# Interactive comment on "Temporal rainfall disaggregation using a micro-canonical cascade model: Possibilities to improve the autocorrelation" *by* Hannes Müller-Thomy

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## 1   Summary

The manuscript deals with micro-canonical Multiplicative Random Cascade (MRC) application for rainfall disaggregation (starting from the daily scale) at sub-hourly scales. Specifically, the Author investigates the effectiveness of some modifications to MRC model proposed by Müller and Haberlandt (2018) in correctly reproducing the autocorrelation function of observed rainfall sub-hourly time series and the occurrence of small rainfall values. This topic is of interest for many hydrological applications; however, the

manuscript presents some weaknesses that should be addressed before the paper can be considered for publication in HESS. My comments are reported in the following; I hope they will be helpful for manuscript improvement.

With appreciation,

Elena Volpi

## 2   General comment

The first concern is related to the motivation of this work. In the Abstract and Introduction Section two main issues that limit the applicability of "classical" MRC models for rainfall disaggregation are mentioned, namely the underestimation of the autocorrelation function of the fine scale process and the presence in the disaggregated series of very small rainfall values, that are not observed in measured data. The first problem does not seem to be of general interest, i.e. it does not characterize all the MRC models, but only the reference model here (that proposed by Müller and Haberlandt, 2018); the second problem is related to the properties of the observed data, which are characterized by a finite resolution so that values smaller than this resolution cannot be recorded. This second problem seems to affect significantly the estimation of the autocorrelation function, as also demonstrated by the results of this work, so that the simulated autocorrelation function does not correspond to the observed one. Hence, the second issue seems to be the fundamental one to improve the effectiveness of the reference model in reproducing the characteristics of the observed data.

Further, only at the end of Introduction Section another fundamental problem characterizing MRC model is mentioned, that is the stationarity of the disaggregated process. The Author cites the paper from Lombardo et al. (2012) where the stationarity issue of MRC model is discussed and an alternative model is proposed that is proved to be stationary. The Author drew inspiration from the latter model, yet not to guarantee

stationarity but only to improve the reference model performance in terms of autocorrelation function. This is the main issue here; based on my opinion, stationarity should be addressed before improving the accuracy of the simulated autocorrelation function.

Finally, I personally believe that the manuscript is rather difficult to follow because most of the mathematic behind the models is not explained (see the specific and technical comments that follow). As an example, it is not clear how many parameters rule the model behavior (in its different modified versions) and how these parameters can be estimated starting from the observed data, even if it seems that in this case the Author does not assume an universal generator (hence a simple-/multi-scaling behavior), am I correct?

## 3 Specific and technical comments

Lines 14-17, page 2. The difference between canonical and micro-canonical is that between downscaling and disaggregation, as pointed out in Koutsoyiannis and Langousis (2011).

- Koutsoyiannis, D. and Langousis, A. (2011). Precipitation. In Treatise on water science, Edited by: Wilderer, P. and Uhlenbrook, S. Vol. 2, 27–78. Oxford: Academic Press.

Lines 14-17, page 2. This seems to be a minor problem, which is in general solved by disaggregating at a finer time scale and then aggregating at the desired one. It might have some implications in parameter estimation, depending on the structure of the generator. Is this the case?

Lines 17-19, page 3. Among the problems that the Author cites that justify the modifications of the MRC proposed in the manuscript, there is the non-stationarity issue.

C3

However, this is not mentioned in the abstract, but only at the end of the Introduction Section, and nowhere else in the manuscript; in other words process stationarity is not considered a problem here (see also the general comment).

Lines 19-20, page 3. The description of the work by Lombardo et al. is not clear and, based on my opinion, what emerges does not correspond to the work done in the cited papers. The Author should improve his synthetic explanation; further, note that the method proposed by Lombardo et al. is not based on a MRC, but on an "additive" cascade.

Lines 26-28, page 3. I'm not sure I fully understood the issue of small values that are generated by the random cascade. Are the small values too small with respect to those characterizing observed rainfall time series at the target temporal resolution? If the reference truth is the observed high-resolution time series, is the Author arguing that the reference truth is not correct? This issue, which constitutes one of the most important motivations of this study, should be better explained to the potential readers (see also the general comment).

Lines 13-14, page 5 and 1-2, page 6. The sentence is not clear.

Line 4, page 6. "direction"?

Line 11, page 6. What does it mean that the aim is achieving a "minimum rainfall intensity"?

Line 12, page 6. Resampling as a subsequent step after disaggregation could be define as a post-processing technique/strategy.

Line 24, page 6. "no" instead of "on"?

Eq. (2). Are the possible outcomes for the three "events" at the disaggregation level 2 all mutually exclusive? In such a case, should the sum of the corresponding probabilities be equal to 1? If this is not the case, the disaggregation scheme at level 2 should be better explained.

C4

Line 4, page 7. What does it mean that the (probability?) parameters depend on the volume? How? Even if this is explained in a previous paper, it should be briefly recalled here for the sake of clarity.

Lines 15-20, page 7 and 1-2, page 8. How many parameters characterize this modified version of the model? Which is the rule of dependence for starting, enclosed, etc. elements in the disaggregated series? The explanation is incomplete, or at least too vague.

Section 3.2.1. Method B adds additional probability parameters, letting them vary with the "position". It is unclear how these multiplicative parameters are determined to re-produce the statistical characteristics of the process across scales based on the structure of the cascade (i.e. b=3 for level one and b=2 for all the remaining levels up to the desired temporal resolution). See also previous comment.

Figure 2 could be improved to help for reader understanding the difference between method B and C (but also starting, ending etc. elements of the cascade). Further, it should be explicitly mention if blue and white denote wet and dry states.

Line 11, page 9. "isolated" instead of the second "ending"?

There is a substantial difference between the approach proposed here and that used in Lombardo et al. (2012, 2017). Here the Author introduces a "conditional" probability that determines the probability of a wet or dry state, while in Lombardo et al. the information of previously disaggregated elements at the same time-resolution is used to feed a linear interpolation model estimating the disaggregated value of the subsequent element (see eq. (7) in Lombardo et al., 2017).

Line 7 page 10. The underestimation of the autocorrelation function characterizing the starting model is not a characteristic of all multiplicative random cascade models, as stated by the Authors in the literature review. Is there a motivation for this? Is it possible to generalize the problem to a specific MRC model (i.e. generator) structure?

C5

Line 17, page 14. If the reference model works well in terms of autocorrelation, that is the main issue here, why two different modifications are proposed?

I do understand that the dataset is not so rich, but I expected to find two separated datasets, one for "calibration" and one for "validation" of the models. The Author should justify this choice.