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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Evidences of relationships between statistics of rainfall extremes and mean annual precipitation: an application for design-storm estimation in northern central Italy" *by* G. Di Baldassarre et al.

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## General statement

The paper formulates a method for regional design storm prediction based on the index storm approach. Regionalisation, that is the selection of homogeneous regions, is replaced by defining regionally-applied relationships between site statistics and mean annual precipitation (MAP). Together with the assumption of an extreme value distribution for the annual maximum precipitation of different durations, this leads to spatially distributed estimates of design storms. The method is developed on precipitation data from a large region in Italy.

HESS is not averse to publishing methods and procedures in hydrological analysis, even if they are not completely new. In this case, I see the deficiency of the current version of the manuscript in that (a) in some parts it does not describe the methods and their statistical basis in sufficient detail, and that (b) it does not inform about the uncertainty involved in the estimation of the design storms. Since this is a paper about an application of a design storm estimation method, it should be possible for readers to be able to unambiguously reproduce the method in other regions, being aware of the assumptions made and the errors involved. In my opinion, the manuscript does not reach this goal in its current form, but can reach it after an adequate revision.

This paper was solicited after the first author won the YSOPP award at the EGU meeting. However, like all papers submitted to HESSD, it is undergoing a regular peer-review process. The manuscript was reviewed by two reviewers who are experts in the field of precipitation analysis and modelling. I encourage the authors to consider the suggestions of the reviewers (some of which are repeated here) and prepare their revised manuscript accordingly. I hope the authors recognise that the efforts of the reviewers and the editor are directed at improving the manuscript from its current form, and that their comments are a valuable opinion. In particular, the criticism of one of the reviewers (Y. Alila) that the paper lacks an attempt at the physical interpretation of the observed extreme precipitation behaviour is to be taken seriously. I am convinced that from their results and rich experience the authors can make some statements about the hydrometeorological processes underlying the data, which will be extremely beneficial to the readers of their final paper.

Based on the comments of the two reviewers, the author's response to one of them, unpublished comments of a third reviewer, and my own reflections, I identify four areas that need to be addressed (in addition to the comments of the reviewers already published in HESSD) in the revised manuscript.

**(1) Homogeneity testing of the region (section 4.1.1)**

The authors use a statistical test to evaluate the degree of homogeneity in stations grouped by MAP. In the original test of Hosking and Wallis (1993), it is assumed that in a homogeneous region the data come from the same parent distribution. Weighted (with record length) average L-moment statistics are used to find the “best” parent distribution, from which samples of the same record lengths and site number are drawn, and the between-site variability of sample and generated L-moments are then compared. Hosking and Wallis (1993) propose several measures V to come up with the heterogeneity measure H, of which the authors use two (p.2401). The authors should add the formulas that are behind these two measures H(1) and H(2) in the text or an appendix.

It is important here that in the method proposed by Hosking and Wallis (1993), the observed variability in L-moments is compared with the mean variability of the simulated data, e.g. like in the application by Alila (1999). From the description of the homogeneity testing in the manuscript on p.2401 it is not clear whether the authors follow through in the same way. It appears that H was not computed by comparing observations with simulations because no simulation is mentioned. This has to be clarified and/or corrected.

Of course, the authors apply the homogeneity test in a slightly different way, they choose a moving window of sites ordered with the site MAP, and test the homogeneity of this subset of sites. The choice of the number of sites included in the subset is rather arbitrary, as was pointed out by one of the reviewers in his comment (P. Bernardara). The authors should consider addressing the issue of the number of sites in the revised manuscript. By increasing the number of sites one necessarily increases the heterogeneity. In the limit, when all sites are included regardless of MAP, one arrives at the heterogeneity measure of the whole region. The whole study region in the manuscript is “certainly heterogeneous” from the point of view of lower order moments as can be seen from the caption of Figure 6. This should also be mentioned in the text of



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the manuscript because it underlines the advantage of using MAP as a regionalisation variable.

