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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatially dependent Intensity-Duration-Frequency curves to support the design of civil infrastructure systems" *by* Phuong Dong Le et al.

Anonymous Referee #3

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In general, the paper is well written. However, I have some concerns regarding the real contribution (novelty), connection with the literature and in particular with copula studies, as well as comparison with other models. Main comments: 1. Some important papers related to the topic are missing and more importantly the comparison with them not only in terms of results but also in terms of advantages and drawbacks (e.g. Bardossy and Pegram, 2009, Durocher et al. 2016 and Requena et al. 2018). 2. Regarding the issues motivating the study: the first one seems to be already fixed by Le et al. 2018b (as indicated on page 5), and the second issue is not clear (seems to be written as a statement not as an issue). 3. The topic can also be closely related to regional frequency analysis or estimation at ungauged basins. The authors did not make

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this connection or show the difference. In the first case (similarity or connection), a huge literature exists and should be considered. 4. The paper focused on a case study (a given set of data). However, the effect of some factors on the performance of the model as not discussed and not studied: for instance, and not limited to, the dimensionality (number of sites) and the size of the subgroups. 5. An important missing element from the paper is the notion of copulas which is the most important when dealing with dependence. There is a huge literature in both hydrology and statistics (even in spatial dependence). I'm surprised to not see it in the paper. 6. In section 4: why the GPD is used directly without model selection procedure? Why it is the same for all sites? The GPD is usually asymptotically justified which is not enough (and less justified in hydrology because of the sample sizes) and does not depend on the data at hand. It should be considered as a distribution among others (like GEV for block extremes). 7. Lines 245-248: please provide other alternative models and justify the choice of your model. 8. The assumption, on page 11 line 215, is it reasonable? Is it verified in your case study? 9. How the hydrological model (ex. WBNM) is integrated in the steps of fig 4? Other comments: 1. Fig 4: Why in the independent model, no fitting is required? What it means? 2. Sentence from lines 237-240 is long and not clear. Please consider reformulating. 3. Page 13: this text requires to be more accurate about the terms and notation. 4. Lines 287-290: is this case not covered by equation 4? 5. All text in page 16 and part of page 17 seems trivial and does not worth all this space. Other more important information deserve this space. 6. It is not clear in section 4.6 if the authors consider one hydrological model (WBNM) or other models (see for instance lines 376 and 384). 7. Line 408 : how you can say the model has reasonable fit? Based on what? And compared to what? 8. Line 538 : I'm not sure about this statement. It is not true in many situations.

Durocher, M., Chebana, F., & Ouarda, T. B. (2016). On the prediction of extreme flood quantiles at ungauged locations with spatial copula. Journal of Hydrology, 533, 523-532.

Bárdossy, A., & Pegram, G. G. S. (2009). Copula based multisite model for daily precipitation simulation. Hydrology and Earth System Sciences, 13(12), 2299-2314.
Requena, A. I., Chebana, F., & Ouarda, T. B. (2018). A functional framework for flow-duration-curve and daily streamflow estimation at ungauged sites. Advances in Water Resources, 113, 328-340.

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