

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

Bruno Merz (Referee #2)

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Recommendation: Major revision

Summary:

“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.”

Response: We thank the reviewer for the assessment of the article. We appreciate the clearly significant effort that the reviewer has spent on the article and hope that, with the modifications detailed below, the article will be acceptable for publication.

Major Comments:

- 1) *“Section 3.4: Monthly Mean Sea-Level Anomalies (MMSLA) are modeled via a regression approach. Although I do not criticize 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.”*

Response: We agree with the reviewer in that the utilized regression approach adds an additional layer of uncertainty into estimates of WLS. This is especially true concerning extremes as the regression is unable to reproduce maximums in MMSLA's. This said, a computational approach (for example a 3-D regional scale coastal model like ROMS) would be extremely computationally expensive. This approach would likely still be imperfect when forced by coarse AOGCM outputs. The taken statistical approach allows a compromise of modelling the leading order effect of MMSLA's on WLS. While admittedly imperfect, this was required computationally and a methodological decision that is well represent in the literature.

While there is uncertainty in MMSLA's, most of the key conclusions from this paper are not affected by this issue. We are mainly interested in changes from historic to future conditions and the bias induced by using regression for MMSLA's (likely a bias low as pointed out by the reviewer) should be similar for both periods. In other words, our bulk estimates of RIs might be biased but the difference from climate change should remain valid. A similar argument could be made about spatial variability in return intervals as MMSLA's effect RI's in an approximately spatially uniform manner (see figure 10).

We agree with the reviewer that the current version of the article does not properly discuss this important point and we have added text discussing the problem in section 3.4, 5.3, and 6.1.

- 2) *“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.*

Response: The reviewer is correct that, in the approach taken by this paper for nonstationary RIs, there is an implicit assumption that removing SLR will make the timeseries “approximately” stationary. This is definitely an assumption and is discussed in detail in section 6.4.3. We agree with the reviewer that this is not clear in the original version of the manuscript and have added additional text explicitly detailing this assumption in section 4. We have additionally added a statement referring the reader to section 6.4.3 for additional discussion.

** 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 previous one.*

Response: We agree with the reviewer that this section is difficult to understand without a visual. In an earlier version of the paper, we had placed the effective return interval curve results here to help with explaining what is meant by this text. During interval review, this was flagged as inappropriate since we were displaying results outside of the “results” section. We also thought about inserting a generic figure here for explanatory purposes, but this was also decided against as the article is already long in terms of figures and word count.

We have significantly revised this section with hopes of increasing overall clarity. We have removed the section quoted by the reviewer and attempted to replace it with a more understandable version. That said, if the reviewer thinks that a figure would still help, we could either move a results figure here or add a generic figure.

** 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?**

Response: We agree with the reviewer that this presentation of results is somewhat non-intuitive. In considering this figure we were worried that, as detailed in the comment, the immediate take away would be that future flooding is less than historic. This is definitely not true. The future RI curve represents recurrence water level events without SLR included. We decided to present the data in this way for two reasons. The first is because we feel that it shows a valuable outcome of the study, specifically (“future RIs as a function of only changing forcing (no SLR)”). The second is that to plot nonstationary Future recurrence intervals (SLR included) on the same plot as stationary historic recurrence intervals is conceptually problematic. Stationary and nonstationary recurrence intervals are not directly equivalent so we feel this would be incorrect. Instead we plot the stationary version of the Future recurrence intervals (that with SLR not included) to emphasize this point.

To clarify this further, we have added (No SLR) to the Future symbol in the legend of Figure 6. We have additionally augmented the text in the description of the figure to further emphasize this point (as well as our reasons for presenting the data in this way). This said, if the reviewer has any further suggestions for how we could improve clarity, we would very much like to incorporate it in the paper. This is an important detail that we don’t want to get lost.

3) *“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.”*

Response: The reviewer here brings up a point of confusion that, in retrospect, we have not explained sufficiently in the paper. Simulations are for the 2 periods mentioned (historic: 1979-1999; future: 2041-2070). From these it is possible to calculate return intervals events with an associated recurrence interval (in this study ranging from 2 – 100 year). Unfortunately, in terms of clarity, our use of effective return intervals adds an additional dimension of time to the concept of return intervals. We have significantly re-worked the section describing effective return intervals with hopes of improving clarity. We have taken the reviewers suggestion to add the 2 periods of simulation to the abstract. We have additionally changed the legend in Figure 9 to say 100-yr Flood Zone (Hist.) and 100-yr Flood zone (2100). We have also added text hopefully clarifying this point.

As to the reviewer’s comment regarding the plotting of the flood zone in 2100, this is a calculable quantity even though we don’t have simulations for this period. A similar argument could be made for the common extreme value analysis (EVA) procedure of calculating a 100-year event without 100 years of simulations. Just as in for that case (EVA), we are using a statistical model. This allows us to make a prediction for the 100 year return interval flood zone in 2100, of course under the constraints of that model (which is just a model and a

simplification). The key assumption is (as pointed out in the reviewer's other comments) SLR is the source of nonstationarity. We have tried to emphasize this more in the revised text of the article as well as included an in depth discussion of this assumption (section 6.4.3).

- 4) *“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?”*

Response: Similarly to the reviewer, we were surprised to find out that the calculated 100-year flood zone for this study and FEMAs were so similar. Unfortunately, the report detailing FEMA's approach is quite opaque, so it is difficult to determine the exact differences between the two methodologies. The FEMA assessment for Coos Bay was performed fairly recently (2014) so does appear to include some process coupling and more modern approaches for estuary systems. This said we cannot conclusively say to what extent.

As an additional point of consideration, this figure only allows for a planform comparison in flood areas. Differences in flood areas are strongly controlled by gradients in terrain. For example, a 1 meter difference in flood elevation can result in a significant difference in flood area for flat regions. Coos Bay, on the other hand, is hemmed in by mountains and so large differences in flood elevations may not result in a significant change in flood area (especially at the scale of the map in the discussed figure).

We decided to include this figure in the paper as we found it to be a valuable validation tool. We felt that validation was a weakness of the study and that this figure provided some confidence moving forward. While we agree with the reviewer's discussion, we still find the figure to be valuable in this regard.

- 5) *“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).”*

Response: This is a very pertinent comment by the reviewer and something that, through the process of this study, we spent a lot of time thinking about. We did not include this discussion in the original manuscript as we were unsure if it was too divergent from the purpose of this paper. This said, we have happily included a brief discussion section in the revised manuscript (section

6.4.2).

Minor Comments:

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

Response: This reference has been removed in the revised manuscript.

- 2) *“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.”*

Response: As the reviewer correctly assumed, this was a typo. The RCM resolution in NARCCAP is 50 km. This has been corrected in the revised manuscript.