Zone and the Lut block a transition from low to high velocity is observable. Although Byrne et al., 1992 assumed that this suture zone separates Lut and Helmand blocks, however, our results show that this suture dose not appear to segment different blocks.

8. Letters in Fig.9 are hardly recognized, which also makes difficult for readers to understand the results. Some improvement is required.

Reply to Referee #2:

The Figure was modified according to the comment.

SED

6, C247–C256, 2014

9. In Page 14-15, the authors discuss the comparison of seismic activity with their tomographic images. Why do not the authors plot earthquake locations on their tomography results in Figs. 8 and 9. It is really interesting and important for this paper.

Reply to Referee #2:

Following the comment, we modified Figs. 10 and 11 of the revised version of the manuscript and plotted the earthquake locations.

10. The paper by Shad Manaman et al. (2011) was referred at line 15 in Page 14. But some explanation on this paper is given earlier part of the text (line 25 in Page15). I think that such explanation should be given where this paper is firstly referred. For me, the method by Shad Manaman et al. (2011) is not understandable. What is the partitioned waveform inversion? The last two paragraphs in Page 15 seem to be redundant.

Reply to Referee #2:

The manuscript was modified as below, page 13 line 2-8:

The low-velocity anomalies beneath the volcanic arc on the maps are similar to those at 16, 20 and 24 s which reveals that the crustal thickness below the Taftan volcano is about 50 km deep, which is compatible with the latest Moho Map obtained for same area by using different approach and data by Shad Manamanet al., 2011 (Fig. 13). The Moho map produced by using surface-wave tomography method to image the S-velocity structure of the upper-mantle and Moho-depth.

And also page 13 line 29-32:

Using the latest Moho Map obtained for same area by different approach and data by Shad Manamanet al., 2011, we investigated the crustal thickness. To be more accurate in analysis we used high resolution version of the Moho map in Shad Manaman et al., 2011, illustrated in Fig. 13.

Partitioned waveform inversion (PWI) method, described in detail by Nolet, 1990 and

SED
6, C247–C256, 2014

van der Lee and Nolet, 1997. The PWI method is applied in two steps: in the first step, the 1-D average S-velocity model and average Moho depth is determined for each event–station pair using a nonlinear waveform inversion. The results are given based on absolute S-velocity variations with depth for each event–station pair. In the second step, the 1-D S-velocity models obtained in the previous step, are first reformulated (normalized) with respect to a common background model (e.g. iasp91 of Kennett and Engdahl, 1991) and are then combined using a damped linear inversion algorithm to image 3-D S-velocity perturbations and Moho depth variations for the studied area. The method is essentially the same as surface wave tomography therefore we will use "surface wave tomography" for the title in order to avoid confusion.

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, however, if the referee insists the last two paragraphs in Page 15 can be removed from the next revision.

11. Earthquake activity related with the Sistan Suture Zone is unclear for me although some events are distributed along the fault. I recommend the more explanation in the text and the improvement of Fig. 1b or the other related figures.

Reply to Referee #2:

We have updated Figs. 1, 10 and 11. Thus, we think that earthquake activity related with the Sistan Suture Zone is clear now. The main text also modified as we mentioned in comment 2, see above.

12. "SSZ" described at line 12 in page 16 is not found in Fig.11.

Reply to Referee #2:

Fig. 13. was modified.

13. In my understanding, the authors discuss discrepancy between their result and that

SED
6, C247–C256, 2014

by Shad Manamen et al. (2011) at the eastern edge of the Straits of Hormuz. They attribute this discrepancy to the structural complexity. This explanation is too simple. They should also consider the difference in methodology and data.

Reply to Referee #2:

As Yao et al., 2006 show in the paper the dispersion curves from ambient noise and surface-wave are very similar in character therefore the methodology and data is not very effective in the results and also because of similarities between our results and the results obtained by Shad Manamen et al., 2011 at main tectonic structures (page 13, lines 2-8, page 14, lines 1-11) we came to the conclusion that the difference at the eastern edge of the Straits of Hormuz is probably due to resolution in the data. Results obtained by Shad Manamen et al., 2011 has poor resolution in this region ($2^\circ \times 2^\circ$), however, we reach to higher resolution in this region ($1^\circ \times 1^\circ$). Therefore we didn't attribute this discrepancy to to the methodology.

We modified the text as below (page 14 lines 27-32 and page 15 lines 1-2):

Since the methodology and data is not very effective in the tomography results (e.g., Yao et al., 2006) and also because of similarities between our results and the results obtained by Shad Manamen et al., 2011 at main tectonic structures such as Taftan-Bazman volcanic arc and Sistan Suture Zone, we came to the conclusion that the difference at the eastern edge of the Straits of Hormuz is probably due to resolution in the data. Results obtained by Shad Manamen et al., 2011 has poor resolution in this region ($2^\circ \times 2^\circ$), however, we reach to higher resolution in this region ($1^\circ \times 1^\circ$).

14. I think that the authors discuss a lot of things in Sec. 6, but they seem to be diverse. What is the most important finding in this paper. Selecting their finding, the authors can construct a schematic structure model in their study area. This is very important in understanding what the authors intend to explain.

Reply to Referee #2:

SED
6, C247–C256, 2014

We removed a paragraph related to two focal mechanisms and also those parts of the discussion that we compare our results with the Moho map (Fig. 13) and modified Fig.1a. Also we have added symbols/text to Fig. 10a in order to represent our main findings instead of schematic map. We modified some parts of the "Discussion" section including the text given below (Page 12, lines 20-24):

In central Makran between the Sistan Suture Zone and the Lut block a transition from low to high velocity is observable. Although Byrne et al., 1992 assumed that this suture zone separates Lut and Helmand blocks, however, our results show that this suture dose not appear to segment different blocks.

References:

Kennett, B. & Engdahl, E., 1991. Traveltimes for global earthquake location and phase identification, Geophys. J. Int., 105, 429–465.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/6/C247/2014/sed-6-C247-2014-supplement.pdf

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 are very grateful to your comments and thoughtful suggestions. Based on these comments and suggestions, we have made careful modifications to the original 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 referees are marked bold below. All changes and modifications are bold in the revised version of manuscript.

With the best regards
Mahsa Abdetedal

PhD student at
Institute of Geophysics,
University of Tehran, Iran

1

Fig. 1. reply-letter

SED
6, C247–C256, 2014

Interactive Comment