

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2021-268

Anonymous Referee #1

Referee comment on "Assessing the dependence structure between oceanographic, fluvial, and pluvial flooding drivers along the United States coastline" by Ahmed A. Nasr et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-268-RC1>, 2021

This is a very interesting study with three explicit objectives: 1) assessing the dependence between four main flood drivers along the coastline of CONUS (surge, wave, rainfall and river discharge), 2) assessing the seasonal patterns of such dependence, and 3) comparing tail dependence detected from observational records and from the reanalysis data. Not only the idea is very interesting, but also the research design is robust and alternative approaches (i.e. POT vs block-maxima and rank vs tail dependence) have been well-discussed. This study and its outputs are very helpful for coastal compound flood hazard assessment and management, as address a very important challenge that compound flooding community is dealing with it. Appropriate characterization of exceedance probability (which is instrumental to risk assessment) is a difficult task when multiple variables are involved and such studies help the flood risk modelers in at least two ways. First, they will now where there is no significant dependence between flooding variables and make the simplifying assumption of independence between variables. Second, in case a significant dependence is detected what type of numerical modeling scheme can provide the most relevant information about the nonlinear interactions between interacting variables. The manuscript is very well-written and results are nicely presented. Thus, I recommend publication of this manuscript in HESS after a minor revision. My comments are mainly about necessary justification/explanations and some editorial. Please, see the details below.

My main concern here is the time-scale of hydrologic variables. While wave and surge are obtained and analyzed on an hourly basis, accumulated daily precipitation depth is taken as a representative of extreme pluvial flooding driver (L167). I doubt that such characteristic of rainfall can be a good representative of extreme precipitation contributing to compound coastal flooding events. This is a relatively long time window that somehow dampens the density of rain storm and so provides irrelevant information for compound coastal flood modeling. Please, explain if such selection was due to lack of data or methodological reasons? Anyways, I didn't find it explicitly discussed in your manuscript. Also, for the two-way selection scheme, I guess 10 days is too long to justify coincidence/concurrence. It's been justified with regard to the lag-time of river flow, but I doubt in any of the selected systems the transition time of the peak river flow from the

upper tributaries to the outlet would be more than 5-6 days, at most. Do you have any reference on this? I am aware of other studies with comparable window length (i.e. 7 days), but "river flow lag time" might not be necessarily sufficient to justify such long sampling window length. Also, this is not a similar process between all cases. For example, such time window with justification from river discharge lag time has nothing to do with sampling wave and surge that have occurred up to 10 days apart. Moreover, even if it justifies the 10 days lag time in Q-S case, it does not automatically justify pairing an extreme surge with a river flow that has happened up to 10 days later. Such unified 10day time window needs a much stronger justification.

I understand the value of information presented in Figures 5, 7, 9, but I don't see much value in the way that these are currently presented. There are simply too much information on a single chart. I suggest either breaking these down to subpanels (a, b, c, ...) that separately plots the pairs, or removing these charts (as kind of similar information is already presented in the heatmaps). Moreover, I found the dashed lines simply meaningless here. Both rank and tail dependence measures used here assess the nonlinear dependencies and I don't understand what a linear regression means here? Please, explain if I am missing an important meaning behind these linear regression practice.

L142: WAM has not been defined yet.

L224: The equation does not seem right to me. There should be a typo in it.

L305-321: Better to clearly explain earlier that numbers inside parenthesis show the number of sites.

L351: Looks like R here represents the Pearson corr coeff. (As mentioned in L441 and L413). If so please clarify first time appears in the manuscript.

L475-479: The sentence is too long. Please, consider breaking it down.

L556: These citations are not necessary (better to say useful) at the end of conclusions.

Great work!