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

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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.

GENERAL COMMENTS The present work, is, to my knowledge the first regional study

of SLR effect on tides in this area (Australasia), even if global studies already provided indications (e.g. Pickering et al, 2017; Schindelegger et al., 2018). In addition, a well-established model is used, together with published methods of analysis. This make this paper suitable for a potential publication. However, the paper lacks a description of the present-day tidal dynamics. The model validation lacks some elements to be fully convincing. In addition, the comparison of the observed trend and modelled trend rises questions on the SLR value choice. The paper is sometime difficult to read (especially when describing results per areas not indicated in maps). The provided physical explanations of the results deserve to be more strongly supported. Some figures and maps are probably lacking (regarding the text), and deserve to be in an Appendix.

MAJOR REMARKS 1.Site description The paper lacks description on the tidal dynamics in the study site, making the results more difficult to interpret. A minimum level of description should be provided. Maps of M2, S2, K1 amplitudes (and perhaps phase) would be useful.

The figure showing the bathymetry of the domain has been expanded to include a chart of M2, and a supplemental figure of K1 has also been included. We hope the additional references to this figure in the text and the expanded description of the control simulation in the Results section [page 10, line 4] help the reader to visualise the changes we describe.

2.Model validation Regarding the validation of the amplitude of M2, S2, K1, statistical information as correlation coefficient and bias would allow to better characterize the model errors, and also to better support the text. In particular, it is stated that the model overestimates K1. An explanation is given. But, looking at Figure 2, I have the impression that S2 is also over-estimated. If this is the case, then the explanation would not be reliable anymore. In addition, there is no physical explanations provided for the sites which are beyond the 2+/- standard deviation and these sites are not identified (we do not know where the model "fails"). Regarding the comparison between the modeled trend and observed trend, I have a concern on the SLR choice. Indeed, I *do not understand why using SLR=1m, rather than a more probable value for the last decades. The underlying assumptions (not stated in section 2.4, but stated later on) is that the changes are proportional to SLR. While this has been proved to be true in some locations, this is can be locally not true. The validation of trends deserve more attention, either by checking the proportionality of changes in the [0-1m] or using a more realistic SLR value (or a non-uniform SLR field) for the last decades. As to me Figure 3 and the text is not fully convincing, I strongly recommend to have a closer look on this point. In addition, the validation should also be done for S2 and K1.*

This comment contains several threads. Our responses to each of those threads are as follows:

(1) Validation of the M2 and K1 control run and discussion of outliers: we have reworked Section 2.4 [page 8, line 5-10] to explain model-to-data discrepancies beyond the 1-sigma-level (stations Williamstown and Geelong). Correlation coefficients and median absolute differences have been included as additional statistical measures. Note that upon exclusion of the two "outliers", RMS differences improve significantly w.r.t. the previous version of the manuscript.

(2) Possible overestimation of K1: even though our new Figure 2 contains slight hints of such an overestimation, we have refrained from speculations in this regard.

(3) SLR choice: we tried to redo both our new global and the Australian domain simulations with a few dm of SLR, but obtained suspicious tidal changes, possibly related to numerical artifacts arising in the model at such small values of SLR. Therefore, the validation is still done with the M2/K1 results for 1 m.

(4) Validation of trends for constituents other than M2: we have performed the necessary computations for K1 and show the corresponding figure in the supplement. Finally, we note that alongside SLR, other processes (e.g., ocean warming, thermocline deepening, changes in shoreline position, local anthropogenic influences such as periodic dredging) could have contributed to observed changes in the tides, so a comparison to

model values from SLR-only scenarios can't be fully convincing. Figure 3 rather documents partial success in capturing observed tidal variability at a number of stations. Additional formulations pertaining to this issue have been added to the text in Section 2.4.

3.Physical mechanisms Several times in the paper, the authors provide some explanation on the results (quality or SLR effect) using the words "probably", "presumably". This weakens the paper. As much as possible, the authors should provide more evidence to support their interpretations. As written in the discussion a series of numerical tests could be done to better asses the resonance and frictional effects. I strongly recommend to perform these experiments in the present paper to really support the interpretations. As a more minor remark, the model does not include advection terms. What could be the effect of neglecting this term on the present results? Is there any literature justifying to neglect it for tide modeling?

