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Solid Earth
Discussions

Interactive comment on "Crustal structure of southeast Australia from teleseismic receiver functions" *by* Mohammed Bello et al.

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While a response to comments is provided as plain text below, we recommend that the full response document, including marked-up revised manuscript and figures, be accessed via the supplementary PDF file provided. It is easier to navigate.

Comment: In the present manuscript, the authors calculated and evaluated Ps receiver functions at a total of 32 seismic stations in southeast Australia, most of them situated along both sides of Bass Strait (Victoria and Tasmania). They applied H-K stacking and receiver function inversion using the neighborhood algorithm, relatively standard techniques that have already been applied to different datasets in E and SE Australia.

The performed processing and analysis was carried out competently, the obtained results appear to be mainly solid, and the manuscript is overall well written. However, I have large reservations about the study's significance. It basically just provides a few (way fewer than is apparent at first glance, see comments below) new data points that do not really show us anything new, and it also does not attempt to gain new insights by combining the obtained information with other existing evidence in a meaningful and potentially novel way. I thus think that the study should not be published in its current shape, but the authors should be encouraged to submit an extended and improved version of the study that tries to, at least, improve on point 2 of my General comments (see below).

Response: We thank the reviewer for their constructive criticism of the manuscript. We acknowledge that the receiver function results we obtain only represent a portion of the stations for which data are available, but we were very careful to remove poor quality data, and this resulted in fairly substantial culling of the dataset, which is inherently noisy due to the proximity of most stations to Bass Strait and the Southern Ocean. An extensive re-examination of the data has been made in response to this point, including re-processing the receiver functions and a less automated assessment. As a result. we have been able to add an additional four H-κ stacking results for the portable network (stations BA03, BA09, BA19 and BA20) and eight NA inversion results for the portable network (BA02, BA07, BA08, BA09, BA13, BA17, BA19 and BA20) – see the revised Table 2. These new results make a substantial contribution to the revised paper, and has meant that the Abstract, Discussion and Conclusion sections have been substantially modified. We also take the point regarding the exploitation of existing evidence to improve our interpretation, and have put considerable effort towards achieving this in the revised manuscript.

Comment: As mentioned above, my main concern with this paper is its lack of significance. This can be broken down into two problems:

1. Data paucity and lack of novel results The abstract talks about receiver functions from 32 stations (24 temporary from the BASS deployment, 8 permanent), but H-K

results are only supplied for 13 stations(7 temporary, 6 permanent; the text says 14 stations but Table 2 only features 13), inversion results only for 6 (only permanent stations).

What the manuscript does not mention is that Moho depth estimates from receiver functions are already available from literature for 5 of the 6 permanent stations investigated (station CAN in Clitheroe et al., 2000; stations MOO, TOO, TAU, YNG in Ford etal., 2010; all 5 are also used in the AusMoho compilation of Kennett et al., 2011), and vp and vp/vs estimates for 4 of the 6 (Ford et al., 2010). This reduces the amount of new results to H-K stacking results from 7 temporary stations, and H-K stacking plus inversion for one permanent station (CNB) for which I could not find previous results.

This is a rather thin data base, and I think the authors should have mentioned the previous results I just listed, and discussed whether their new values agree or disagree with these previous findings (they are largely consistent, as far as I can see). Failure to do that appears to wrongly imply that all reported values are novel. Comparison is only undertaken with other previously analyzed stations in the region (Figure 9), which adds to this impression. Lastly, it would make sense to compare the obtained crustal thicknesses to the Australia-wide Moho model AusMoho (freely available from the webpage of ANU:http://rses.anu.edu.au/seismology/AuSREM/AusMoho/). This interpolated model offers predictions (interpolated values) for the positions of the newly analysed stations, thus it offers the possibility to check whether these results change or confirm the cur-rent state of knowledge (at first glance, they rather confirm).

Response: We acknowledge that what we wrote could be interpreted as a claim that we obtain receiver functions from all 32 stations, so have added a subsequent sentence that clarifies the actual number of usable receiver functions we extract (see lines 13-15). As noted above, we have re-assessed the temporary station data, and applied different receiver function assessment criteria, which has yielded additional results that we hope will help allay the reviewer's concern regarding data paucity.

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Discussion paper

In response to the second point, we now make mention of the previous Moho depth estimates for five of the six permanent stations, and show that they are consistent with our results. See lines 398-407.

In response to the last point, we have added a new figure (Figure 11) which compares all of our Moho depth results with AusREM estimates. As the reviewer notes, they are in general quite consistent, but with a few exceptions that we discuss in a new section (7.3) in the manuscript – see lines 560-597.

Comment: 2. Lack of constraints for interpretation The authors offer a detailed introduction to models of crustal formation and geologic evolution of SE Australia in Sections 1 and 2. I am no expert in these things, but I get the impression that a thorough survey of the literature was performed. The Discussion section then attempts to relate the rather poor data base (see above) to all kinds of geological processes that have been previously proposed. I think the authors are doing an OK job in relating their results to some published works, but the central problem is that the few newly obtained data are mostly interpreted in isolation. It would make a lot of sense to do cross plots with results from other geophysical studies, trying to see more by combining different datasets. This is not done at all, which is even more surprising considering that the first author published two other seismological studies on the same area, even partially using the same stations, last year (Bello et al.,2019a,b; teleseismic tomography and shear-wave splitting). While both of these methods rather illuminate deeper, sub-crustal structure, a combined interpretation would allow a much better discussion and potentially offer novel insights. I wonder why this is not done here, unless the authors want to publish this in yet another paper (which would be slicing it rather thinly). Looking at these previous papers, I also wonder why there was no attempt to use at least some of the huge number of WOMBAT temporary stations that was harvested for the teleseismic tomography in the present study, to derive some more (novel) data points.

