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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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Interactive comment on "The Effects of Changing Climate on Estuarine Water Levels: A United States Pacific Northwest Case Study" by Kai Parker et al. Response to Anonymous Referee #1 Received and published: 5 February 2019 Recommendation: Major revision Summary:

"The authors have proposed a modeling framework to quantify the extreme estuarine water levels (WL) under climate change. Their framework, taking oceanic, atmospheric and hydrologic processes into account estimates extreme water levels in two estuaries of US Pacific Northwest. The idea of integrated modeling that couples processes

across scales to estimate extreme estuarine water levels is interesting, and I believe this idea deserves publishing in NHESS, however after a major revision. There are a few issues with this version that I explain below: "

Response: We appreciate the reviewer's assessment of the paper and especially their detailed suggestions as to how to improve the manuscript. We have carefully incorporated all the reviewers comments and our responses can be seen in the following text. Our comments have additionally been attached as a pdf for better readability. We hope that, with the reviewer's comments integrated into the new manuscript, the study will be ready for publication.

Major Comments: 1) "First one, which the authors themselves have briefly mentioned, the climate and sea level rise projections used here are relatively old-dated! Publishing research based on 4th IPCC assessment report, while IPCC 5th has been around for years and IPCC 6th is coming out soon, needs a valid justification. To me, saying "was the only climate product, at the start of this project" is not enough. I expect a good justification that the current results are still useful for the audience. "

Response: We agree with the reviewer that using a more recent climate scenario could increase the strength of manuscript. Unfortunately, this project has required significant time and was started before/early in the IPCC 5th assessments availability. The choice of NARCAAP, and therefore the IPCC 4th assessment's climate scenarios, was based on data availability at the time of the studies start. We note in the manuscript:

"There are other downscaled climate products available (e.g., MACA; Abatzoglou [2013]) that are based on more current IPCC 5th assessment scenarios; however, NARCCAP was the only climate product, at the start of this project, that provided the necessary offshore coverage with the higher resolution RCM. Most other products are masked so that data are only available on land surfaces while this project required information across the ocean as well."

Additionally, the climate scenario used in the paper is consistent with current climate trends and is well within the variability of IPCC 5th assessment scenarios. Therefore, we would argue that the conclusions from the study are still valid and a useful contribution to the scientific community. To clarify this with the reader we have included the following paragraph in the revised manuscript,

“While the usage of NARCCAP data forces this project’s reliance an older climate scenario, this does not mean that results are out of alignment with current climate projections. Rather, the A2 SRES scenario is well within the variability of the new scenarios framework of the IPCC 5th assessment. A direct comparison of 4th and 5th IPCC assessment climate scenarios is impossible due to a conceptual change in how scenarios are handled [Nakicenovic et al., 2014; O’Neil et al., 2013]. However, work by Van Vuuren and Carter [2013] has shown that the A2 SRES scenario approximately maps to the representative concentration pathway (RCP) 8,5 and shared socio-economic pathway (SSP) 3 scenario. Since the publication of the IPCC 4th assessment, baseline emissions have been within the range presented within the SRES scenarios [IPCC, 2007] with emissions tracking closer to the higher range of scenarios [The Copenhagen Diagnosis, 2009]. This supports the usage of the A2 scenario for near term projections.”

Utilized SLR projections were based on a study expanding on IPCC 4th assessment projections [NRC,2012]. While it would have been possible to utilize IPCC 5th assessment SLR projections, the NRC report provides local SLR estimates for the Pacific Coast. Vertical land motion in particular is very important at the study sites and it was decided that using slightly older local projections would be of a higher accuracy than a newer global projection. Additionally, using IPCC 4th assessment SLR projections provided consistency rather than mixing and matching forcing across assessments.

2) “At the end of section 4 (Lines 1-9 in Page 9), the non-linear interactions between SLR and tide/surge (i.e. Wahl, 2017, Sea-level rise and storm surges, relationship status: complicated!, Environ. Res. Lett., https://doi.org/10.1088/1748-9326/aa8eba ; Devlin et al., 2017, Coupling of sea level and tidal range changes, with implications

for future water levels, Scientific Reports, vol 7, 17021) should be highlighted that are missed here.

Response: We thank the reviewer for pointing out that this section is somewhat misleading in saying that non-stationarity is removed through the use of a variable datum. Rather this is an assumption that is discussed further in section 6.4.3. We have included an additional explanation of nonstationarity as a function of non-linear interactions within section 6.4.3. as well as acknowledged this assumption in the section discussed by the reviewer.

