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Natural Hazards and Earth System Sciences
Discussions
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Interactive comment on "What's streamflow got to do with it? A probabilistic simulation of the competing oceanographic and fluvial processes driving extreme along-river water levels" *by* Katherine A. Serafin et al.

Anonymous Referee #1

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General comments Serafin et al present a new framework for examining the joint influence of several coastal and riverine processes on water levels in estuarine environments, and show very clearly that the 100-yr ocean or 100-yr streamflow event does not always produce the 100-yr along-river water level. It is a novel piece of work using a clever methodological framework, resulting in an analysis that can assesses non-stationary water levels from a multivariate joint distribution and truly decompose coastal water levels. As such, I believe that the research forms an important contribution to the increasingly important field of compound flood risk assessment. The

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manuscript is well written in terms of language, but parts of it feel to long or could be helped by restructuring. There are also some specific methodological issues that require further explanation, as described in the following review. Nevertheless, if these can be sufficiently responded to, I believe that this paper would provide a very valuable addition to the literature.

Main comments

The introduction is generally well written and reviews most of the relevant literature. However, some important concepts for the paper are not fully introduced or defined. For example, a formal definition of compound flooding is missing. On page 2, line 20 (and also later at page 25, line 15) the authors imply that probabilistic simulations of water levels have not yet been done considering ocean and onland processes, and that this has only been done for specific events. However, Bevacqua et al (2017) van den Hurk et al. (2015), and Couasnon et al. (2018) have used probabilistic simulations. The current paper certainly adds value to the research carried out in those studies, but it would be prudent to mention them and how the current study advances.

In terms of the overall structure, the methods section (section 3, but also parts of section 4) are sometimes difficult to follow. The really interesting part here is the new overall framework. However, this overall framework sometimes gets lost in the details of the various specific models used, which can be rather lengthy (e.g. the part on HEC-RAS). It would be beneficial to the reader to highlight the overall methodological framework more clearly at the start of the methods section, for example with a flowchart. This would highlight more clearly the major novelty of this paper. It is of course also necessary to give details of the various components used for each part of the framework, such as HEC-RAS. But by emphasizing more the framework, it would be clear that one could also use the overall assessment framework with other hydrodynamic models, if one wished to do so.

Following on, it may help to move some of the details to Supplementary Information.

General background information about setting up HEC-RAS can be shortened, and the essential parts for this study could be moved to supplementary information. This would improve overall readability of this section.

Related to the previous comments on structure, the part on HEC-RAS model validation (3.2.1) seems out of place in the methods section. It could be moved to the section on validation or in my opinion better to still to supplementary information.

My main methodological concern relates to the use of steady flow simulations. As the authors state themselves in the discussion, the steep catchment of the mountainous environment means a short response time for rainfall. It also calls into question the validity of using steady-state flow for the analysis. I would like the authors to explain this choice and explain what it means for the overall results? Has there been any sensitivity assessment of the results compared to an unsteady state simulation, for example?

It is not clear what Manning's coefficients are used on the flooplains. It is stated that they are estimated using 2011 Land Cover data from the Western Washington Land Cover Change Analysis project (NOAA, 2012) and visual inspection of aerial imagery. But what values were selected for different land use classes? Moreover, on page 8, line 20 the Manning coefficient of "0.005" is very low and not really representative of natural river states. Is there a specific reason for this?

How are the high water level events constructed? The possible presence of autocorrelation in the data is not mentioned – it would be good to test for this or report the results of such a test if it has been done already.

Other suggestions

Figure 13: the grey dashed lines presumably belong to the 4 different return periods shown – it would be easier for the reader to use the same colours (but dashed) instead of grey.

Caption of figure 13: “the pink shaded area represents a transition zone, where neither event drives the water level”. The last part is not clearly phrased. Do you mean the zone where the water level is not driven by either the coastal or river drivers alone?

Page 26, lines 14-15: “At low tide, a high river discharge may promote drainage of the floodwater into the ocean (Kumbier et al., 2018), increasing water levels for days at a time and prolonging exposure to flooding”. Why would a low tide that promotes drainage to the ocean lead to increased water levels? Would the opposite not lead to backwater effects?

In the abstract it is stated that “Understanding the relative forcing of extreme water levels along an ocean-to-river gradient will better prepare communities within inlets and estuaries for the compounding impacts of various environmental forcing”. A similar statement can be found in the conclusions. I feel that this requires more nuance. There are many steps that would be needed to make these (important) scientific insights usable by a local community for preparing themselves.

Page 17-line 14-15: “ADCIRC simulations confirm this phenomenon, as the river discharge peak is modeled exactly at low tide (Figure 5)”. I find it hard to see that when looking at Figure 5. Maybe help the reader a bit more? For me it seems more to be at high tide but maybe there is something I am missing.

Textual changes

Page 3, line 30. Change “...experiencing relative sea level rates of...” to “...experiencing relative sea level change rates of...” (similar comment in line 31).

Page 8, lines 10-11: add “in most cases”.

Page 8, line 30 (and the rest of the text): where is Toke Point tide gauge on Figure 1?

Page 11, line 12. Change “periosd” to “period”

Page 14, line 13. Change “subsituting" to “substituting”

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Page 23, line 19: suggest to remove “regardless of the likelihood” (it is already in the return level events?)

Page 23-line 5 and 8: add “a” and “b” to Figure 13 to help the reader.

References not mentioned in manuscript

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Couasnon A, Sebastian A and Morales-Nápoles O 2018 A Copula-based bayesian network for modeling compound flood hazard from riverine and coastal interactions at the catchment scale: An application to the houston ship channel, Texas. Water, 10, 9, 1190 âĂć Van den Hurk B, van Meijgaard E, de Valk P, van Heeringen J and Gooijer J 2015 Analysis of a compounding surge and precipitaiton event in the Netherlands Environ. Res. Lett. 10, 035001

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