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Interactive comment on "A new discrete multiplicative random cascade model for downscaling intermittent rainfall fields" *by* Marc Schleiss

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1 Summary

The manuscript proposes a new approach for rainfall disaggregation, based on the framework of Multiplicative Random Cascade (MRC) yet able to overcome the its limitations, namely possible over-parametrization or limited accuracy in presence of intermittency (more specifically, for a large amount of zero values). The Author investigates the effectiveness of a disaggregation scheme based on the notions of scaling and Equal Volume Areas (EVA), with maximum three parameters that can be inferred

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Discussion paper

directly from the coarse-scale data. Investigation is performed for 2D (spatial) rainfall fields. The proposed approach allows for intermittency simulation without explicitly coding zero values and their distribution and structure; on the other hand, the simulated rainfall field needs to be re-sampled in grid cells from variable to constant size (i.e., re-gridding).

2 General comment

The manuscript is well written and organized, and the topic is surely of interest for the hydrologic community. Hence, I suggest to consider the manuscript for publication in HESS. Before this, more details are necessary to allow for reader understanding and repeatability of the work (that is also the application of the framework in different contests). The most important problem is that the theoretical properties of the simulated process, that are the marginal probability distribution function and the joint distribution, are not explicitly presented and discussed; these are fundamental to understand e.g. if the model is based on the same scaling property ruling the theoretical behavior of the traditional MRC. I have some additional concerns that are listed below. I hope they will be helpful for manuscript improvement.

3 Specific and technical comments

- I'm curious about the reasons why 2D simulation is discussed instead of "starting" from the simpler 1D case, i.e. for temporal disaggregation, which is important for several application problems. The Author should at least discuss the applicability to the framework to temporal disaggregation.
- It should be important also to mention one fundamental problem characterizing

MRC model, i.e. the stationarity of the disaggregated process. The Author mention the paper from Lombardo et al. (2017); yet in a previous paper (Lombardo et al., 2012) the authors demonstrated why MRC does not generate stationary processes. The Author should discuss the stationarity issue for the model proposed here and generally mention the possible limitation of the framework for temporal and spatial disaggregation.

- A figure could help to better explain the splitting rule.
- Line 17, page 5. How can we reproduce the stochastic properties of the process of alternation of wet and dry cells? If we do not explicitly model this process, how does the structure of this process depend on the model formulation?
- Lines 12-17, page 7. Is it possible to gain the same or similar advantage by using a different generator?
- Section 2.5. Since it is not clear which are the theoretical properties of the simulated random field (see general comment), it is difficult to follow this section.
- Lines 21-22. Is it possible to quantify this additional uncertainty?
- Lines 29-30. Based on this, it seems that the theoretical behavior of the simulated process is determined based on empirical reasons. Is this correct?
- Line 10, page 9. Smoother than the observed one, being based on interpolation ... Thus, can this method be considered a disaggregation model? Instead of linear interpolation the Author could have considered kriging, which preserves the variogram.
- Lines 13-18, page 9. This configuration of the MRC is indeed not very common in the literature; how this configuration affects the reliability of the disaggregation model? Can we expect better results (as depicted in figures 7, 8 and 11) by using

the most common approach in the literature or one based on a larger number of parameters? Models can be compared also in case of a different complexity; furthermore, the parameters of disaggregation models could also be estimated by considering different additional properties of the generated process (e.g. the expected value of the number of wet cells in the spatial case or the dry spell average length in the case of time disaggregation).

- The calibration procedure is not totally clear to me; additional efforts are required to explain it in theory and practice.
- I'm not sure figure 6 is really useful. Are the numerical differences between EVA and traditional approach W values (as depicted in figure 6) really significant from a practical point of view?
- Given the large amount of observed rainfall fields, it could be of interest to understand how estimated model parameters depend on large scale event characteristics, so that a general parametrization valid for all the events can be found. This means calibrating the model based on observed small scale observations when available to apply the model to other events.
- Is it possible to compare simulated and observed fields in terms of the spatial structure of variability (spatial correlation) of the wet/dry alternation process? See also previous comment on this.
- The Author uses R^2 as a metric for model performance with respect to observation; why not using a different metric, not based on the normality assumption?

With appreciation, Elena Volpi

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