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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Regionalizing non-parametric precipitation amount models on different temporal scales" *by* Tobias Mosthaf and András Bárdossy

Anonymous Referee #2

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Paper summary

The study by Mosthaf and Bárdossy "Regionalizing non-parametric precipitation amount models on different temporal scales" establishes a comparison between different parametric models and a new proposed non-parametric method, used for regionalization of rainfall distributions over various temporal scales (ranging from 1 hour to 1 month), in the federal state of Baden-Württemberg, in Germany. Especially for sub-daily scales, the Authors consider rainfall records which are higher than predefined quantile values, to account for measurement errors associated with low rainfall intensities. The basis of the proposed approach is an observed persistence of the empirical distribution functions over different quantiles, which is apparent for all measurement

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locations in the region of study. For the evaluation of each method's performance, two distribution measures are adopted, while results show that parametric approaches are overall better than the proposed non parametric one, for scales higher than 2 hours. Also, it is shown that the use of additional information from daily measurements is beneficial.

General Comment

Although the subject of the manuscript is of interest and the idea on which the proposed approach is based is new, the overall level of the manuscript and significance of work is not sufficient. Thus, a suggestion of major revisions or rejection applies. Below, I explicitly refer to each one of my concerns.

Major Comment 1

A first major concern is the excessive length of the manuscript: The text consists of 22 pages (single line spacing), while the Authors also present 8 tables and 10 figures. My suggestion is for the Authors to reduce the length of the manuscript at least 25%. Indeed, there are some sections that could be provided in the Appendix, or as supplementary information, or even completely removed. The latter is also true for some tables and figures. For example, since the article is not a review paper, sections 3, 6 8.1-8.2, and 9 should be reduced in size and some parts should be provided in the Appendix. Evidently, the proposed methodology is explained quite "late" in the text (page 13), while its application and comparison to parametric models is provided in page 20. Similarly, apart from the tables and figures associated with the above sections, table 6 essentially provides the same results as table 5. There is no need for both tables. Although, the Authors can more efficiently decide which parts of the text should be reduced in size, my point is that the present version of the manuscript is too explanatory in some cases including unnecessary information. In other words, the text should be more focused on the findings of the present work.

Major Comment 2

A second concern is the actual innovation and value of the presented work. Although the basis of the proposed non parametric approach is new and of potential interest, according to the obtained results, the parametric models are more effective, both in terms of point-wise estimation (Tables 4, 5 and 6) and regionalization (Tables 7 and 8). Evidently, with the only exception being the hourly rainfall, where the non parametric approach is consistently the best performing one for both samples 1 and 2, and both seasons, overall, the parametric models result in smaller distributional-related errors. Moreover, even in the case where the non parametric and the parametric methods would be of the same overall performance, the parametric approaches may again be preferred since they can be more effectively used for addressing risk and estimating rainfall extremes in periods different than the control one (i.e. 1997-2011): contrary to the non-parametric approaches, theoretical distribution models allow for more robust rainfall estimates, with approximate validity also beyond the range of the historical data in the considered control period (see Langousis et al., 2016a and references therein). That said, although the idea presented in sections 7 and 8.3 is potentially important, the results and the associated discussion in the rest sections do not support or indicate a substantial innovation or significance.

Major Comment 3

The parametric models used in this study (section 6.2), although four in number, do not include a Pareto distribution. In their conclusions, Authors mention that Pareto distributions can be also tested in the future, however, in my humble opinion, this is not sufficient. At least for the comparison section to be complete, one should include in the analysis a Pareto model (e.g. Generalized Pareto Distribution) in this study, where the proposed approach is explained and compared with other common methods. Pareto distributions have been indicated as a very efficient class for modeling daily rainfall, while towards the latter, some studies have concluded that they outperform exponential models (see Papalexiou et al., 2013; Langousis et al., 2016b and references therein).

Comment 1

In section 5, the Authors assess the consistency with which one can apply Ordinary Kriging for interpolating rainfall distributions. In doing so, they evaluate the "similarity" between different rainfall distributions for increasing distances for all temporal scales, and they present the results in Figure 3. In lines 9-10, page 6, the Authors state: "The graphs in Fig. 3 show a decreasing similarity of the distribution functions with increasing distances over all temporal resolutions, as. . .". However, the latter comment is not accurate for temporal scales higher or equal to 1 day. Evidently, there is no significant difference in the value of the adopted statistic even for distances on the order of 40 km. More discussion or even investigation is needed on this matter.

Comment 2

Lines 5-7, page 14: How the interpolation of the non parametric distribution functions is established to the target location? Is this done based on the new proposed approach described in section 8.3? If yes, it should be explained more explicitly. If no, then the Authors should include the approach used in section 9 (for cdf idw), in the comparison of section 11.3.2.

Comment 3

Lines 4-6, page 18: The readers are not able to validate these statements. The rankings provided in tables 5 and 6, are combined, i.e. they summarize the performance of each model based on both criteria (38) and (40). Due the latter, the discussion of Figure 8 can be challenged as well.

Comment 4

Table 5 shows remarkably high errors (about 5000) in the performance of the exponential and mixed exponential models in case of the monthly precipitation amounts, for both seasons. Considering that Table 5 corresponds to wise point estimation, such high errors indicate complete inconsistency in the fitting of each model to the data, which is not reasonable to me, especially if on considers that in the case of 5-days

scale, the corresponding errors are on the order of 30. The Authors should discuss this result.

Comment 5

Line 15, page 9: Instead of MOM, why not using L-moments estimator (PWM)?

Comment 6

Concerning the level of writing, apart from the length of the manuscript which has already been discussed (see major comment 1), there numerous cases of ambiguity and typos, which means that the Authors need to refine their text. Below, I mention just a few examples: 1) line 25, page 1: "In order to run. . .for these sites.". Ambiguous sentence, please rephrase. 2) line 25, page 3: "only gauges with. . . are chosen.". Please explain better. 3) equation (1): since this equation refers to precipitation amount (see line 13 in the same page), please replace $-\infty \leq x < \infty$ with $0 \leq x < \infty$. 4) line 9, page 5 (and in other points throughout the manuscript): please replace "0.95 Qth" with "Qth =0.95". 5) lines 16-17, page 5: "85% is defined. . ." Ambiguous sentence, please rephrase. 6) line 9, page 5: "between 0.2 and 1.7 mm". This is inconsistent with the corresponding value in Table 1.

References

Langousis, A., A. Mamalakis, M. Puliga and R. Deidda (2016b) Threshold detection for the generalized Pareto distribution: Review of representative methods and application to the NOAA NCDC daily rainfall database, Water Resour. Res., 52, doi:10.1002/2015WR018502.

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Papalexiou, S.M., D. Koutsoyiannis and C. Makropoulos (2013) How extreme is

extreme? An assessment of daily rainfall distribution tails, Hydrol. Earth Syst. Sci., 17(2), 851-862.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-458/hess-2016-458-RC2-supplement.pdf

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