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Discussions

Interactive comment on "Estimating the Probability of Compound Floods in Estuarine Regions" *by* Wenyan Wu et al.

Anonymous Referee #2

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The manuscript introduces and discusses the strength and weaknesses of three approaches commonly applied to analyze compound flooding in estuaries. The approaches are demonstrated in the Swan River system in Western Australia where some of the discussed advantage and disadvantages manifest. The manuscript is informative, topical, and generally well written. In my opinion, after implementing the revisions given below (and those given by the other reviewer!!) the manuscript will warrant publication in HESS.

General comments.

-Since half of the manuscript is a review of the methods used in previous studies more references to previous applications of the discussed approaches would be desirable. Please see the specific comments for some examples of where this is the case.

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-In the introduction, the two physical processes causing estuarine flooding are described in detail, however, a discussion regarding the possible mechanisms enhancing estuarine water levels due to the interaction of the two processes is missing.

-Section 2.4 would benefit from a similar brief discussion on the methods of selecting multivariate extremes perhaps a summary of Zheng et al. (2014). Also, the multivariate statistical methods used to estimate the probability of compound flood events e.g. regression type models (Serafin et al. 2019), standard copulas (Muñoz et al. 2020), Vine copula (Bevacqua et al. 2017) and conditional exceedance models (Jane et al. 2020) should so be discussed or at least listed. The selection of design events i.e. the issues with choosing hazard scenarios and the use of meta models to increase the efficiency of the numerical models also warrant a mention.

-The description of the method in Section 4.3 could be improved a lot. For instance, the link between the DVM grid and probability model is not clear to me.

Specific comments

Line 45: Wahl et al. (2015) analyzed the temporal variation in the dependence between precipitation and surge in the USA. Consider adding as a reference at the end of this sentence.

Line 48: This sentence is rather strong given that there are locations with gauges in the 'joint probably zone' and the results of a univariate probability analysis maybe satisfactory. Consider removing "if ever".

Line 90: Please consider referencing one of the many studies that have demonstrated this (see Santiago-Collazo et al. 2019).

Figure 2: This caption is the only place the word 'pathway' mentioned. Since pathway 1 concerns approach 2 and pathway 2 approach 3 consider changing the label numbers to 2 and 3 and mentioning in the caption that approach 1 just uses observational data.

Line 101: Does this not vary with distance along the channel? As stated later in the manuscript: “The region downstream of Sw10 is mainly storm tide dominated; the region upstream Sw16 (near the Perth 320 Airport) is mainly flow dominated; and the region between Sw10 and Sw16 has significant joint impact from both tail water levels at Fremantle and upstream flow, and therefore is referred to as the ‘joint probability zone’.”

Line 206: Should add some examples here.

Line 244: I think the aim is to derive a series of multivariate ‘design events’ rather than ‘translating the boundary conditions into a series of multivariate ‘design events’.

Line 245: “These approaches are the multivariate analogy of applying IFD curves for delineating design rainfall ‘events’ with pre-defined probabilities, which are then converted into streamflow events of an equivalent probability.” It is the streamflow event that corresponds to (or is associated with) the rainfall event with the predetermined probability not the streamflow events of an equivalent probability.

Line 249: Rephrase. I do not believe that “conversion” is the correct term here. The multivariate distribution describes the probability of the continuous boundary conditions.

Line 249: Also, sometimes called a “response function”! Not all the events will result in a flood. Would “flood magnitude” be more accurately termed “water level”?

Line 255: “The use of copulas or equivalent formulations (e.g. unit Fréchet transformations) enables the factorisation of multivariate distributions into a set of marginal distributions that capture the defining features of the variables of interest, together with a joint probability distribution that describes their interaction.” 42 word sentence!! The joint distribution typically includes the marginal distribution and the dependence structure.

Line 268: Second, because the drivers of estuarine flooding are factorised through the multivariate distribution, it becomes easier to incorporate the effects of climate change while preserving key dependencies between variables." This and the advantage discussed in the next sentence requires the assumption that the dependencies between the variables is stationary which should be stated. Also, "separating" maybe an easier term for readers to grasp than "factorizing" here and elsewhere.

Line 272: The downscaling approach in Bevacqua et al. (2017) which related the water level in a 'joint probability zone' to the meteorological forcing's as a way of accounting for climate change may be of interest.

Line 283: This sentence is very long and discusses two related but distinct issues. Please divide into two sentences.

Line 284: Consider adding MacPherson et al. (2019) here as another method of accounting for the temporal shape of surge peaks in stochastic modelling and Environment Agency (2019) for an example where a single shape is derived to represent the largest surge peaks at a site.

Line 287: I suggest adding a reference to a review of the numerical models used to study compound flooding by Santiago-Collazo et al. (2019) here.

Line 296: Typo. Missing an "of" after "range".

Lines 299: Grammar could be improved at the end of the sentence which starts on this line. Figure 3: Caption needs improving e.g. need to state what the colors of the points denote. Also, it is not clear why the Swan-Avon basin is split into two sections.

