Review by Elena Volpi, Referee #2

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

I thank Elena Volpi for her useful and constructive comments and all her time, she spend on the manuscript. A point-by-point reply can be found below. All page and line numbers refer to the original submission.

2 General comment

1. 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.
From the literature review it is obvious that the reproduction of the autocorrelation function is a general problem of the micro-canonical cascade models, whereby under- and overestimations can occur. The introduced position definitions and the resampling approach were proven to solve the underestimation and it is assumed, that an overestimation would be reduced by both approaches as well. Both approaches are transferable to other cascade models and hence represent general improvements/solutions, which are not restricted to the micro-canonical cascade model applied in this study.
Of course, I agree with the reviewer that the generation of too small rainfall intensities is an important issue, especially since all rainfall characteristic comparisons are biased if the time steps with hydrologic irrelevant rainfall intensities are taken into account. Solving this second issue provides the basement for solving the first issue.

2. 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 stationarity. 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.
Indeed, the problem of stationarity is not solved in this manuscript, but this was also not the intention. The inspiration from Lombardo et al. (2012) for the current study refers "only" to the determination of the most important time steps to generate highly correlated time series. However, the analysed position definitions as well as the resampling approach can be transferred to other cascade models, so also later for stationary models.

3. 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?
The reviewer points out missing information about the method and its implication. A new subsection was added (Section 3.1.4, including a model parameter comparison) as well as a paragraph for the parameter estimation. All detailed comments from the reviewer have been addressed and can be found below. The rainfall generator is described by Eq. 2 (Eq. 4 for method B and C) for b=3 and Eq. 3. for b=2.

3 Specific and technical comments

4. 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)
The additional information was implemented in the manuscript:
„Based on Koutsoyiannis and Langousis (2011), the canonical version of the cascade model (conservation of rainfall amount on average during the disaggregation, e.g. Molnar and Burlando, 2005, Paschalis et al., 2012) represents a downscaling technique, while the micro-canonical version (exact rainfall amount conservation for each time step, e.g. Olsson, 1998, Güntner et al., 2001, Licznar et al., 2011, 2015) represents a disaggregation technique."

5. 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?
I assume the comment refers to P2L17-22. The implication of putting a1440 min rainfall amount (1 d) into 1280 min and start the disaggregation is that you will end up with a day which is 160 min (~2.5 h) too short. So either you assume that the missing time steps have a rainfall amount of 0 mm (the question is then: Where to put them?) or you apply any transformation to the daily time series before the disaggregation. Both approaches will affect the resulting rainfall time series characteristics. The following sentence was added:
„Of course, by the application of an additional transformation process a desired temporal resolution can be achieved, whereby the transformation process affects the characteristics of the disaggregated time series."

6. 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. 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).
Of course, non-stationarity is a problem, but it is not the motivation for the analysis carried out in this study. Indeed, mentioning the problem in the introduction was misleading and this sentence has been removed (see also the reply to your comment 20).

7. 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.
The reviewer is right, the disaggregation models differ. The related parts of the text have been rephrased, so that only the information of Lombardo et al. is briefly cited, which are the most worth time steps to consider for the generation of highly correlated time series under the burden of computational efforts. A new paragraph has been added to the section "5.3 Study limitation" for clarification:
"Fourth, although method C is based on a finding in Lombardo et al. (2012, 2017), the disaggregation method differs from the additive cascade model in Lombardo et al. (2012, 2017). Hence, the by Lombardo et al. identified problem of non-stationarity of the disaggregation is not solved by the introduced cascade model variants and remains an open challenge for further studies."
Please see also the reply to your comments 6 and 20.

8. 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).
I thank the reviewer for pointing out this vague lines. This part of the introduction has been rephrased to:
"Koutsoyiannis et al. (2003) argue that it is unclear, if the values generated by the cascade model are too small in comparison to the observed minimum rainfall intensities or if the resolution of the measurement device is not fine enough to observe the very small rainfall intensities generated by the cascade model. From a practical point of view, these low-intensity time steps are not important, but they have an impact on the autocorrelation function. To enable comparisons between the autocorrelation of observed and disaggregated rainfall time series two methods are analysed in this study which ensure a minimum rainfall intensity in the disaggregated time series."

9. Lines 13-14, page 5 and 1-2, page 6. The sentence is not clear.
The reviewer is right, the sentences were more misleading than helpful. Both sentences were removed from the manuscript.