We have followed the first suggestion and removed many of the conjectures in the original manuscript. With the efforts and textual changes demanded by this revision and the new simulations, we think that more numerical tests fall beyond the scope of our study. As far as the advection terms are concerned, these can be neglected with little drop in accuracy (as per Egbert et al., 2004), with the added benefit of reducing the computational workload of the model runs. Also, test runs by one of the authors (M. Schindelegger) with another tidal model showed that M2 responses around Australia to a 2 m uniform SLR change by no more than 2 mm between simulations with and without advective terms.

4.Figures - Maps of M2, S2, K1 amplitudes are lacking.

Maps of M2 and K1 amplitudes are now included alongside the bathymetry and in the supplementary information respectively.

- The text relies on many results, which are not shown (e.g. tide changes of M2, S2, K1 for SLR different that the 1 and 7 m shown in the paper). Such figures would be

useful and could be added in Appendix.

The text is now more focused on what is shown in the figures and does no more rely on results for SLR other than 1 and 7 m. Note also that there are only slight variations in the spatial distribution of the tide changes for SLR different to 1 m and 7 m; we have therefore refrained from including these results in the appendix (or supplement).

- The text describes the results using the names of many locations. A map indicating all this locations is needed (a reader not knowing Australia will have to make a big effort to follow the description).

Figure 1 now includes markers showing the location of discussed topographical features, and labels naming specific bodies of water.

- In the text, there are also some comments on tide changes south of Australia. Some figures to support this text would be useful, in appendix for instance.

Figure 5, showing the south coast of Australia, has been included in the manuscript.

"ON-LINE" REMARKS - P1-Line 14: sentence "At sea level . . .." is a bit strange. Why insisting on well-suited farming?

The wording has been altered to a more general description of the suitability of coasts for human settlement, rather than a specific example. Reference to "At sea level" has been changed to "Coastal areas"

- P1-Line 16: provide a number together with the 85% would be more meaningful

A population number has been included alongside the population percentage: "85% of the population of Australia (approximately 19.9 million people; ABS, 2016)"

P2-Line 12: Pickering et al., 2012 -> Pickering et al., 2017

Modified as suggested by the reviewer

- P5-Line 7: why focusing on M2, S2, K1? Some explanations should be provided.

Perhaps they are the dominant tidal components but it should be said (relying on reference or map?).

Our new simulations focus on only M2 and K1 which are the dominant semi-diurnal and diurnal constituents in the domain which is now referenced to in Section 2.1. We have included Figures showing the control M2 and K1 amplitudes.

- P5-Line 25: as the authors made the computation under non-uniform SLR, this would be useful/interesting to add in appendix the tide changes induced considering the non-uniform SLR.

A figure showing the difference in result between uniform and non-uniform SLR has been included in the supplement, discussing our investigation into using spatially non-uniform SLR patterns

- P7-Line 3-5: "These statistics . . .". The authors do not provide enough evidence that this is the spatial resolution that could explain the discrepancies. More detailed analysis is required to support this hypothesis.

New analysis shows good agreement with all stations apart from Williamstown and Geelong, which are unique in that they are confined within Port Phillip Bay. This narrow headlands which confine this bay are too small a feature to be captured by the model resolution. How this area is treated in the model is now discussed.

- P8-Line 2-3: remind that this was for a given range of SLR in "Idier et al. (2017)", and also for a given area (NW European shelf).

Modified as suggested by the reviewer.

- P14-Line 14: "SLR has a broadly linear effect on the amplitude of the semi-diurnal constituents out on the open shelf, but causes increasingly large semi-diurnal amplitudes, and correspondingly high tidal dissipations, within embayments such as King Sound". I did not see "the linear" effect on the figures. Looking at Figure 4, 8 and 9, notable differences can be observed offshore between the two SLR scenarios. This point

deserves more explanation, and probably some kind of maps showing proportionality coefficients of tide changes with SLR, as for instance in (Pickering et al., 2017) or in (Idier et al., 2017).

With the new simulations we have performed this point of ours is no longer valid and has been removed from the text.

- P15-Line 16: why was it computationally necessary to cross the shelf? Are the authors referring here to computational time? If yes, then it should be stated more clearly and computation time should be provided. In addition, one simulation on a larger domain for a very large SLR would allow estimating the effect of the assumption that tidal components are unchanged on this north boundary.

This point is has been addressed by the new simulations and the formulations in question have been removed from the text.

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