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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Multivariate statistical modelling of extreme coastal water levels and the effect of climate variability: a case study in the Netherlands" *by* Victor M. Santos et al.

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We, the authors, thank the anonymous reviewer for providing a thorough and comprehensive review. We acknowledge their suggestions will help us improve the quality of the manuscript and hope the suggested amendments satisfy their concerns. Below we provide our point-by-point response to their comments.

1. In general, there are a lot of references to figures in the supplemental information that feel as if they are written in the same manner that one would normally refer to an in-text figure. If showing these figures are crucial to communicating the results, then I feel they should be in the main paper. Otherwise I suggest rewriting the sections (i.e.

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4.1.2, 4.1.3, 4.1.4, etc.) to explain the results in words without referencing a take-away point that a reader would need to see a figure to understand. You can then tell the reader that further information is available in the supplement.

There are too many figures in the supplementary material to be included in the main text. Yet most of them provide relevant insights to the study. We will therefore rewrite Section 4 to better explain the results in words while keeping the references to the supplementary figures within brackets.

2. The introductory paragraph refers to the same author/lab groups efforts in 5 straight individual sentences. While subsequent paragraphs show the author's have a broad grasp on literature beyond this one lineage, I recommend broadening the background to highlight that the motivation for this work does not arise simply from one group's efforts. There are many other works that have identified and attempted to account for multivariate climate drivers of compounding events (e.g. Anderson et al. 2019, Serafin et al. 2014, Rueda et al. 2016).

The introduction provides a comprehensive list of studies from different groups, which is not meant to include all relevant compound analysis studies to date but to give an overview of the state of the art. However, we will broaden the background by including a few more references as suggested by the reviewer but without lengthening the introduction excessively.

3. I'll admit I am confused by the tidal variability included in Figure 1. The text at Line 114 indicates that the tide cycle is added but doesn't give any specifics (I suggest adding these specifics to improve transparency). Figure 1 makes it look like all 800 events had the maximum occur at the same phase of the tide? Otherwise the bold tidal level would be a more flat line with a large envelope of variability around it? If the events do all occur at the same tidal phase then that would be a significant limitation of this work. Perhaps Figure 1 is only a single example taken from the 800 annual and the text caption for the Figure could be rewritten to prevent the interpretation that it is

derived from all 800 scenarios.

As stated below in our answer to question 6, we will add more details about the data. An artificial extension of the historical astronomical tide between 1950 and 2000 was added to the modelled storm surge data. Figure 1 is a composite (average) plot and therefore not taken from a single event but derived from all 800 annual maximum water levels. The catchment around Lauwersmeer is a managed water system in which gates open during low tide allowing the water to discharge into the sea by gravity, as mentioned in lines 148-150. Therefore, it is reasonable that the annual maximum water level always occurs at approximately the same phase of the tide (close to the minimum tide). This is, however, not a limitation of this work. Most water managed systems are expected to have similar discharge strategies. On the other hand, the framework proposed is general and it can be applied to any water system, once the appropriate predictors have been identified. In the revised version of the manuscript we will add a paragraph to discuss the transferability of our modelling framework to other study areas

4. Are copulas fit to purely empirical distributions? At which point the underlying assumption is that the 800 events can accurately represent the tails of the distributions? If this is the assumption being made then I think it should be explicitly stated in the manuscript and acknowledged as a potential limitation for obtaining extremes.

Yes, copulas are fit to empirical distribution, so the choice of the copula does not depend on the marginal distribution. This implies that we assume that 800 events can accurately represent the correlation between large percentiles of the surge and precipitation predictors, but not the tails of the distributions of the surge and precipitation predictors, as we use marginal density functions to obtain the final joint probability density function. This is a common approach followed in previous studies, even when using significantly shorter records (e.g. Jane et al., 2020). In lines 315-322 of the manuscript we discussed the challenges encountered when assessing the degree of compoundness for large return periods (e.g., 800 years)

Reference: 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.

5. Although a paragraph at the beginning of Section 2 does describe the study site, I think an annotated figure of the coast, the physical point where all data is obtained, and the square area or arial footprint of the watershed catching the precipitation could aid the manuscript. I was left wondering about the coastal configuration, proximity to open water, and proximity to human altered landscapes.

