Solid Earth Discuss., 5, C634–C637, 2013
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Interactive comment on "Seismic structure of the lithosphere beneath the ocean islands near the mid-oceanic ridges" *by* C. Haldar et al.

Anonymous Referee #1

Received and published: 9 December 2013

The manuscript topic is extremely interesting, and the authors started a great work, that can add new information to the actual knowledge of the lithosphere system. Indeed they analyze the receiver functions obtained at 5 stations located in the vicinity of the mod-oceanic ridges (MOR), in order to estimate crustal and lithosphere thickness. It seems to me though, that the proposed velocity models have not been properly tested and some presented features are not required by the data. For this reason I suggest a major review of the manuscript before being considered for publication. I list in detail my observations below.

A. Section 3. The author interprets the RF by using velocity models constructed by referring to previously defined velocities from active seismic. These 1D simple models are used to constrain the seismic velocities in the upper crust only. I found three main

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problems with the modeling of the deeper discontinuities:

1- There is no description on how the modeling has been done. A description of the modeling procedure is mandatory.

2- Because of the lack of modeling description, I assume that the velocity model at each station has been constructed by trial and error, simply modifying manually the initial parameters to obtain a synthetic wiggle similar to the observed RF. This kind of modeling, could be useful as starting indication to explore the different possibilities and rule out improbable models. It is not enough though in order to give meaningful, quantitative constraints on the deep structure. A clear example of this is the model proposed for station ROSA. The velocity drop at the LAB goes from 4.5 to about 3.4 km/s. This would correspond to 24% velocity decrease at the LAB, which is a value way larger of what I have previously seen. Normally the velocity drop at the LAB is in the order of 2-4%, it can reach 10% or 1% as extreme maximum and minimum values, but not 24%. If the author wants really to infer such a velocity decrease, he should comment this result, and argument it referring to previous literature. Moreover the velocity model at this same station (ROSA) displays a positive velocity jump at the Moho that is extremely strong. According to my experience, a 2 interface model for the crust (including one shallower layer as made of low velocity sediments) would produce multiple phases that could explain the negative pulse at about 4 s, and would give a more realistic positive jump at the Moho. According to me the author did not spend enough time in modeling. There are different ways of retrieving the interface depth, either by depth migrating the RF or by performing a search in a defined parameter space (e.g. Sambridge M. 1999a. Geophysical inversion with a neighbourhood algorithm – I. Searching a parameter space, Geophys. J. Int., 138, 479–494, or e.g. Vinnik L.P., Reigber C., Aleshin I.M., Kosarev G.L., Kaban M.K., Oreshin S.I., Roecker S.W, 2004. Receiver function tomography of the central Tien Shan, Earth planet. Sci. Lett., 255(1–2), 131–146).

3- Since different combinations of velocities and thicknesses for the strata have not been tested, there is also no information on the errors estimates on depth and Vs. In

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table 1, depth uncertainties are stated, but there is no explanation in the text on how these uncertainties have been determined. The errors on velocity and layer thickness must be included in the model description.

B. Page 4, Lines 15-20. It is no clear in this description how the RFs have been calculated. Which technique has been used? Are there any references? Did the authors write their own code? These questions have to be addressed in the manuscript.

C. Page 6, Lines 5-8. This comment is also linked to the next comment (D): The argument of the triple junction has been cleared in 3 lines, without any description. The author first have to explain better where the stations are located with respect to the MOR and to the triple junction (since this is not clear so far), and then give arguments on how the presence of the triple junction influences the structures below ROSA and PSCM.

D. Page 5, Line 20. The author write that PSCM and ROSA are at about 10 km away from the MOR. It seems to me instead, that ROSA and PSCM are $\sim$200 km away from the Mid Atlantic ridge, while PSCM is on the Terceira Rift, while ROSA is about 100 km away from it. Section 3.2. The author writes that station RPN is close to the Rano Kau Ridge, and station MCQ is in the Middle of the Macquarie Ridge. I would suggest, besides the first global map with the station location, to add smaller maps for each station location together with toponym.

E. Page 5, Line 15. The author states that in the S-velocity model for station PSCM he needed to insert a top low velocity layer, but in the model I cannot notice any of this. Where is it? Is there an error in the text or in the figure?

F. Figure 2 and Paragraph 3.2. For station MCQ the LAB pulse is so close to the STD of the RF to instill the doubt that the pulse could be real. I think that too much emphasis has been given to the interpretation of this negative pulse. Moreover an explanation of why the LAB at this station is so much deeper with respect to the average from other stations is lacking. References to studies in the same region, or in other geographic

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regions displaying similar lithosphere structure must be recalled when discussing this result.

G. Section 5. Line 25. More references are needed.

Page 2, Line 16: locales. . .. . ..generated → locus of new lithosphere generation

Page 3, Lines 7-8: particularly. . ...comes → particularly near the ridges, comes

Page 4, Lines 1-2: data. . ..together → data recorded at stations located on five oceanic islands near MORs (Figure 1), together..

Page 6, Line 1: PASCM → PSCM

Interactive comment on Solid Earth Discuss., 5, 1641, 2013.