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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The Effects of Changing Climate on Estuarine Water Levels: A United States Pacific Northwest Case Study" *by* Kai Parker et al.

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This is a highly interesting study, using coupled, high-resolution and long-term modeling for assessing the flood hazard to 2 estuaries. The study is well motivated as it argues that the nonlinear interactions between the different drivers of flooding are not well understood in estuaries (in general) and at the US Pacific NW coast (in particular). I feel that the main novelty is the process-based modeling framework for analyzing climate change impacts to the different forcings of estuarine flooding. Some interesting conclusions are drawn, which challenge widespread assumptions, such as the bathtub approximation and adding an uncoupled high tide and high non-tidal residual to obtain

(compound) flooding magnitudes.

MAJOR COMMENTS:

- Section 3.4: Monthly Mean Sea-Level Anomalies (MMSLA) are modeled via a regression approach. Although I do not critize the regression approach, I wonder whether the variability of the modeled MMSLA agrees with the observed one. Regression provides smoother responses, but the variability might be important when one looks at extreme water levels. Hence, is this regression step a source for underestimation of extremes? This should be clarified.

- Effective RIs and assumptions about nonstationarity: I would like to see a more accessible presentation of the concept of the study in relation to nonstationarity. I have not understood the concept. For example: * Section 4: Here it is explained that it is possible to separate the calculation of RIs and the nonstationarity of the time series. I do not fully understand what this means: Do you assume that the nonstationary is only a consequence of SLR? But other changes related to climate, such a changes in the wave climate, could also introduce changes in extreme water levels, right? I am confused and would like to see a clearer explanation. * Page 8, Line 33: I do not understand what it means that one can "... add the amount of nonstationarity (for this study, SLR) corresponding to the year of interest...". I guess this remark is related to the privious one. * Section 5.1, last paragraph and Figure 6: Again I do not understand the explanation of effective RIs and the locations of intersection between historic and future effective RIs. Why are the historic water levels higher than the future water levels when we have SLR?

- Future period: I am (partially) confused about the definition of 'future'. Please be clear about the future period (but also about the historic period) in the abstract, text and figures. For example, it would be good to give the 2 periods (historic: 1979-1999; future: 2041-2070) already in the abstract. Further, Fig. 9 says in the legend that the Flood Zone in 2100 is shown (although simulations have not been performed for this

period!) and the effective RI WL for the year 2050.

- One of the main conclusions is that extreme water levels are generally compound events, i.e. (in my understanding) the joint occurrence of different drivers, where the coupling between processes should be taken into account. On the other hand, the comparison with the FEMA flood zones seems to demonstrate that a simpler approach (I assume that the FEMA flood zones are not based on such a sophisticated model setup) leads to very similar results. Doesn't this invalidate your conclusion about the importance of compound events and the necessity to use coupled models?

- One of the limitations, acknowledged by the authors, is that the uncertainty is not included. They argue that one cannot use ensembles for this kind of complex model setup. I can follow their argument, but I would like to see a frank discussion about this tradeoff: Given limited resources, should I go for simpler models & ensembles or complex models without uncertainty quantification? I understand that there is no general answer to this question, but maybe the authors can discuss the different arguments and "recommend" what one could or should do (maybe considering different purposes, e.g. planning of flood defense).

MINOR COMMENTS:

- Page 2, Line 1 and Page 12, Line 19: Please use only literature which has been published.

- Page 4, Line 25: I am surprised to learn that the RCMs have a super high resolution: "... Spatial resolution for models within NARCCAP is 50 m for RCM variables...". I just want to make sure that this is not a typo.

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