

Interactive comment on “The impact of sea-level rise on tidal characteristics around Australasia” by Alexander Harker et al.

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I recommend major revisions for this manuscript. The topic, tidal changes around the Australasia region, is interesting. The study adds to the literature on projected tidal changes in response to sea level rise with a high-resolution model of the Australasia region. As far as I can tell, the study has been done competently. The writing is generally clear, with some exceptions noted below. The main cause for my concern is that, as far as I can tell, the authors have used TPXO boundary conditions throughout their study. The TPXO boundary conditions are from the present day, meaning that the tides along the boundaries are not responding to changes in sea level rise. The fact that the regional model has some skill in simulating observed tidal changes suggests that maybe this is OK. On the other hand, the authors have a high-resolution forward global

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tide model available to them; why didn't they use it here? Global tide models would respond to the changes in sea level, thus providing more natural boundary conditions. Would the computational expense be too great? If so, say so, and provide some evidence for that, or at least make it more clear that we shouldn't worry too much about this. If it is feasible, I suggest that the authors use the global tide models to complement at least some of the simulations with TPXO boundary conditions.

We thank the reviewer for their constructive commentary about our work. Comments received about the implementation of the boundary conditions of the domain prompted a review and recalculation of our simulations, resulting in significantly different results. We moved from using elevations as prescribed by TPXO8 to those from a global forward model. For computational cost and time reasons this model was run using the M2 and K1 constituents only, making it necessary to limit the manuscript results to these constituents. Unfortunately this means our discussion of S2 no longer appears in the manuscript, and any suggestions pertaining to this section cannot be addressed.

Other important suggestions:

1) Where does the SAL term come from in this regional model? This is an important detail, that should be described.

The SAL term is derived from TPXO8 data. This has been clarified in Section 2.1.; "The model solves equations 1–2 using forcing from the astronomic tide generating potential only...and the SAL term is derived from TPXO8"

2) Page 5, lines 20-29: The 1 and 7 meter sea level rise values are justified, but the 3, 5, 15, and 20 meter sea level rise values are not explicitly justified. 3 and 5 lies between 1 and 7, the latter being an "extreme value" so I'm guessing that might justify the 3 and 5 meter values; but again, it would be nicer if the authors themselves made an explicit justification. And the 15 and 20 meter values are not justified at all.

Our new simulations have used 1, 3, 5, 7 and 12 m SLR. The 3 and 5 m values follow

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from Wilmes et al., 2017, (as the global mean sea level increase from the collapse of just the marine sectors of the West Antarctic Ice Sheet and the total collapse of the WAIS respectively), which is now stated in Section 2.3. Whilst 12 m may not be physically justified, it allows us to see how trends may continue; this has been stated in the text.

3) It seems to me that readers would take more away from the discussion of Figure 1 if the tide trends were compared to the MSL trends/increases. Are the tidal trends comparable? Other papers e.g. Jay 2009 have commented on this-in some regions, the tidal and MSL trends are comparable. This helps the readers to envision the societal significance of the tidal trends. I suggest adding some commentary on this for the Australasia region.

This is a good point. A brief discussion of this has been added to the introduction.

Minor comments:

Page 6 line 4-suggest "With a few exceptions, record lengths are short, but all . . ."

Page 6 line 20-suggest ". . .constituent amplitudes. . ." in place of ". . .constituents amplitudes. . ."

The above modifications suggested by the reviewer have been made where possible (some sentences referred to no longer exist in the manuscript)

Page 7, line 29-can the stated greater impact of sea level rise on tides be justified with a citation or some other source of information?

This comment was written with a current review paper by Haigh et al. (submitted to Reviews of Geophysics) in mind. We could cite it as "gray literature" here but rather prefer to wait, see how this reference paper evolves, and include it later during the editing process.

Page 9, line 8-suggest "amplification of the tide" (insert "the")

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Figure 6 caption – “W m $\ddot{E}E_2$ ” i.e. should instead be “W m $\ddot{E}E_2$ ”

Page 12, line 12-suggest “. . .Arafura Sea. The changes in S2 amplitude appear similar to the changes in M2 amplitude, including. . .”

Page 13, lines 3-5-suggest “Because S2 is a tidal constituent, its response. . .”

Page 13, line 6-suggest “In contrast to the M2 behavior, above 7 m. . .”

Page 13, line 10-the phrase beginning with “yet” sounds odd to me. The M2 and S2 dissipation patterns are similar but the M2 values are much larger. That is not surprising, so inserting a phrase beginning with “yet” seems out-of-place, to me at least. Minor point, but I suggest omitting this phrase.

Page 13, line 13-suggest “The K1 changes are relatively limited compared to the changes in the semi-diurnal constituents examined here.”

Page 14, line 6-this sentence reads awkwardly. Please improve the grammar.

Page 14, line 17-I believe that the word after “SLR” should be “on” not “of”

Page 15, lines 10-11-“impact to” should be “impact on”

Page 15, line 13-“model concerns” is an odd-sounding phrase

Page 15, line 15-“changes to changes”. Is this what you want to say?

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