10. Line 4, page 6. "direction"?
The sentence has been reduced to:
"The autocorrelation function is based on two elements: the covariance $s_{t1,t2}$ of the original and the shifted time series (t_1 and t_2), that describes the ~~direction of the~~ relation of both time series, and the standard deviations of both time series, s_{t1} and s_{t2}, for the standardization of the covariance."

11. Line 11, page 6. What does it mean that the aim is achieving a "minimum rainfall intensity"?
The text has been rephrased to:
"…to achieve the same minimum rainfall intensity as in the observed time series, to enable comparisons between the autocorrelation in observed and disaggregated rainfall time series."

12. Line 12, page 6. Resampling as a subsequent step after disaggregation could be define as a post-processing technique/strategy.
The term "subsequent step" was replaced by "post-processing strategy" throughout the manuscript.

3. Line 24, page 6. "no" instead of "on"?
I thank the reviewer for this error spot, it was corrected.

14. 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.
Indeed, in Eq. (2) the occurrence of rainfall amounts in the three 8 h time steps is independent from each other. In a first step, based on the probabilities P(0/0/1), P(0/½/½) and P(1/3 / 1/3 / 1/3) (which sum up to ∑P=1), the number of wet 8 h time steps is identified. The choice of the wet intervals among all three possible intervals.

15. 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.
The following explanation has been added:
"Müller and Haberlandt (2015) have shown that for days with high rainfall amounts (above a quantile $q_{0.998}$) the probability for two and especially three wet 8 h time steps is much higher than for lower daily rainfall amounts. Without a consideration of this volume-dependency of the parameters, the probability is too high that high daily rainfall amounts are put into one 8 h time step, which will lead to an overestimation of extreme rainfall values."

16. 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.
Regarding the parameter number, Table 3 has been added to the manuscript. The explanation has been extended by the following sentences to describe the parameter estimation procedure more concise:
"To summarize the previous explanation regarding parameter estimation: for each temporal scale two fine time steps are aggregated (or three finer time steps for *b*=3, respectively) to one coarser time step, whereby the position and the volume class of the coarser time step determines to which position-volume class-combination the current splitting belongs. The cascade model parameters are then estimated over all splittings of a position-volume class combination."

17. 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 reproduce 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.
I assume that this comment refers to section 3.1.2. Additional to the added explanation (see my reply to your previous comment) a summarizing figure (Fig. 3) has been added, including all position definitions applied in this study.

18. 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.
We thank the reviewer for her suggestion. We added the temporal scales to Fig. 2. Since a new figure (Fig. 3) has been added to the manuscript, which illustrates in detail the different possible position classes, the colours have been removed from Fig. 2.

19. Line 11, page 9. "isolated" instead of the second "ending"?
The reviewer is right, "ending" was replaced by "isolated".

20. 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).
The reviewer is right, only the identification of the time steps „most worth to consider“ has been overtaken from Lombardo et al. (2017), while the disaggregation method itself differs. The related part of the manuscript (P3L17-20) has been rephrased to:
„The second, more complex modification follows an idea of Lombardo et al. (2012, 2017). Lombardo et al. analysed which time steps are most worth to consider to generate highly correlated time series under the burden of computational efforts. Their conclusion is adapted in this investigation.“

21. 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?
I'm not sure if the text index refers to the comment (this reply is to the comment itself).
First, the deviation of the autocorrelation in the generated time series from the observed time series refers to micro-canonical cascade models, not cascade models in general. This information has been added where possible in the introduction.
Second, under- and overestimations are reported by the references in the introduction. But indeed, the previous results from the basic model for this study leads to underestimations of the autocorrelation.
However, it is assumed that with the new position definitions and the resampling approach under- and overestimations of the autocorrelation function can be avoided and improved subsequently, respectively.

22. 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?
In the referred sentence only the results for lag-1 autocorrelation are discussed, how it is done very often for rainfall characteristics. However, as shown later in e.g. Fig. 7 and related paragraphs, the analysis of lag-1 is not enough, the whole autocorrelation function has to be analysed, or at least more lags as done in this study for lag-6 and lag-36.

23. 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.
The maximum time series length is 20 years. For the parameter-intensive method C an artificial shortening of the time series should be avoided. Also, for later "real-life" applications, the whole high-resolution time series is used for parameter estimation for the disaggregation of the daily time series. Hence, a shortening of the time series could lead to a worsening of the disaggregation results, which would not occur otherwise. The following explanation was added in the validation section 3.1.4:
„A split-sampling into calibration and validation period was not carried out to keep the time series as long as possible for the parameter estimation (see also the discussion in Section 3.1.4).“