Review of the paper: "Estimating the Probability of Compound Floods in Estuarine Regions Wu" by Wu et al.

The authors provide a comparative review of three alternative approaches for assessing the local compound flooding probability. They implement the approaches for studying compound flooding in the estuary of the Swan River in Western Australia. Such an application provides a basis for discussing the advantages and limitations of the three approaches.

Overall, I did find the study very interesting and timely. I recommend to revise the manuscript based on my comments prior to publishing the manuscript.

In general, I found the introduction pleasant to read, but I would recommend improving the presentation of the methodology, especially of Method 3. In fact, I found it particularly difficult to understand some components of Method 3.

The discussion of the advantages and limitations include a part regarding the limitations/advantages that the approaches have for assessing the climate change effect on compound flooding probability. The idea of discussing this topic is certainly interesting, but it requires some revision in my view. For example, the authors mainly refer to the possibility of including changes in the dependence between the drivers through method 3, but it is not discussed the relevance of the change in the marginal distributions, which is fundamental. In particular, I understand that the authors state that method 2 is difficult to be considered for assessing climate change as it requires time series of storm tide and precipitation for the future. This would be an issue also for method 3, despite the fact that they claim that method 3 can consider climate change effects easily. See comments related on this topic below.

Specific comments

L47, I would cite the paper from Wahl at the end of the sentence (alreay metnioned innt he manuscript).

L64, please, consider merging this sentence with the last sentence of the previous paragraph (on the same topic).

L76 I suggest: that produce an inverse barometric effect and on-shore winds, which in turn leads to storm surges and waves

L78 water -> oceanic water level (to make clear you are referring to the sea component only in this sentence).

116, typology

128 "and considered here" after identified

L 129-130, Consider using "compound flood" here and elsewhere when referring to the compound flooding water level, such to make clear that you are referring to the resulting water level from the two drivers. For example, in the caption of Fig 2. I can certainly say that this would have made my reading easier.

L 146 “numerical” is fine here? In method 2 you may not need a numeric model (i.e., hydrodynamical model) rather use an e.g., statistical model (personally, I do not see that as numerical). I see that in your case you use numerical modelling, but this part of the manuscript appear of a more general nature.

L 166 Similarly to the above, doesn't dynamically refer to something that is not statistical? Anyway, I would modify, to make clear that such modelling can also be purely statistical.

L 159 Consider adding Approach 1,2,3, also earlier on, to give a better orientation to the reader.

L 174, do you mean 30 years of data to estimate the 1-in-100 years return level? Anyway, you may want to qualify "estimate", anything can be estimated, but would that estimate be too uncertain or not?

L 185, During a discussion among colleagues, it was hypothesised that this may be related to the fact that often there is interest in measuring either the sea level or the river discharge and therefore no stations are collocated at the interface between the two. What do you think about this? Discuss it if you think that this is relevant. I guess that this appears also discussed/hypothesised in Paprotny et al. (“Compoun flood potential in Europe”).

L 188, plaase, make it clear that you are referring to the need of transforming flow into the water level

L 205, Do you have a reference? Not sure if this was given earlier.

L 227, “Although…”. Consider moving this to the beginning of the next paragraph

In general, regarding sections 2.2-2.4, 230-241, I believe that the reader would benefit from finding dome additional references to works where similar approaches have been used. In my view, this can help, especially in a work like that aims at reviewing available methods.

L230-241, I find this part a bit too strong in the statements. Hydrodynamical modelling works based on oceanic and streamflow input that is available from climate models. I see that there are uncertainties and that storm tide and river flow need to be obtained based on computationally expensive modelling. But some data are available out there that can be helpful to assess the climate change impact on compound flooding even with Approach 2. I would suggest discussing this topic more.

L252, Isn’t it what you produce the value of the structural variable rather than the variable itself? You use a “function” to convert a given bivariate event into a water level.

L 262, “condition.” Please, provide a reference, where this method is described

L 266, how to select the design events? Multiple pairs in the bivariate space can have the same probability to occur, i.e. return period. Therefore the selection is not as easy as in the univariate case. This is discussed in the paper of Moftakhari et al. (2019). A brief discussion (2/3 sentences) on these issues is welcome.

L 268, please, clarify this sentence.

L 298 “Due to...complexity”, Or is it that none has rally tried to develop one?

L368, Authors tend to oppose GPD and GEV as alternative approaches. Do you expect any differences in terms of uncertainties? Also, you use the GPD to estimate return periods/level. Shouldn't you also provide an equation for that?

L 390, Please, refer explicitly to the fact that extreme H may also be driven by non-extreme conditions of either of the drivers, therefore this should be taken into account when defining the threshold for Q and T.

L 390, It is not clear to me why you need to account for the low water level periods through the resampling approach, given that you will fit the GPD only to the extremes. I understand that is necessary to be aware of the time in between the peaks to estimate the return periods, but why simulating it?

