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Interactive comment on "Do climate-informed extreme value statistics improve the estimation of flood probabilities in Europe?" *by* Eva Steirou et al.

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The manuscript investigates the effectiveness of performing climate-informed extreme value analysis for flood probability estimation at the European scale. More specifically, the Authors analyze the effects of large-scale circulation patterns on seasonal extreme distributions by accounting for the relationship between extreme probabilities and climatic indices. As stated by the Authors, climatic indices are considered in recent literature works to justify or explain a non-stationary behavior depicted by extreme events. In this regard, the innovative contribution of this paper is to perform a large-scale analysis, at a spatial scale that is "comparable" to that of the climatic indices considered

in the work aiming at defining the conditional probability distribution of extreme flood events and proving coherent spatial patterns.

The manuscript is well written and organized; the methodology is almost well described, even if additional details could be included to help for reader understanding, and conclusions are well supported by results. Finally, within the Conclusion Section a detailed list of the limitations of the study is provided. Summarizing, the topic is of interest for the scientific community and the manuscript deserves to be considered for publication in this Journal. I have some comments about the work that are listed in the following paragraph; I hope that they will be helpful for manuscript improvement.

1. The Authors hint in the Introduction Section at the nonstationary framework incorporating climatic indices into flood frequency analysis, but they do not make a clear distinction between periodicity (or cyclo-stationarity) and trends (in the mean or variance). For the sake of clarity, this could be discussed from the very beginning of the manuscript (e.g. at line 49). Are the Authors assuming stationarity which is a "prerequisite to make inference from data", as discussed in detail by the cited papers by Koutsoyiannis and Montanari (2015) and Serinaldi and Kilsby (2015)?

2. At line 136 the Authors define the model driven by climatic indices as "climatic-informed model", justifying this choice based on the fact that "if covariates have a stochastic structure and no deterministic component, the resulting distribution is not truly nonstationary". I do agree on this, as the Authors states at line 135 that the climatic indices are stochastic process not showing clear trends. But, it is expected they are characterized by persistence and/or periodicity. A detailed description of the stochastic behavior of the climatic indices is missing in the manuscript, while they are clearly described from a physical point of view (lines 59-90). E.g. which are the relevant time-period and is the period covered by observations long enough to catch climatic indices periodicities?

3. Even if the aim of the work is to find results at the European scale, I would suggest the Authors to add a figure showing results for a single station, as an illustrative example to explain the methodology and the rationale behind it (e.g. the structure of the climatic informed GEV). Similar to figure 7, it could be of interest to show the evolution in time of the climatic indices (see comments 2) and the performance of classical GEV and climatic-informed GEV, especially for quantile extrapolation, with uncertainty bounds.

4. If I understand correctly, conditional models preferred o classical GEV in Table 1 are those respecting both criteria (minimum value of DIC and significantly different from zero coefficient of linear variation with the climatic indices); this could be highlighted in the result section from the beginning of the section. The number of times (stations) each conditional model is preferred with respect to classical GEV is not so high, being in the best case the 44% and on average at about 20%. The use of two criteria does not seem to affect this result much (as in lines 276-280); hence, the evidence of the climatic informed model does not appear to be very strong, even if clear spatial patterns emerge. The latter is the more relevant result, based on my opinion, and this should be stressed in the abstract and conclusion sections.

5. Since spatial patterns are influenced by correlations among climatic indices (that are illustrated in the supplementary material as spatial maps), I suggest the Authors to report in the manuscript a table summarizing cross-correlations among the indices (even if they are not an exhaustive measure of the underling complex physical phenomena).

6. Lines 276-279. DIC is a measure of model evidence; even if the climatic informed model has a smaller value of DIC with respect to classical GEV, the difference among the two values is probably not enough to results in a “strong evidence” of the first model compared to the second one. See, e.g., Kass and Raftery (1995)

where two different interpretations of the Bayes factor are provided.

   - Kass, R. E., Raftery, A. E. (1995). Bayes factors. Journal of the american statistical association, 90(430), 773-795.

7. Figure 7 compares conditional (climate informed) and unconditional quantiles considering p=0.01 for three stations. It should be clearly stated that conditional quantiles are computed in this case based on the observed values of the climatic indices year by year.

8. As the climate informed models have a larger number of parameters (one more in this case) to be estimated based on data, it is expected that their uncertainty bounds are larger than those provided by classical GEV. In other words, nonstationarity flood frequency analysis adds an additional component of uncertainty if the model between parameters and covariates is estimated from data and not fully a-priori defined based on additional physical information (Serinaldi and Kilsby, 2015). However, this is not what emerges from figure 7. This issue should be clarified.

9. Lines 329-330. This should be true if the climate indices can be accurately predicted. The issue should be discussed further since it is closely related to the implications of the results presented in the paper for practical applications. Furthermore, I’m asking myself if the improvement in flood quantile estimate at the local scale thanks to climate indices is really significant from a practical point of view given the large uncertainty that characterizes all the estimates (fig. 7); I would like to read a comment on this from the Authors.

10. Line 58. The Authors could also consider the recent paper from Serinaldi et al. (2018) discussing limitations of nonstationary detection based on trend tests.

   - Serinaldi, F., Kilsby, C. G., Lombardo, F. (2018). Untenable nonstationarity: An assessment of the fitness for purpose of trend tests in hydrology.

Advances in Water Resources, 111, 132-155.

11. Line 71. A reference is needed.
12. Please definite t after eq. (3).
13. Line 173. Please define what is meant by non-informative priors in this case. If the non-informative prior is a uniform distribution, its support (range of variability of the random parameter) could have effects of posterior distribution and evidence estimation.
14. Eq. (8). y or Y?
15. Line 194. Please define $\bar{\theta}$.
16. Line 219. Are the Authors assuming a Gaussian (marginal) distribution for climatic indices? The assumptions on those variables and their stochastic behavior are not clear (see also general comments 2 and 3).