3) “Also, other alternatives for non-stationary frequency analysis should be explained as well for interested readers (i.e. Cheng L., AghaKouchak A., Gilleland E., Katz R.W., 2014, Non-stationary Extreme Value Analysis in a Changing Climate, Climatic Change, 127(2), 353-369, doi: 10.1007/s10584-014-1254-5.).

Response: We thank the reviewer for pointing out that we have not directly discussed a non-stationary approach. We have modified the paragraph starting at Page 8, L 37 to read as follows:

“Nonstationary extreme value analysis has recently seen a wide range of applications to coastal problems [Corbella and Stretch, 2012; Katz, 2013; Wahl and Chambers, 2016; Wahl et al., 2015]. Nonstationarity is generally incorporated within the GEV (Generalized Extreme Value) model by using time dependent parameters either as a linear or exponential function [Cheng et al., 2014; Ruggiero et al., 2010], a cyclical trigonometric function [Mendez et al., 2008; Minguez et al., 2010] or as a more complicated function of covariates [Weisse et al., 2014]. This study chooses an alternative to a nonstationary GEV approach primarily due to the format of the data from this study. WL data are output from the model in reference to MSL. This results in a WL time series that does not show any discontinuity or trend from changing sea level, a signal that would only be visible if viewing WLs relative to a non-tidal datum. This approximate stationarity, as a function of datum, makes it possible to separate the calculation of RIs and the nonstationarity of the time series. This avoids the complications of fitting a non-stationary GEV (Generalized Extreme Value distribution) and the corresponding loss of degrees of freedom from estimating the nonstationary trend from the data. Furthermore, most nonstationary GEV analyses are forced to use a priori simplistic functions due to limited degrees of freedom. This approach allows a more complicated trend that follows experienced SLR (approximately cubic for this study). This approach of treating the resulting MSL timeseries as stationary is an assumption/simplification and is discussed further in the section 6.4.3."

4) "Page 4, L9-10: the hindcast and forecast periods are not the same length. This makes them incomparable, right¿'

Response: We acknowledge that ideally the hindcast and forecast periods would be the same length for statistical comparability between the two segments. Unfortunately, this was impossible due to data availability and the requirements for the various modeling components. We would argue that the two periods are still comparable, although with different uncertainty in extreme value estimates for the two periods. This is well exhibited in Figure 5 through the difference in confidence interval width between the historic and future period RI curves. We agree with the reviewer that this mismatch in confidence between estimates is unfortunate, but it was a compromise that we had to make in incorporating so many modeling components with differing data needs.

Minor Comments:

1) "Page 1, L 31-32: There are some studies to be cited here, i.e. Jay et al., 2016, Tidal-Fluvial and Estuarine Processes in the Lower Columbia River: II. Water Level Models, Floodplain Wetland Inundation, and System Zones, Estuaries and Coasts, Volume 39, Issue 5, pp 1299–1324; and the references therein."

Response: We thank the reviewer for bringing this article to our attention. We have added the reference to the revised manuscript.

2) "Page 2, L18: a relevant recent citation Gallien, et al., 2018, Coastal Flood Modeling Challenges in Defended Urban Backshores, Geosciences 2018, 8(12), 450; https://doi.org/10.3390/geosciences8120450."

Response: We thank the reviewer for pointing us to this excellent article and we have added the reference to the revised manuscript.

3) Page 6, L4: Please provide more details about the quantile mapping technique used here.

Response: While we agree with the reviewer that a clear identification of the utilized bias correction methods is important for transparency in the study's methodology, we think that a full description is beyond the scope of the paper. As an alternative we note that the utilized procedures are well described in a previous paper, Parker and Hill, 2017. We have added the following to the revised manuscript to direct interested readers to this resource:

"A full description of the utilized bias correction procedure, both the bivariate method utilized for wave modeling and the univariate method used for other variables, is beyond the scope of this paper. Instead, the reader is directed to Parker and Hill., [2017] for a more detailed description."

We hope that the reviewer finds this as an acceptable alternative to a significant lengthening of the article text length to descript the bias correction methodology.

We once again would like to the thank the reviewer for their time and effort. We think that the resulting paper is significantly stronger than the original due to the careful input of the reviewer. Thank you.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-383/nhess-2018-383-AC1-supplement.pdf

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