Review #2 of

"Regionalizing non-parametric precipitation amount models on different temporal scales"

submitted to *Hydrology and Earth System Sciences*

March 2017

My initial suggestion for the manuscript "Regionalizing non-parametric precipitation amount models on different temporal scales" was "major revisions" accompanied with nine comments. The authors addressed some of my comments adequately. Major comments 2 and 3 were not addressed. Below, I comment on authors' responses (red-colored text) concerning the two unaddressed comments.

Comment 1

Reviewer 2:

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.

Authors:

Answer: We disagree with the first part of this comment, where the reviewer proposes not to publish this manuscript, because the non parametric method only performed best for the hourly resolution. If we only had shown results for the hourly distribution, this statement would possibly have been vice versa. However, we presented the results for several temporal resolutions, as we also wanted to present the deficiencies of the newly developed non-parametric method. Even if the method performed worse over all temporal resolutions, we would consider it as important to publish the method. This may prevent the investigation of this method by another hydrologist and further more the methodology could be applied to distributions corresponding to other variables (where e.g. multi modal distributions are present). Additionally, we have shown that daily gauges are of great use for the interpolation of sub-daily distributions. The philosophy of only allowing methods for publication, which always perform best, may lead to cherry picking of the results and prevent an open discussion in science. Regarding the estimation of rainfall extremes, non-parametric kernel density estimations may exhibit problems. However, using a Gaussian kernel also allows for extrapolation beyond the range of the historical data, which still needs to be evaluated. The study mentioned from the reviewer (Langousis et al., 2016a) investigates daily rainfall extremes, but not, how it is for different temporal resolutions? Also more investigations are required to answer this question. In addition, depending on the application, rainfall extremes *do not always have such a decisive character. An example is real-time control of sewer systems, where average and larger values are more important, as rainfall extremes cannot be controlled by the system anyway.*

Reviewer 2:

I agree with some of the arguments stated in this paragraph. However, my official suggestion was not "rejection of the manuscript". I suggested "major revisions". The reason for this is that although I had major concerns about the level of the manuscript, in terms of innovation and presentation efficiency (see my nine comments in the first round of revisions), I recognized and also indicated the potential importance of authors' results. With that being said, authors' statement "*We disagree with the first part of this comment, where the reviewer proposes not to publish this manuscript…*", and more importantly their whole discussion on my philosophy (towards what is worth publishing and what is not) are not based on my actual and official suggestion. The authors should not so easily jump into conclusions and judge a reviewer's judgment or philosophy based on their assumptions about reviewer's opinion, and not based on his/her actual and official suggestion.

The authors also state: *Even if the method performed worse over all temporal resolutions, we would consider it as important to publish the method.*
This is only authors' opinion. A reviewer needs to point out all possible shortcomings of a proposed method. The final decision will be made by the handling Editor.

Concerning the technical part, in their response the authors state: *The study mentioned from the reviewer (Langousis et al., 2016a) investigates daily rainfall extremes, but not, how it is for different temporal resolutions?*
The study I mentioned refers to daily rainfall but not only to rainfall extremes. Also, note that both references provided by the authors themselves (see their conclusions) refer to daily rainfall (not to finer temporal scales), and consider the use of mixed Pareto-type distributions (see also my next comment).

<u>Comment 2</u>

Reviewer 2:

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).

Authors:

Answer: In the references mentioned by the reviewer, the focus lies on extremes of daily rainfall, whereas in our investigations we only exclude very small rainfall values for each aggregation due to measurement errors and minor importance (see Table 1 in the manuscript). Additionally the focus of the manuscript lies on regionalization, which can influence the performance of a theoretical distribution and was to our knowledge not yet investigated for the whole range of daily rainfall values using Pareto type distributions. However, Pareto type distributions are very interesting and their regionalization performance could be looked at in a different paper.

Reviewer 2:

In a recent study (see doi: 10.1002/2016WR019578) a parametric approach for simultaneous bias correction and regionalization of climate model rainfall is proposed based on the use of GPD above a certain threshold (mixed type). It is proved that it outperforms the nonparametric alternative.

In any case though, since the authors themselves think that Pareto type distributions are very interesting and their regionalization performance should be looked, I do not see the reason that they are unwilling to add a Pareto type distribution in their analysis. Their current investigation may be regarded incomplete.

General comment:

In their responses, the authors either commented on my philosophy (based on what they think I think of their work, and not on my actual suggestion), or they only stated that Pareto-type distributions are potentially interesting. Yet, they are unwilling to include a Pareto-type distribution in their analysis. I consider both my comments unaddressed. I acknowledge that most of my other comments are addressed, thus, I change my suggestion from “major revisions” to “moderate revisions”. A second round of revisions is needed.