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Discussions

Interactive comment on "Compound flood potential in Europe" *by* Dominik Paprotny et al.

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We would like to thank the referee for taking the time to analyse our manuscript, and for his valuable comments. We list the comments (C) and our responses (R) below.

C: It is not easy to keep track of the many different names of datasets used in the dependence analyses. While some of datasets are summarized in Table 1, the level of details provided could be improved. For example, for precipitation observation it would be nice to also provide the dataset name (E-OBS), and the names of models used (i.e., Delft3D) could also be provided here. Additionally, it would help the readers to summarize the combinations that are examined in the study (section 2.1), perhaps in table labeling with corresponding figures/tables to allow for quick crosschecking. Some more descriptions could also help with the occasional confusion of which table columns are observations and which are models.

R: We will rework the table to make it more consistent with the text of the methodology and results, and we will add references to particular figures of the manuscript and supplement.

C: While the analyses with copulas seem robust, improving the readability of the copula method description would help the readers to understand the results better. In general the readers should not need to read a lot of the cited literature in order to interpret results comprehensively in this study. For tail dependence the authors provided an example in P10L15-16 and elaborated a bit in conclusions (P14L11, "i.e. extreme events are not more likely to co-occur than more moderate events"), which should be mentioned much earlier in the text to motivate the analyses of finding best fit copula and the implication of having Gumbel vs Frank copulas. Figure 3 could also add theoretical probability density contours figures for the two copulas along with the actual data in display, which can also help with the explanation of copulas and tail dependence, concepts that are central for further understanding the results. Additionally, the "Blanket Test" score (equation 2) could be provided in the modified figure 3 along with the plots to provide readers a better sense in interpreting the scores shown in the figures. In which software is this score computed (is it R)? Will there be cases that two or three copulas have quite similar test scores for some stations? It would help to also include a supplementary figure of theoretical probability density contours figures for all 7 copulas.

R: In accordance to the reviewer's suggestions, we will (1) make a clearer explanation of the significance of analysing tail dependency analysis; (2) add probability density contours to Fig. 3 with additional supplementary figures for all copula types; (3) we will provide M statistic scores for the examples in Fig. 3 and mention that the calculation was made by our own implementation in Matlab.

C: In describing the equations for computing the composite indices of compound flood potential, it is better to explicitly provide the equation for transforming each variable "into decimal logarithm and standardized in the interval [0,1]" (P6L12). This will also help to understand why it is “1-S1” in Equation 4 (P6).

R: We will add the equation to make the computation more explicit.

C: Have you considered other precipitation indices such as 5-day precipitation, and the dependence of precipitation and river discharge?

R: We only used one-day precipitation as a proxy for flash flood hazard, and river discharges for floods caused by rainfall of longer duration, hence analysis of multiday precipitation would be redundant as it the cause of extreme discharges rather than an additional hazard component. For the same reason we didn't consider precipitation-river discharge correlation.

C: Have you considered studying the compound flood potential under climate change? It may be beyond the scope of this paper but could probably be mentioned in discussion.

R: We haven't included future projections as we were more interested in ability of models to recreate observed joint distributions, and most results didn't show good accuracy of models. We will nonetheless expand the section 4.4 “Future research outlook” with discussion on how to analyse compound flood potential under climate change.

C: Most of the validations and analyses are performed with correlation. Have you also examined biases of the models? R: We did analyse the biases noticeable in the graphs, though we didn't compute indicators like NSE. We will add NSE in the revision to provide more explicit information on biases.

C: Are all the computed correlations rank correlations? If so please make consistent reference in text. R: Yes, all are rank correlations; we will make it more clear in text.

C: Have you examined how different the three precipitation datasets are i.e. in terms of multi-year mean spatial pattern for overlapping period, extremes and trends?

R: We didn't analyse those aspects as for comparing joint occurrence in observations

and models the timing of extremes is more relevant than the exact value of extremes, or differences in trends.

C: Among the river gauges, what is the largest difference (%) in catchment area with your selection criteria (P4L19)?

R: Not counting instances where catchment area was lower than the minimum grid size in EFAS (25 km2), the largest difference was 98% (average 15%).

C: In the results the performance of different model/forcing combinations are described, however I would like to see some more discussions on why certain combinations perform better, as not all notable performance differences are discussed.

R: The manuscript will be rechecked to make sure the text is comprehensive enough.

C: A bit confused on the explanation regarding difference in correlation (P8L21-22), would you perhaps elaborate a bit more?

R: In this case the results are influenced by the availability of observations for comparison. Comparison in Fig. 5a has more gauges in Fig. 5b because we didn't have observations from Mediterranean gauges that would cover years 1970-2005 (which is necessary to compare with models forced by EURO-CORDEX). Because the model in Fig. 5a had very poor performance particularly for Mediterranean stations, which are missing in 5b, the correlation in 5b is much higher. As we note further in the text, taking into account only the same set of stations for both figures would have yielded higher correlation in 5a. We will make this more clear in the text.

C: For figures showing rank correlation, would it be better to make grids with weak/insignificant rank correlation grey?

R: It would be possible, however we wanted to highlight areas where correlation turns negative and also to avoid confusion, as greying out areas with weak correlation could be interpreted by a reader as areas with statistically insignificant correlation.

C: There are a few sentences/typos that need to be corrected/clarified. For example, in P5L16 what do you mean by "rotated copula"? In P8L34 what does "78 or 43" mean? In P9L29 please explain "significant wave heights" for reader who may not be familiar with this variable. Also in L8L29 it should be Rˆ2 instead of R2. While already well written in general, it would be nice for the authors to review the paper thoroughly once more to make some necessary corrections in the text.

R: We will recheck the manuscript for errors and typos. Regarding the mentioned examples: (1) we will clarify that "rotated copula" meant assuming positive correlation instead of negative, and then taking the inverse of the marginal distribution, which is a necessary workaround in case of negative correlations in Gumbel and Clayton copulas; (2) we will clarify that "78 or 43" means the number of stations for which comparable data are available; (3) we will add a definition of significant wave height.

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