

Interactive comment on “An objective cross-validation framework for mapping rainfall hazard based on rain gauge data” by Juliette Blanchet et al.

Anonymous Referee #2

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The paper describes a method for mapping distribution parameters from single rain gauge record across a domain, but lacks a proper discussion of the role/significance of the proposed framework in the landscape of rainfall hazard mapping: in that respect, if the proposed framework is really a (significant) step forward then the authors should demonstrate it by comparing it to the state-of-the-art in rainfall hazard mapping, which they as such also describe in the introduction (page 2, line 4-14). I am left with the impression of reading a technical report rather than a paper significantly advancing the field (which does not mean there is no advancement per se, but it is difficult to judge at this stage).

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I do not understand why classic interpolation of rainfall of a certain frequency comes along with issues of zero values (even more obvious when mapping amounts and their exceedance probabilities), see page 2, line 10-11. A certain exceedance probability of rainfall is >0 by definition, and we are talking about regional hazard maps and not about scenarios (i.e. rainstorms). Or am I getting something wrong? Also, provided the issue exists, the authors address a solution themselves (which is the analytical transformation), which brings me back to the issue of ideally comparing the proposed framework to the state-of-the-art.

I also do not understand why one would feed a distributed rainfall-runoff-model with a rainfall-frequency map (page 1, line 17-18). The resulting rainfall-runoff-model output is highly artificial, not much telling about a realistic hydrological scenario. In hydrological hazard/risk assessment, one would probably conduct scenario-based analyses based on potential (realistic) rainstorms or continuously simulate rainfall time series to feed an RR-model, to get insights about (extreme) runoff events. But maybe I did not understand what the authors intend to say here; citations of related work would maybe clarify.

Modelling only two seasons is a clear limitation, and so is the assumption of stationarity. The same applies to the three weather patterns, which is another constrain. I am also in doubt that the Gamma (or the mixture of a Gamma) is suitable in other regions, especially in the tail. That is, it remains open whether the framework is really applicable to other regions. The authors put that into question themselves (e.g. page 24, line 30, among others). So besides comparing the proposed method to the state-of-the-art, a second study area (other climate, more seasons) would be – in my eyes - very important.

What is also missing is a proper discussion of the uncertainties of the rainfall records in mountain regions. It would be important to consider these observation errors in the framework, again for the proposed model and the state-of-the-art in comparison, to really understand all implications.

A figure describing the entire framework would be important, it is difficult to follow all steps and practitioners will for sure appreciate a presentation that is a tad more “hands-on”.

I would recommend a final proofread by a native speaker, there is quite a number of minor language related mistakes throughout the manuscript, not a big deal but just a few examples:

“One of the difficulty”; “models for nonzeros rainfall”; “Similar idea is used”; “in the same time”; “independently on each others”

Sharing the original observation data (other journals even demand it) to allow for reproducing the results is recommended.

In summary, at this stage, I am in doubt that the reader really understands the added value of the method, why and when established rainfall hazard mapping methods are competitive and if the method is applicable to another region/climate.

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