Line 309: If URS is an acronym it needs to be defined.

Line 321: Also commonly referred as the 'transition zone' which could be added here.

Line 331: Poor grammar. The term "good quality" is not defined, and it should be made clearer the numbers at the end of the sentence refer to water levels. Is the data missing randomly throughout the series or is there a pattern e.g. missing values only occur during storms? This should be explored.

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Line 369: Is the Mrl plot method the approach used to find the GPD threshold in the other approaches listed?

Line 379: “One advantage of using the peak-over-threshold model for Approach 2 is that censoring can be used to improve the efficiency of full continuous simulation using a 2D hydrodynamic model, as only values above certain high thresholds are fully accounted for.” I am a little confused here as the explanation in the introduction implies all of the water levels will be simulated when applying this approach. I appreciate the censoring is a good idea.

Line 387: “By selecting all of the time periods when at least one of the boundary conditions is above the pre-determined threshold, this approach aims to simulate all water levels H above a specified high threshold value.” Moderately high values of both boundary conditions could produce high water levels above a specified highwater level threshold, but these will not be accounted for in the suggested approach.

Line 412: “a random sample of simulation period (e.g. 1,000 hours)” Is this a continuous 1,000 hour period?

Line 441: “In total, 28 flood events with flood drivers”, why 28 events?

Line 459: “Finally, flood levels at the locations of interest (Step 3) are superimposed onto the bivariate dependence model 460 (Step 2) to estimate associated return periods.” Not clear.

Line 524: How are the independence and full dependence return periods calculated? Once added consider rearraigning some text so that Figure 9 is discussed in the same paragraph in which it is introduced.

Line 531: Results reported in this paragraph are similar to those in Moftakhari et al. (2019) and Serafin et al. (2019) and probably elsewhere which could be cited here.

Line 551-554: “This is very likely due to the systematic difference between the observed flood level data (with a maximum value of 1.92 m within the 22 years’ data) and flood levels simulated using the MIKE21 model (with a maximum level of 1.86 m within the 31 years' analysis period) at this location." Interesting, is this due to a shortcoming of the MIKE21 model or the (short) distance between the two locations?

Line 572-574: "This over-estimation of flood levels for a given return period from Method 3 can potentially lead to over-conservative estimation of flood risk and costly flood prevention infrastructure." Or does using method 2 under-estimate flood levels and lead to under design?

Table 2: "Generally more difficult to account for a large number of flood drivers." Please expand on this. There are methods which allow the extension to more variables without restrictive assumptions regarding the nature of the dependence.

Table 2: "Can be used to assess future conditions with dependence structure reflecting future changes". Very general and also could be true for approach 2 if the continuous simulation was run for future projected climatic conditions?

Line 583: Perhaps "established" is more suitable than "well-developed"?

Line 607: "maintain key dependence between the boundary conditions". The dependence may change with time. I would highlight the fact that the method has the potential to account for climate change (unlike approach 1) as a benefit of the approach. The fact that the data is readily available is more of an (important) aside rather than a strength or limitation of the model.

Line 611-613: "By separating the dependence estimation from the flood probability estimating process, future flood probability can be estimated by updating the dependence structure between flood drivers under these conditions without the requirement of additional flood simulation runs." Again, under the assumption that the dependence structure remains stationary.

L616-622: Some mention of this should go at the end of the results section. References:

Environment Agency (2019) Coastal flood boundary conditions for the UK: update 2018 Technical summary report SC060064/TR6, Bristol, UK, pp. 19-113.

Jane, R., Cadavid, L., Obeysekera, J. & Wahl, T. 2020. Multivariate statistical modelling of the drivers of compound flood events in South Florida. Natural Hazards and Earth System Sciences, 20(10), 2681-2699.

MacPherson, L. R., Arns, A., Dangendorf, S., Vafeidis, A. T., & Jensen, J. (2019). A stochastic extreme sea level model for the German Baltic Sea coast. Journal of Geophysical Research: Oceans, 124, 2054– 2071. https://doi.org/10.1029/2018JC014718.

Muñoz, D. F., Moftakhari, H., & Moradkhani, H. (2020). Compound effects of flood drivers and wetland elevation correction on coastal flood hazard assessment. Water Resources Research, 56, e2020WR027544. https://doi.org/10.1029/2020WR027544.

Santiago-Collazo, F. L., Bilskie, M. V., & Hagen, S. C. (2019). A comprehensive review of compound inundation models in low-gradient coastal watersheds. Environmental Modelling & Software, 119, 166-181.

Serafin, K. A., Ruggiero, P., Parker, K., &Hill, D. F. (2019) What's streamflow got to do with it? A probabilistic simulation of the competing oceanographic and fluvial processes driving extreme along-river water levels, Nat. Hazards Earth Syst. Sci., 19, 1415–1431, https://doi.org/10.5194/nhess-19-1415-2019

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