Title: Spatially dependent Intensity-Duration-Frequency curves to support the design of civil infrastructure systems

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Response to the Reviewer #3

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:

Response: Thank you for your comments. We respond in detail below (your comments in italic font and our responses in normal font).

Major comment #1:

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).

Response: Thank you for the suggestion. We will add discussion on these paper to the revised manuscript.

Major comment #2:

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).

Response: Thank you for pointing this out. We will rewrite this section to make it clearer. The second issue relates to the spatial properties of asymptotic dependence (explored in Le et al., 2018a). While these two issues have been separately addressed in previous papers, the contribution is to show how to combine the methods to solve a realistic design problem.

References used for this response:

Le, P. D., Davison, A. C., Engelke, S., Leonard, M., and Westra, S.: Dependence properties of spatial rainfall extremes and areal reduction factors, Journal of Hydrology, Submitted, 2018a.

Le, P. D., Leonard, M., and Westra, S.: Modeling Spatial Dependence of Rainfall Extremes Across Multiple Durations, Water Resources Research, 54, 2233-2248, 2018b.

Major comment #3:

3. The topic can also be closely related to regional frequency analysis or estimation at ungauged basins. The authors did not make this connection or show the difference. In the first case (similarity or connection), a huge literature exists and should be considered.

Response: Thanks for your comment. We will discuss differences to regional frequency analysis and methods of estimation in the revised manuscript.

Major comment #4:

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.

Response:

Thanks for your comment. We will review the methodology more closely to include additional details on factors of the model performance, including discussion on the effects of additional sites.

Major comment #5:

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.

Response: We will add literature on copulas into the revised manuscript.

Major comment #6:

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).

Response: Thank you for this comment. We used the GPD because, in contrast to block maxima, it allows us to consider concurrent rainfall extremes and therefore enables the study of dependence. The intention in this paper is not to work through repetitive fitting of different distributions, but to demonstrate a plausible method based on joint rainfall extremes for the design of linear infrastructure. The same distribution is used at each site with variation at each site carried by the parameters. The marginal model adopted is not perfect, but it is plausible, and sufficient for the intent of showing the application of rainfall dependence to design.

Major comment #7:

7. Lines 245-248: please provide other alternative models and justify the choice of your model.

Response: Thank you. We will add justification of the choice of the Brown-Resnick model in the revised manuscript. For example, Le et al. (2018a) show it has better performance than the extremal-t model.

Le, P. D., Davison, A. C., Engelke, S., Leonard, M., and Westra, S.: Dependence properties of spatial rainfall extremes and areal reduction factors, Journal of Hydrology, Submitted, 2018a.

Major comment #8:

8. The assumption, on page 11 line 215, is it reasonable? Is it verified in your case study?

Response: Thank you very much. The assumption of AEP neutrality in rainfall-runoff design is a standard assumption when using IDF curves. While the assumption is in widespread use, it is not without limitation and we will provide brief discussion and reference to two papers that explore this issue.

Bennett, B., Leonard, M., Deng, Y., & Westra, S. (2018). An empirical investigation into the effect of antecedent precipitation on flood volume. Journal of Hydrology, 567, 435-445.

Rahman, A., Weinmann, P. E., Hoang, T. M. T., & Laurenson, E. M. (2002). Monte Carlo simulation of flood frequency curves from rainfall. Journal of Hydrology, 256(3-4), 196-210.

Major comment #9:

9. How the hydrological model (ex. WBNM) is integrated in the steps of fig 4?

Response: The hydrological model (i.e. WBNM) is used to transform the conditional rainfall to conditional flow. A label will be added in the revised version of the manuscript to show this (on the arrow between the see the squares for Section 4.5 and Section 4.6 in the top-right of Figure 4).

Minor comment #1:

1. Fig 4: Why in the independent model, no fitting is required? What it means?

Response: Thank you for pointing this out. The term "the independent model" here is not clear. We will change it to "the case of independence" and will clarify that we mean the case where rainfall extremes occur independently in space.

Minor comment #2:

2. Sentence from lines 237-240 is long and not clear. Please consider reformulating.

Response: Thank you. We will reword these sentences in the revised manuscript.

Minor comment #3:

3. Page 13: this text requires to be more accurate about the terms and notation.

Response: Thank you very much. We will clarify this text in the revised manuscript.

Minor comment #4:

4. Lines 287-290: is this case not covered by equation 4?

Response: Thank you. We will rewrite this comment on equation 4. We will clarify that the equation can be used for both cases, but that difference parameters will be required, since the dependence of a short duration extreme given a longer duration extreme is not the same as the dependence of a long duration extreme given a short duration.

Minor comment #5:

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.

Response: We will put remove this material and put it as supplementary, which will create significantly more space. We would like to leave it in supplementary material rather than remove it outright because some readers may prefer the straightforward explanation given the practical design focus in the paper.

Minor comment #6:

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).

Response: Thank you for your comment. There is only one type of model (WBNM), but different configurations for each catchment. We will clarify this in the revised text.

Minor comment #7:

7. Line 408 : how you can say the model has reasonable fit? Based on what? And compared to what?

Response: Thank you. We will more explicitly indicate that the comment on fitting relates to Figure 8. We will also emphasize that the main feature of the model shown in these figures is the relationship at h=0, for the case of dependence between two different durations at the same location.

Minor comment #8:

8. Line 538 : I'm not sure about this statement. It is not true in many situations.

Response: Thank you for your comment. We will restrict our commentary to conventional hydrological design that is based on IDF curves, which is more defensible than the original comment which was too general. By construction IDF curves are focused are point-wise estimators of extremes, thus a given design is focused on independent application of univariate statistics.