Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2021-268

Anonymous Referee #2

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-RC2, 2021

Congrats to the authors for putting together an interesting and generally well-written manuscript. The study uses observational and model-based data to characterize the pair-wise dependence between various flood drivers (storm surge, waves, rainfall, river discharge) for sites across the entire CONUS and for different seasons. The authors then compare model-based dependence against the observed dependence structures between flood drivers to investigate the ability of models to represent multi-hazard dependence. I recommend publication after addressing some points I raise below.

Main Points:

- There are significant differences between the Kendall tau and tail dependence for the seasonal analysis. In particular, the kendall tau analysis suggests that the correlation is higher during the tropical season compared to extra tropical season (for the majority of locations and pairs). But the tail dependence analysis basically shows the exact opposite (figure 8 note all the blue boxes compared to the prevalence of red boxes in figure 6). The authors mention that some differences could be due to sampling differences and autocorrelation of the time series used for the tail dependence analysis, but I don't think this fully explains the large discrepancy between the two methods. Moreover, the authors do not comment on how to interpret these differences. Which metric should we use to quantify hazard dependence?
- I didn't see the KL divergence results presented anywhere in the manuscript. As the authors point out the KL metric takes into account the entire dependence structure rather than just the tail dependence, so I was expecting to see/read the KL values for different locations and pairs. I think this needs to be added to the manuscript
- I don't think the discussion could be organized better. Since one of the objectives/novelties of this work is to investigate hazard correlation across the entire

CONUS, the discussion should be used to highlight regional trends. I suggest devoting one paragraph for each region, highlighting the main hazard correlations (and the meteorological reasons for the correlation), and then describing which season multi-hazard events are more likely to occur and why. This should be followed by a paragraph discussing the differences between the tail dependence and kendall tau analysis (see the first bullet). Then I would have a paragraph describing the comparison of the model with the observations. There are a lot of results presented in this paper so the discussion really needs to summarize everything in an organized manner.

- I would also suggest adding some discussion about the limitations of using a bivariate approach for this analysis. For example, along the west coast of Florida, there is significant correlation between almost all the hazard pairs (Figure 3), suggesting that there may also be a significant threat for three or even all four of the hazards to occur simultaneously.

Specific Comments:

Abstract:

Line 33-34: change "elderlies" to "the elderly" and "the poor" to "low income communities"

Section 3.1: It's not clear to me why two different dependence measures are implemented in this work (Kendall tau and tail dependence). Both measures aim to investigate dependence between hazard extremes (either indirectly by selecting the maximum value per year or by directly utilizing the 90^{th} percentile threshold). What is the benefit of using two different metrics? What insight does the Kendall tau provide compared to the tail dependence (and vice versa)? The motivation for using these metrics needs to be stated explicitly here.

Figure 3: I suggest breaking up the East Coast region into two regions: Southeast and Northeast. It's clear from Figure 3 that the Southeast region (up to North Carolina) shows similarities with the Gulf Coast region. In both regions the occurrence of TC events likely causes significant dependence between surge/discharge and surge/precipitation. However, moving to the northeast there are relatively few sites with significant S_P/P_S or S_Q/Q_S correlation. Therefore, I think it makes more sense to talk about the Southeast and Northeast rather than the entire East Coast as a whole.

Section 4.2:

It doesn't really make sense to talk about a tropical cyclone season for the West coast. I think it's valid to look at the seasonal differences between dependence values for locations in the West coast, but the authors should provide some justification for why June-November vs December-May still makes sense since TCs do not impact the West coast.

Figure 5: Again, I think it would be more interesting to show the southeast and northeast regions separately.

Section 4.3:

Where exactly were the KL results presented? The authors mention that the KL divergence provides information about whether the dependence structure is significantly different between observations and model, but I did not find any figure/table presenting the KL results.

Figure 10: The caption here is not correct. The caption refers to differences between seasons but I believe this figure is supposed to be showing the difference between reanalysis and observations

Figure 12: Caption incorrect again.

Line 480-481: I don't understand this sentence: "not all extreme precipitation events can occur with moderate or high discharge, but extreme discharge events can occur with moderate or high precipitation". I think you mean that high precipitation can occur in the absence of high river discharge, but high river discharge usually occurs simultaneously with high precipitation.