Response: The reviewer makes a fair point with regard to the lack of comparison with the results from other relevant studies. As a consequence, we have made the following changes: 1. As noted above, we now compare our results with the AuSMoho model (Figure 11 and Section 7.3). 2. We also compare our results in Tasmania to the study of Rawlinson et al. (2001) who invert wide-angle traveltimes (both reflection and refraction) for crustal velocity and Moho geometry (see Supplementary Figure S9). 3. We also include a new figure, which provides a joint interpretation of our results from teleseismic tomography, shear wave splitting and receiver function analysis (see Figure 12) and discuss it in detail in a new section (7.4) – see lines 598-623.

With regard to the last point, the WOMBAT stations were all short period stations (1 Hz corner frequency), many were only vertical component, and deployment times were often less than a year. All the Victorian stations and those deployed in northern Tasmania were vertical component, which means that it was not possible to extract receiver functions from them.

Comment: ll.24/25: I found it rather confusing that the authors talk about vp/vs ratio and Poisson ratio separately (here and also in Section 7.2), although these properties are directly related (see Equation 2) and thus one does not offer additional information compared to the other.

Response: We agree with the reviewer's comment, and have revised the manuscript so that it just refers to Vp/Vs ratio (see Abstract and Section 7.2).

Comment: ll.54-57: Some parts of a sentence are apparently missing here.

Response: As far as we can tell, the sentence in question appears to be complete. However, we have broken it up into two sentences, which hopefully improves its clarity (see lines 67-71).

Comment: ll.93-100: If VanDieland is just a conceptual microcontental block in one of the models that is routinely used to explain the genesis of the region, why is the term used to reference station locations (e.g. Table 2). Shouldn't geographical regions that are independent of interpretation framework be used for this?

Response: This is a fair point, and we have now removed such references (e.g. Table 2).

Comment: ll.102-154 (Section 3): I personally dislike this type of listing of existing studies, going study by study and explaining the methodology of each. This is unnecessarily bloated and in the end the reader doesn't take away much beyond "people have worked in this area before". It would be better to include the geophysical evidence into the presentation of evolution concepts given in Section 2 (and partly Section 1).

Response: We think it is worthwhile to summarise the results of previous geophysical studies, so have retained this section. However, we do acknowledge the reviewer's point, and therefore have changed it considerably to focus more on the outcomes of these studies. This includes largely removing the first paragraph, which merely lists the range of techniques that have been applied and relevant references. See lines 116-185 of the revised manuscript.

Comment: l.152: part of the sentence is missing (?)

Response: This sentence appears to be complete as far as we can tell.

Comment: ll.174/175: I disagree on this claim

Response: We have deleted this sentence (see lines 221-222).

Comment: l.185: "by using the clarity of the direct arrivals"; was this a purely visual selection or were there fixed criteria? Some more detail would be useful.

Response: We have added "visual clarity" to the sentence in question (see line 240).

Comment: l.208: Although the paper of Zhu and Kanamori (2000) is cited here, the used weighting scheme (0.6/0.3/0.1) is not the same as in that paper (0.7/0.2/0.1).

Response: We have removed the citation (see line 270-271).

Comment: l.213: How robust is the use of standard deviations as uncertainty estimates when there are only 4-6 measurements (as is the case for 5 of the 13 stations, see Table 2)? This should at least be mentioned/discussed.

Response: This is a fair point, and we have added an additional sentence to explain the limitations of this approach (see lines 276-278).

Comment: l.240: "Our strict criteria ...": what were these criteria? It would be worth explaining how this was done, especially since it leads to a reduction from 32 to 6 stations.

Response: We have rephrased this sentence to make it clear that we are using visual criteria for acceptance (see line 305-306).

Comment: l.243: Why have a subchapter 5.3.1 if there is no 5.3.2?

Response: We have removed this heading (see line 308).

Comment: l.294ff: Why is it not even mentioned that there is a huge difference in Moho depth between the two different applied analysis techniques (H-K and inversion) for all (3) stations in the Lachlan Fold Belt that were investigated with both methods (see Table2)? These differences are around 10 km, thus very significant.

Response: We have revisited these Moho picks from the NA inversion, and determined that they were not done correctly. As can be seen in Table 2, they have been changed, with YNG H-κ depth=37 km, NA depth=35km; CAN H- κ depth=39 km, NA depth=40 km; CNB Hκ - depth=38 km, NA depth=39 km. However, it is worth noting that previous RF inversion results have favoured a Moho that is $\sim$10 km deeper beneath this region. We have discussed this in Section 7.1 – see lines 432-440.

Comment: Figures 6 and 9: I find the black-to-white color scale not to be a very good choice here, it is quite hard to see at a glance where e.g. the crust is thick and where thin. A different color scale may be more appropriate.

Response: These figures have been redone using a colour scale that makes it easier to distinguish between thick and thin crust - see Figures 6 and 10.

Comment: Figures 7 and 8: It would be useful to show where the Moho was picked in the shear-wave velocity models. Taking the values from Table 2, I actually disagree with the picks for stations YNG and CAN. In both of these cases, the clearer jump in vs is much shallower than what is listed in Table 2 (around 35 and 40 km instead of 48 and 49km), which would also be much more consistent with H-K results.

Response: The pick locations have now been included in Figures 7-9, noting that we also include two example RF inversions from the temporary array in the new Figure 9. As noted in a previous comment, the original picks for YHG, CAN and CNB were too deep, and have been revised, so the reviewer is correct. However, please also refer to lines 432-440 of the manuscript for an explanation of why the precise Moho depth might be difficult to estimate here.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-74/se-2020-74-AC1-supplement.pdf

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