Remarks on the manuscript egusphere-2023-1318

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I have a few remarks that may help improve the manuscript.

Concerning the limited use of upper/lower tail dependence coefficients in the hydro-climatic literature, I suggest improving the literature review. This would reveal, for instance, that most of the estimators of tail dependence coefficients $\lambda_{\mathrm{L/U}}$ are strongly biased, yielding positive values even if the dependence structure has $\lambda_{\mathrm{L/U}}$ equal to zero. This depends on the fact that these estimators (including Schmidt-Stadtmüller and Capéraà-Fougères-Genest) rely on the implicit or explicit (but not negligible) assumption that the underlying dependence structure is actually characterized by upper tail dependence. In other words, while these estimators may be considered non-parametric in the sense that they do not require the specification of a given copula family, they are strongly parametric in the sense that they require that the underlying copula belongs to a very specific class of models (i.e. those with true tail dependence, basically EV copulas, copulas belonging to EV attraction domain, or similar). These issues are discussed in depth by Serinaldi et al. (2015).

Shuffling procedure should be better explained. If the time series of 3-month precipitation P and SM are shuffled by keeping the correspondence of the observed pairs (P_i, SM_i), this destroys the (possible) serial correlation but keeps the the overall cross-dependence, and therefore summary statistics such as Kendall τ_{K} and $\lambda_{\mathrm{L/U}}$. On the other hand, if the shuffling procedure does not retain the pair-wise correspondence between the observed pairs (P_i, SM_i), it destroys the whole cross-dependence structure, not only the upper tail dependence. However, samples resulting from the latter procedure are not informative for the problem at hand. In fact, to build the confidence

intervals (CIs) in Fig. 2, we need samples reproducing all the properties of the observed samples but the tail dependence. Roughly speaking, we need samples keeping e.g. the values of Kendall τ_K but with $\lambda_{L/U} = 0$. This is fundamental for a fair assessment of the actual width of the CIs because the above-mentioned estimators of $\lambda_{L/U}$ are biased, and the estimates of $\lambda_{L/U}$ are strongly related to the global dependence measured by e.g. Kendall τ_K (see Serinaldi et al., 2015). If the shuffling procedure keeps the overall cross-dependence removing the upper tail dependence only, therefore the CIs are OK, but the Authors should explain in more detail how they shuffled the data to obtain this effect. Conversely, if the shuffling procedure is just a naïve resampling (bootstrap) destroying any form of dependence, CIs refer to a case which is not comparable with the estimates coming from the observed samples. In other words, CI width is strongly underestimated, and tail symmetry (under sampling uncertainty) cannot be excluded for much more than just the $\cong 50\%$ of locations.

Since the copula-based analysis and modeling reported in the manuscript require independent samples of the pairs (P_i, SM_i) (leaving the above-mentioned bias issues aside), I take for granted that the data are pre-processed to account for seasonality and serial correlation as well as spatial correlation across the region. In this respect, more details about how this is done can help reproduce analysis and results.

Sincerely

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References

Serinaldi, F., Bárdossy, A. Kilsby, C.G. Upper tail dependence in rainfall extremes: would we know it if we saw it?. Stoch Environ Res Risk Assess 29, 12111233 (2015). https://doi.org/10.1007/s00477-014-0946-8