

## ***Interactive comment on “Joint return periods in hydrology: a critical and practical review focusing on synthetic design hydrograph estimation” by S. Vandenberghe et al.***

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To the best of our knowledge, this is the first paper that provides a thorough review and analysis of different approaches to the definition of the Return Period in a multivariate context. We appreciate its approach and content, and we endorse its publication. In order to make the paper even more interesting and useful for the potential readers, some remarks are listed below: these might help the Authors to fix some points.

**Page 6794, Line(s) 14–16.** The Authors claim: “Instead of using Eq. (9) to select the most likely point, the full likelihood function  $f_{XY}$  over the  $t$ -isoline could be seen  
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as a univariate density function (PDF)...”.

Note that  $f_{XY}$  is only a “proxy” of the density, or, better, a “weight function”, as pointed out in [1]: it does not integrate to 1. Same remark at page 6795, lines 3–5.

**Page 6794, Line(s) 23–25.** The Authors claim: “This ensemble could then be used to run simulations from which detailed information on the uncertainty of specific design parameters can be assessed...”.

The use of “ensembles” may provide information about the statistics of critical events, but it does not yield a design vector: thus, it should be considered as an auxiliary (and not as an alternative) step of the whole assessment / identification procedure.

**Page 6795, Line(s) 15.** The Authors claim: “The SDH is defined as a hydrograph with an assigned return period...”.

What notion of return period is considered here? Univariate? Multivariate? Please make it clear.

**Page 6799, Line(s) 18–19.** The Authors claim: “These different distribution types each represent a different kind of tail behaviour, namely a light tail (Gumbel), a heavy tail with infinite variance (Fréchet)...”.

Note that the heavy tail of the Fréchet law does not necessarily yield a distribution with infinite variance (it depends on the value of the shape parameter).

**Page 6799, Line(s) 26–27.** The Authors claim: “Therefore, the distributions are fitted to the data after subtraction of the minimum value to ensure a proper fit in the tails...”.

Instead of modifying the data base, it would have been better (and enough) to add and fix a suitable position parameter into the fitted distribution.

**Page 6800, Line(s) 8–9.** The Authors claim: “Despite this, the GEV does not fit  $D$  well, even though various statistics showing it as the best fit. Seemingly, the lower portion of the curve is reasonably well approximated by a GEV, resulting in the significance of its fit, despite the poor representation of the tail. . .”.

We are not completely convinced by this explanation. We would suggest to try and check the survival function, and see whether the empirical distribution shows a linear tail behavior in a log-log plane (i.e., a Pareto-like behavior), or a linear tail behavior in a semi-log plane (i.e., an Exponential-like behavior). Also, the  $p$ -value for the Exponential distribution is small (only 0.0443): the Authors should try and fix this point, maybe by testing some other univariate laws.

**Page 6801, Line(s) 1 and 4.** The Authors claim: “Also, some ties are present, especially for  $D$ , but they will be neglected . . . we use maximum likelihood estimation to fit a copula. . .”.

The Authors should explain how they deal with the ties, i.e. how they fix and rank the pseudo-observations needed to fit the copula: this is a fundamental point when constructing a copula-based model, and a better explanation may be useful for the potential readers.

**Page 6801, Line(s) 21–22.** The Authors claim: “Thus, the vine-copula yields the best fit within this set of copula families. . .”.

Apparently, Figure 3 shows a singular component for  $(Q_p, V_p)$ , but, in principle, the vine-copula construction is unable to play with it (absolutely continuous copulas with density are required). The Authors should discuss and make this point clear.

**Page 6806, Line(s) 7–9.** The Authors claim: “It is also evident that the more variables are included (2-D vs. 3-D), the smaller the design quantiles become as more complexity of the process of generating extremes is captured. . .”.

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This phenomenon is known as the “dimensionality paradox”, and is discussed and theoretically motivated in [2]. The fact that the design quantiles get smaller by increasing the dimensionality of the problem is not due to the fact that the larger the dimension the better is the model (a better description of the phenomenon does not necessarily imply that the design quantiles are smaller).

**Page 6807, Line(s) 4–6.** The Authors claim: “Finally, a practitioner should also be very aware of the fact that the JRP methods based on the Kendall distribution function as presented here are only valid for variables that are positively associated, and with a focus on extremes in terms of large values. . .”.

This claim is misleading, and should be discarded. (1) It is false that “the JRP methods based on the Kendall distribution function are only valid for variables that are positively associated”: the Kendall’s approach works for any copula, showing either a positive or a negative measure of association. (2) It is false that “the focus is on extremes in terms of large values”: general formulas are given, and the Kendall’s approach works both for, say, floods and droughts (i.e., large and small values).

**REFERENCES.** The bibliography should be fixed, since several errors and wrong citations are present.

## REFERENCES

- [1] Salvadori, G., De Michele, C., and Durante, F.: “On the return period and design in a multivariate framework”, *Hydrol. Earth Syst. Sci.*, **15**, 3293-3305, doi:10.5194/hess-15-3293-2011, 2011.
- [2] Salvadori, G., and De Michele, C.: “Multivariate Extreme Value Methods”. In *Extremes in a Changing Climate*, AghaKouchak, A., et al. Editors, Springer, 2012 (in press).

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