L412, Are you simulating also a fraction only of the low water level and then using such a short simulation to fill a longer part of the time series? Please, explain better.

L 426, This is shorter than then 31years, which correspond to about 271560hours. I would highlight this explicitly as it is relevant as you implicitly suggest.

L 436, “conditions”, refer to variables to guide the reader (storm tide and river discharge)

L437, Introduce the “grid” or make it clear what the grid is here in this context.

L 442, “250 years”, what return period? The univariate of the individual drivers? This is unclear. Also, you may need to clarify what type of data are you using for the boundary conditions. You seem to have 22 years of data of storm tide only, how did you estimate the 250 year return period without massive uncertainties?

L 445, The dependence between the drivers within the 28 events? Please, clarify.

L458, I see that you use MIKE21 for Methods 1 and MIKE FLOOD for method 3. Can this be responsible for the differences in the results based on the two methods? Please, discuss.

L 459, step 1 was not introduced formally.

L460, this small paragraph is not clear to me. Please, explain better for people who are not familiar with the method.

472, In the methods, please mention how you retrieved the uncertainties in the estimate (based on the uncertainties in the fitted parameters).

488, I suggest to highlight that this location was used in Method 1 (so to allow for a comparison).

L 498, Aren't you also for Method 2 using the MRL plot applied to the H water level? Please specify if not done already and include a discussion on this within this sentence (one may expect that MRL to define a thresholds such result in similar uncertainties at all locations.

L 509, Why do you use the values maximising the dependence? Understanding when the dependence is maximised provides interesting information on the physical system, however, the dependence values that are relevant from a point of view of the impact is that between the variables at the same time. In fact, the storm tide and the river flow interact at the same time in the real world.

Fig 8, The 2D simulations receive as input time series of T and Q, therefore a question arises: which is the value of the time series that you consider as that to be reported on the x and y axes?

The plots, e.g., panel c, suggests that for a given 10year return level of Q, when T becomes larger (from 0 to 1-year return period), H decreases. This is physically inconsistent. Such inconsistent behaviours seem to occur in the range of T AND Q below 1-year return levels. Do you have an explanation for that? If the explanation is convincing, one would then consider not showing values in this bivariate range (up to 1-year return level for both variables).

L 524, How do you estimate the case of complete dependence/independent variables? I understand that you get the water level based on 2d simulation with input T and Q observed time series. If they were not time series, I could see the concept of independence, but in this context, I find it unclear. This comment is related to that on the general explanation of method 3.

L 550 I would reverse the sentence, highlighting the result based on method 2 and 3

compared to 1, the observation-based method. Hence, “2 and 3 lead to lower estimates than 1…”

L 565 can the comparison be affected also by the difference model type used in method 2 and 3?

Table 2,
Disadvantages for method 2, “Difficult to asses future conditions…”: Why isn't this the case also for case 3?
The advantage for method 3 about future conditions: This does not seem to me as simple as stated. Please discuss. By the way, also changes in the marginals should be included, which appears to be the most relevant for future changes and at least the change for which we have the highest confidence (the confidence on the changes in the dependence is small). I would suggest dicussing this taking into account the following papers (at least):

- About changes in the dependence: Wahl et al. (Nature Climate Change)highlights a change in the dependence in the past, Bevacque et al (https://eartharxiv.org/repository/view/293/) highlights tha changes in the dependence are uncertain, and Ganguli and Merz (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL084220) also discuss changes in the dependence for the past.

- Moftakhari et al.,(PNAS) and Bevacque et al (above) about projected changes in the marginals (i.e. Storm surge, precipitation, and sea level rise).

L 590 “stationarity”, add “in the estuarine characteristics”. You are referring not to the meteorological conditions here, so make it clear, please.

L598 Could you clarify/discuss why should accounting for the dependence explicitly be an advantage (compared to method 1)? Thanks.

602, Personally, I would add something along this line. “incorporated in the modelling framework”, add: “through considering the most recent bathymetry characteristic of the estuary when interested in the present-day estimate of the flooding probability”.

L 609, There is high-resolution data of sea level (storm surge/waves) and precipitation available, though I understand that especially for sea level, these are rare and in general can be uncertain. There are climate models. How can they be used to solve the issue? The fact that data is not widely available at high resolution does not mean, I think, that this is something to negatively judge this method given that I am not sure about what would a better alternative be.

612, “updating”, Are you referring to update with respect to changes due to climate change?

If not, please discuss the climate change issue, as this is done in the other two cases.

If yes, please clarify. In addition, I do not understand how you would estimate the changes in the dependence. Do not we have the same issue as in method 2? Also, we have the problem that we need to estimate changes in the marginals, not only in the dependence. See comments above regarding this topic too.

649, See comment above about climate change. This needs to be discussed carefully.

L 655, “Implementation of each approach available” -> “approaches available”

Best regards.