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

Anonymous Referee #1

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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

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Discussion paper

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 I thus think that the study should not be published in its current shape, but the authors should be encouraged to submit an extended and improved version of the study that tries to, at least, improve on point 2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).

General comments:

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 et al., 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 current state of knowledge (at first glance, they rather confirm).

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.

In the following I will supply more minor comments by line number:

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.

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

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?

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).

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

ll.174/175: I disagree on this claim

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

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).

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.

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.

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

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 Table 2)? These differences are around 10 km, thus very significant.

Figures:

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.

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 49 km), which would also be much more consistent with H-K results

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