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Hydrology and Earth System Sciences Discussions

Interactive comment on "Circulation pattern based parameterization of a multiplicative random cascade for disaggregation of daily rainfall under nonstationary climatic conditions" *by* D. Lisniak et al.

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In this study, the Authors propose a parameterization approach for multiplicative random cascade models (MRCs) to account for circulation pattern (CP) class, large scale (daily) rainfall depth, position of the rainfall pulse along the time series and time scale. The suggested disaggregation procedure also implies a varying branching number across the disaggregation stages (from 24h to 1h). The paper is clear and well written; however, in my opinion, some technical points must be carefully considered and

checked.

Specific comments

The proposed approach is pragmatic and the Authors recognize that the modified discrete MRCs could not be considered as multifractal at all. Even though this approach could be criticized by some experts who pay more attention on theoretical issues (see e.g. the comments provided by Dr. Lovejoy on a paper suggesting a multinomial branching approach http://www.hydrol-earth-syst-sci-discuss.net/7/5267/2010/hessd-7-5267-2010-discussion.html), the MRC algorithms are widely used. Nevertheless, Lombardo et al. (2012) proved that the discrete branching algorithms are characterized by intrinsic nonstationarity. This aspect probably affects the simulated rainfall sequences in a way which could be not so evident in the rainfall summary statistics; however, the problem must be mentioned and/or taken into account by using for instance the algorithms suggested by Lombardo et al. (2012). It is worth noting that the spectral-based algorithms devised for the universal multifractal models are not affected by the above mentioned problem.

The element of novelty of the proposed model is the dependence between the circulation pattern class (CP) and the model parameters. In principle, every MRC model can be split in different submodels by stratifying the parameters in an arbitrary number of classes accounting for forcing covariates. However, further sources of complexity should be introduced only if there is a clear improvement. In my opinion, at the present stage, the empirical results shown in the manuscript do not clearly justify the use of CPs. Figures 5, 6, and 7 show that the models with and without covariates are statistically indistinguishable (the CDF confidence intervals overlap), whereas it is difficult to draw definite conclusions based on the ACFs without reporting the corresponding confidence intervals. Moreover, it should be mentioned that the proposed models implicitly assume that the relationships between covariates (e.g., CPs) and parameters estimated in the calibration period are deemed to be valid also in the validation period.

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Since the Authors explicitly developed the model to account for possible nonstationarity, the assumption that the model structure does not change can be a bit contradictory even though it is a common assumption in the literature.

The Authors state that the similar performance of the MRCs models in the validation period is a surprising result; however, the bad performance of the benchmark rainfall generator is perhaps more surprising. In my understanding, the rainfall generator samples 24h blocks of hourly rainfall patterns (block bootstrap) and then applies them to the daily rainfall totals available in the validation period. I suppose that the hourly 24h rainfall profiles are rescaled in order to preserve the daily rainfall depth. If it is so, the rainfall properties at daily scale should be exactly preserved because the rainfall generator is implicitly microcanonical. This means that both MRCs models and the rainfall generator should reproduce exactly the moments at the daily scale. Therefore, the bias of the moments at 24h time scale exhibited by the rainfall generator (bottom panel of Fig. 8) and the systematic bias of the CDF of 1h rainfall (Fig. 5) seem not to be consistent. It is worth noting that the constant bias (shift) of the survival functions in a logarithm scale should correspond to a multiplicative error. Therefore, I guess that the rainfall generator was applied for the validation period without adjusting correctly for the daily mass preservation. Please, double check the correctness of the results.

The Authors discuss the difference between the scaling properties of the rainfall for the periods 1969-1979 and 1989-1999 on the basis of the moment-scale power-law relationships shown in Fig. 3. The overlap of different moments makes the comparison rather difficult. However, a careful inspection reveals that the difference of the slopes could be not so evident. In particular, given the small sample size (5 points), the difference could be not statistically significant. I suggest (1) to redraw the figure to make it clearer (avoiding any overlap), (2) to test the difference of the slopes and (3) to complement the figure with the diagrams of the exponent of the moment scaling relationships K(q) taking the uncertainty into account. A discussion about the uncertainty of K(q) is provided by Villarini et al. (2007). In order to support the statements concerning the

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difference between the scaling properties of the two periods, these aspects must be carefully checked.

Minor remarks

P10118L20: Talking about intermittency, the Authors may be interested to the work of Mascaro et al. (2012)

P10126L1: “he” should be “the”

P10128L5-10: The Authors discuss the nonstationarity of the climate. Even though this not the place to discuss about climate stationarity or nonstationarity, it is worth noting that the fractal and multifractal models are well devised to describe the (wide) fluctuations of processes characterized by short and long range dependence. Therefore, the good performance of simple MRC models (without CPs) in the validation period perhaps indicates that the hypothesized nonstationarity can be explained in terms of inherent fluctuations of a stationary process with short and/or long range dependence rather than in terms of nonstationary processes. Moreover, in my understanding, some of the references cited by the Authors deal with trend detection procedures that do not explicitly account for temporal and spatial dependence as well as the multiple testing problem. Perhaps, the model results can give the opportunity to carefully reanalyse the rainfall data of the studied area in a future work.

P10128L1-10: Please, number and order the figures according to their first citation in the text.

References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 10115, 2012.