**(2) Confidence interval of empirical regional model (section 4.1.2)**

The empirical regional model is formulated and fit to the site L-moment ratios (L-CV and L-CS) as a function of MAP for different precipitation durations. I believe it is important to state here which optimisation procedure was used. For instance, because the uncertainty of the L-moment estimators is dependent on sample size as was pointed out by one of the reviewers (P. Bernardara), it may be wise to weight the site L-moment ratios in fitting the empirical model by the site sample size. Could the authors please comment on that?

The authors describe in length how the reliability of the empirical model was assessed through Monte Carlo simulations. This appears to me to be a standard way of generating confidence limits by simulation. I ask that the authors clarify whether this is the case, or what the novelty is here that I miss. In fact, Hosking (1990) and others have shown that L-moments are remarkably distribution independent, that is asymptotically (with large n) L-moment estimates converge to a multivariate normal distribution, where only the variance is a function of the underlying distribution. Figure 7 suggests that the generated confidence limits are symmetric, so perhaps normally distributed. In this context I also ask that the authors explain the need for the results in Table 4. If the data are iid then using the procedure above at the significance level of 5% one would expect on the average that 5% of the sample values lie outside of the interval, same is true for 10%. What is the significance of the fact that the values reported in Table 4 are not exactly coincident? If this is found not to be significant, please drop the table.

**(3) Performance of the regional model**

In the abstract and in the conclusions the authors state that “the proposed model is able to reproduce the statistical properties of rainfall extremes observed for the study region”. However the authors do not analyse the performance of the model in this



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paper in the context of reproducing precipitation extremes.

For example, it would have been relatively straightforward to conduct a Monte Carlo analysis to show if the regional model produces more accurate estimates of the design storms than at-site GEV distribution fits, e.g. similar to the analysis of Alila (1999). Another option would have been to estimate the accuracy of the regional model on ungauged sites by jacknife analysis, as the authors did in Brath et al. (2003). In my opinion such an analysis is acutely needed in a paper that presents a new method/model/dataset for prediction purposes. Can the authors please comment on this?

An additional interesting element was mentioned by one of the reviewers (P. Bernardara), in that the uncertainty in the predictive model of h(d,T) in equation (1) comes from uncertainty in the index storm and in the growth factor. Which of these two uncertainties are more important? Is it possible to quantify them relative to each other?

Finally, it is mentioned in section 2.2. and shown in section 4.2 how the uncertainty in the spatial interpolation of the index storm may be determined; and an example for 1-hour, 24-hour precipitation and MAP with two kriging approaches is given. Like one of the reviewers (P. Bernardara) I do not see why the MAPr method which accounts for the influence of elevation should perform worse than the MAP method with ordinary kriging. The authors state rather vaguely that this is due to the “geographic area considered herein and the particular raingauge network of our study”. I believe an attempt to explain this better would be beneficial.

**(4) Dependence between sites**

In conducting statistical analyses of the type of data used in this study it is commonly assumed that the data at the sites are independent and identically distributed. I do not find the issue of spatial dependence mentioned in the manuscript? Was spatial independence tested? Is this issue not important in the context of this paper? I would ask that the authors comment on that.

**Technical suggestions**

a) p.2395, line 25: You mean to say that the index storm is site dependent?

b) p.2397, bottom: Please mention some arguments why the L-moment method is superior to others, and therefore used here.

c) p.2400, line 5: Please remove Figure 4 and the reference to it. The data shown in Figure 4 are also shown in Figure 5, the comparison with the empirical relationships of Alila (1999) are not relevant at this point.

d) p.2411: I suggest to join Tables 2 and 3 into one.

e) p.2412: Remove Table 4 (see discussion point 2 above).

f) p.2418: Remove Figure 4 (see technical suggestion c above).

**References**

Alila (1999): A hierarchical approach for the regionalization of precipitation annual maxima in Canada, J. Geophys. Res., 104(D24), 31,645-31,655.

Brath, Castellarin and Montanari (2003): Assessing the reliability of regional depth-duration-frequency equations for gaged and ungaged sites, Water Resour. Res., 39(12), 1367.

Hosking (1990): L-moments: Analysis and estimation of distributions using linear combination of order statistics, J. R. Statist. Soc. B, 52(1), 105-124.

Hosking and Wallis (1993): Some statistics useful in regional frequency analysis, Water Resour. Res., 29(2), 271-281.

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