

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: This carefully researched and well-written contribution places solid constraints on the crustal structure of southeast Australia by the construction and inversion of teleseismic receiver functions underneath a series of high-quality seismic stations. Building on these results for the thickness and sharpness of the crust, the authors put forward a tectonic interpretation, or rather a substantiation of earlier geological theories, involving magmatic underplating, which places the structure of the region into a

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proper geodynamic context.

I have relatively little to offer in the form of scientific criticism or comments on the seismological methods, which are sound, well-established, and well executed, although I am making a number of suggestions related to the presentation of the materials.

I am judging the paper primarily on its seismological merits, and not on the finer points of the interpretation. My main point related to the interpretation is that the comparison with earlier results by other authors is mostly qualitative, in the form of a color-coded figure, where I would have preferred a more detailed cross-comparison including a statistical analysis of uncertainty. How different can two crustal models made at two nearby stations be before tension develops with the interpretation? How different can two crustal models made at the same station be before we must dig into the details in order to interpret one of them as “better”, or both of them as “equivalent”? The authors leave a bit of material on the table here.

I am attaching a hand-annotated manuscript. I will number and restate my most important comments here. I will not repeat “obvious” but necessary corrections here.

Response: We thank the reviewer for the positive comments, and in the revised manuscript, attempt to improve on the quantitative nature of the comparison with previous results.

Comment: L261 What are those degrees of freedom, how do you determine them? The reference to Gouveia and Scales is too vague.

Response: The degrees of freedom is equal to the number of observations minus the number of inversion parameters, which we now state in the manuscript (see lines 328-329).

Comment: L310 In the same vain. I know it is hard to formally justify, but if you have the right number of degrees of freedom, and you have the right amount of independence in the entries of the summand, the reduced-chi-squared value that you should be aiming

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for is 1. Are you looking at the distribution of your misfits to establish that they ARE indeed chi-squared distributed? Are you sure that you are using the right amount of degrees of freedom? Are you sure that your lowest chi-squared values are not overly optimistic (as in: that they could be nearly perfect fits to models with too many free parameters).

Response: This is a fair point, and ideally one would be aiming for a value of 1 to fit the data. However, apart from getting the number of degrees of freedom right, the noise estimate is also a factor, and its absolute value is poorly constrained. This may be why the chi-square values are on the low side, but we think it is reasonable to consider our measure of chi-square as a relative indicator of data fit, which we now acknowledge on lines 393-397. It is also worth noting that it is fairly typical of NA RF inversion studies to end up with chi-square misfit values well below 1 (e.g. Wu et al., 2015: Crustal shear wave velocity structure in the northeastern Tibet based on the Neighbourhood algorithm inversion of receiver functions. *Geophysical Journal International*, 212, 1920-1931)

Comment: L832 I assume we are talking about the same criterion here, and so the caption should explicitly refer to it.

On the whole, I would like to read more about your misfit criterion, and I would like you to make explicit the now implicit distributional assumptions made about your metric.

Response: Yes, this is the same misfit measure, which is now clarified in the paper (see line 1014).

Comment: L831 I definitely would put the numbers in call-out boxes on the maps also. A color scale is hard to read for some, and any additional clarity that can be gleaned from a multiplicity of representation is to be welcomed.

Response: We now point out the depth values on the plots.

Comment: L835 Let the caption teach us how to read the top and bottom axes in the

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left-hand panel.

Response: We have changed the caption as requested (see line 1012).

Comment: L850 Again, it is hard to see differences when they are presented on a busy colored map in a smooth gray-scale representation. A table would be nice in the main text. Spell out the differences, attempt to make sense of them relative to their uncertainty and their spatial proximity. Make us confident that your study is not just “another opinion”, make us confident that other studies weren’t just “another study”, in other words: integrate the results of your and other studies and talk us through the similarities and differences. In the text, emphasize the common points and the differences, in particular in light of the interpretation.

Response: We have changed the relevant figures (Figure 6 and 10) to make it easier to read the variations in Moho depth, and changed the discussion to make it more integrated, along the lines suggested by the reviewer. Table 2 also provides a quantitative summary of all our new results.

Comment: L10, L15, L26 “understanding”, “this”, “explains” -> those are all vague terms. After reading the manuscript it became clear to me that you had more detail in mind, some of which you have room to put into the abstract.

Response: We have made some modifications to the Abstract to improve upon clarity.

Comment: L17 “postulated Precambrian continental” -> I propose “putative” if the postulate refers to the fragment being “continental” or “putatively” if it refers to being “Precambrian”.

Response: We have implemented this change (see line 21).

Comment: L50, L55 -> Establish a consistent notation and typographical conventions

Response: This has been done.

Comment: L150, L152, L186, L186 -> Fix typos and inconsistencies

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Response: This has been corrected.

Comment: L287 “relatively average to high” -> we need a basis for comparison, and a different word than “average” - in my book, values are not “average” unless they are “averages”, and you most likely mean that these values are “unremarkable”, “usually/frequently observed” (compared to what then?)

Response: The paragraph containing this text has been deleted as part of the revisions.

Comment: L325 -> Fix typo/inconsistency

Response: Typo has been corrected.

Comment: L376 There is a lack of referencing in this sentence, which must refer to specific studies for each of the assertions made in it. Also “depicted” is not the greatest choice of word here.

Response: We have replaced “depicted” with “revealed”, and included a reference to the work of Christensen (1996). The reference to Owens and Zandt (1997) in the second point refers to the relationship between partial melt and Poisson’s ratio. See lines 487-489.

Comment: L447 -> Fix typo/inconsistency

Response: Done.

Comment: L777, L788, L791, L809 -> Fix capitalization

Response: Done

Comment: L798 Personally I would leave ETOPO1 out of the caption unless I was willing to put a color scale to it. At this scale and with this projection and without a color scale it’s immaterial what topography model is being used.

Response: We agree, but it is a journal requirement to state the source of any information we use in figures that was obtained from outside the current study.

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Comment: L802 I would label the phases with letters on the graph also, right now the colors are not all that distinct on the screen, and they won’t be on a black and white printer or photocopier, either.

Response: This suggestion has been implemented – see Figure 4.

Annotated manuscript: We also implemented the minor hand-annotated suggestions in the manuscript provided by Reviewer 2, which were mainly typos and other straight forward edits.

Please also note the supplement to this comment:

<https://se.copernicus.org/preprints/se-2020-74/se-2020-74-AC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-74>, 2020.

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