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Interactive
Comment

Interactive comment on "Seismic structure of the lithosphere beneath the ocean islands near the mid-oceanic ridges" *by* C. Haldar et al.

C. Haldar et al.

prakashk@ngri.res.in

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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 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.

REPLY: We have now described the modeling procedure in the text. At each station, the stacked traces are inverted to obtain models that best fit the data.

2- Because of the lack of modeling description, I assume that the velocity model at

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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).

REPLY: It is correct that the forward modeling that we adopted earlier, was performed by iteratively modifying the initial parameters to obtain a good fit between the observed an synthetic traces. We have now performed an inversion of the stacked traces at each station to obtain the models in a less subjective manner. A discussion about the velocity contrast across LAB has been discussed in the Sub section 4.3. Also now, we have changed out Table accordingly. After inversion, we have the gradation discontinuities, so we have tabulated only the depth values.

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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 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.

REPLY: Now we inverted the receiver function traces. We incorporated the description of the modeling in the text and error analysis.

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.

REPLY: Now we have mentioned in the DATA and Method section. We did mention the references of the methods used for receiver function analysis (e.g. Burdick and Langston, 1977; Langston, 1977; Vinnik, 1977). We have now included a description of the technique used for calculating the P-receiver function. Regarding the computation, the analysis has been done in Seismic Handler (already mentioned in the Acknowledgments). We have now modified Figure 1 by zooming the regions around the stations, in order to have a clear idea of their relative locations with respect to the mid-oceanic ridges. We now explain how the triple junction could have a possible influence on the structure, although we find it difficult to find an explanation for the difference in plate thickness values below ROSA and PSCM except that ROSA is closer to the main north-Atlantic ridge as discussed in subsection 4.1.

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 âĹij200 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.

REPLY: It is true that PSCM and ROSA are about 200km away from the Mid-Atlantic ridge, however, there is another ridge passing north of Africa that forms an arm of the triple junction. We regret the mistake in addressing this rift as the Mid-Atlantic ridge. Please see the inset where PSCM and ROSA are shown separately. Station MCQ is in the middle of the Macquarie Ridge (e.g. R. A. Duncan and R. Varne, 1988; Ian Tapley, Arjan H. Dijkstra and Henk Brolsma, 2004). As per the suggestion, we have now included inset maps for each station separately in the same Figure 1.

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?

REPLY: Thanks for pointing out the mistake. We intended station ROSA and not PSCM. Now it is corrected.

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

REPLY: Now we did inversion and the depths are constrained better. The values for

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LAB depth for MCQ has come out to be ∼49km. Now we modified the text accordingly.

G. Section 5. Line 25. More references are needed. REPLY: We have added references for RF only.

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

REPLY: Change made

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

REPLY: Change made

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

Page 6, Line 1: PASCM → PSCM

REPLY: Now it is changed to PSCM

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

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