We agree with the Reviewer that a figure of the study site featuring the characteristics stated above may help readers to have a better understanding of the system. We will add a figure of the study area in the revised manuscript.

6. I think the paragraph between lines 112-120 could grow to be multiple paragraphs that detail the methodology from van den Hurk et al. (2015), as this manuscript is heavily dependent on that work.

We understand the suggestion of the Reviewer. However, we feel that adding multiple paragraphs talking about an already published work might seem redundant, as interested readers can refer to the other article. We will include more details about the methodology implemented by van den Hurk et al. (2015) while trying to be concise.

7. Although the explanation of copulas is suitable for publication, I think that the author's dynamical interpretation of the final copulas could be useful. By that I mean, why does a Frank copula fit better and what does that tell us about the dynamics of the compound hazard?

For the 2D case, we obtained a rotated Tawn copula (90 degrees) with negative (weak) correlation between the chosen predictors. As comprehensively explained in Section 4.1, the dynamical interpretation of the correlation coefficient is not straightforward. For example, a negative correlation between predictors does not lead to a negative correlation between the underlying drivers (surge and precipitation) as the predictors are conditioned to the impact variable. Comparison with the shuffled data reveals that drivers are indeed positively correlated, although the correlation is not very strong and therefore does not lead to a positive correlation between the conditioned predictors. This, for example, agrees with obtaining a copula with asymptotic independence between the predictors. We will briefly comment on the main traits of the chosen copula in the revised version of the manuscript. However, we prefer not to extensively discuss the interpretation of the copula of conditioned predictors as it might contribute to confusion about the physical understanding of the underlying (unconditional) drivers.

8. I think the usefulness of this case study to readers may be improved by including a commentary on what physical processes are or are not being wrapped up into the relatively broad predictors. Does the original modeling framework exhibit sea level anomalies at longer frequencies than just meteorological surges and tides (i.e. monthly or seasonal anomalies)? Does the location of this virtual tide gauge experience waves? Perhaps a paragraph at the end of the discussion could address limitations and how extensible the study is to other sites.

We agree with the reviewer about clarifying framework transferability and limitations in our manuscript. Although the results presented here are site speciïňĄc, the general framework can be transferred to other locations, given the availability of relatively long overlapping records of flooding drivers and impact variable. If the size of the database needs to be extended prior developing a multivariate statistical framework, a Regional Climate Model (RGM) and a hydrological management simulator to derive empirical estimates could be used (e.g., van den Hurk et al, 2015). Depending on the size and resolution of the RCM, appropriate computational resources may be required. Defining appropriate predictors that optimizes the performance of an impact function depends on the hydrological characteristics and management of a given system. For systems with low or no management, we would expect a more straightforward construction of an impact function, but appropriate lags between drivers and impacts should be accounted for. Characterizing probability distributions that precisely describe the marginals and fitting copulas that accurately capture the dependence structure largely depend on data availability.

The proposed framework assumes waves are not an important driver of inland extreme water levels, and only low-frequency sea-level components are accounted for. This is reasonable considering the characteristics of the study area: 1) sheltering effects of barrier islands protecting from extreme wave climate and 2) shallow waters inducing wave breaking for large wave heights. On the contrary, surge is a relevant driver of extreme coastal water levels in such shallow water environments. However, if our framework were to be implemented in areas exposed to extreme waves, ocean wave predictors would need to be included in the model. Yet the proposed framework described in Section 3 would still be valid.

The surge is calculated from the meteorological forcing for all relevant time scales, from daily to multi-annual, using the empirical relationship between surge and model generated wind. Apart from the astronomical tide no other sources of variability are incorporated in the sea level records. Therefore, the main limitation of this study is the exclusion of long-term nonstationary sea level processes, such as sea-level rise which plays a large role in increasing extreme water levels (Taherkhani et al., 2020). However, since our focus is on the assessment of historical extreme sea level climate with focus on the effect of climate variability, this assumption is reasonable.

Reference: Taherkhani, M., Vitousek, S., Barnard, P. L., Frazer, N., Anderson, T. R., & Fletcher, C. H. (2020). Sea-level rise exponentially increases coastal flood frequency. Scientific reports, 10(1), 1-17.

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