Ocean Science
Discussions

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

Anonymous Referee #1

Received and published: 15 October 2018

SUITABILITY

The subject of the paper, i.e. the study of the impact of sea-level rise on tidal characteristics around Australasia falls within the fields covered by Ocean Sciences.

SUMMARY

The manuscript setup an established tidal model on the Australia area, focussing on the SLR effect on the Australasia area. A validation of the model is presented for present-day conditions. The SLR is implemented through a water depth increase. The study focusses on M2, S2 and K1 components changes, allowing the land to be flooded or not, while the investigated SLR values range from 1 to 20 m, and the paper focuses on the 1 and 7 m results. The model results for SLR=1 m are compared with tide gauge

processed data. Then, the effect of SLR on the tidal components around Australasia is described. Some elements of physical mechanisms are provided.

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.

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.

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?

4.Figures

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

- 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 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).

- 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.

"ON-LINE" REMARKS

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

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

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

- 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?).

- 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.

- 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.

- 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).

- 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, no-

table 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).

- 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.