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Discussions

Interactive comment on "Precipitation variability within an urban monitoring network in terms of microcanonical cascade generators" *by* P. Licznar et al.

Anonymous Referee #2

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The paper under review studies a rainfall disaggregation technique, known as microcanonical multiplicative random cascade, in which a rainfall amount R at a time interval Δt is partitioned into two subintervals of equal size $\Delta t/2$. This is accomplished through multiplying the rainfall amount R by a random variable W bounded in [0, 1] thus producing the rainfall depth of the first subinterval (that of the second is then the remainder). This procedure is applied progressively starting from quasi-daily time intervals (1280 min) until the time resolution becomes of 5 min.

While many researchers have earlier used this technique (e.g. the first Author himself already published two other papers similar to the paper under review, see i.e. Licznar et

al., 2011a, b), the contribution of this paper should be understanding the rain variability at small timescales in an urban area by means of microcanonical cascade generators. Then, the Authors propose and analyze a local precipitation model at high temporal resolution which should be able to capture the rain variability at small timescales in an urban area. The method is applied to rainfall records from an urban network of 25 gauges in Warsaw, Poland (note that the same dataset has been already used by Rupp et al. (2012) to calibrate another multiplicative random cascade model devised for the spatial downscaling of rainfall).

Despite being in general well written and focusing on an interesting research topic, it is my opinion that this paper does not make a valuable contribution to hydrological sciences. Therefore, I regret to say that I do not recommend its publication in HESS. In the following, I briefly list below my major concerns.

1) I can hardly find the novelty of this paper with respect to Licznar et al. (2011a, b) and Rupp et al. (2012). I mean that the vast majority of the concepts presented in the paper under review seems to have been already discussed elsewhere.

2) The Authors state (p. 5253, lines 14-18): "(...) when some local precipitation datasets are accessible, questions and doubts about the representativeness and reliability of data arise. Synthetic time series, generated from precipitation models, could be considered as probable precipitation scenarios to feed hydrodynamic urban drainage system models". Actually, this is a general comment, but it is very important in my view. I believe that the dangers for science become most evident when models (abstracts of more complex real-world problems, generally rendered in mathematical terms) are assumed to be more reliable than observational data (even if uncertain). This is particularly the case for statistical models like the model proposed in the paper under review.

3) Please, note that there is ongoing discussion about the inappropriateness of multiplicative random cascade models in providing credible simulations of rainfall time series. For example, Lombardo et al. (2012) show that the autocorrelation function of the

simulated series corresponds to a non-stationary process simply inherent to the model structure (see also Mandelbrot, 1974; Over, 1995; Veneziano and Langousis, 2010). The Authors should investigate whether or not their model is affected by this problem, because the reproduction of the autocorrelations as well as marginal probabilities are major requirements for statistical models.

4) The Authors are also encouraged to study the statistical implications of the so-called "overlapping moving window algorithm" for the calculation of the breakdown coefficients (see paper eq. (2)). In other words, the Authors should investigate the joint distributional properties of their simulations when using the classical non-overlapping and their overlapping methods. I guess the dependence of the generated rainfall at a certain time interval with the time intervals preceding and following it may change significantly for the two methods.

5) It is well acknowledged that parsimony is a very important and desirable property in good modelling practice. However, the model proposed is over-parameterized and thus not parsimonious because it uses a somewhat artificial probability distribution for the breakdown coefficients (i.e. 2N-B distribution, which combines two Normal (N) and one Beta (B) distribution). Then, being a complicated discrete-time model, it does not correspond to a continuous time process; but natural processes typically evolve in continuous time.

6) Furthermore, as the Authors have posed the 2N-B distribution as an assumption, rather than deriving it theoretically from other principles. Then, they should apply a goodness-of-fit test to justify their choice of the 2N-B distribution.

7) The reason for fitting a statistical model to data is to make conclusions about some essential characteristics of the population from which the data were drawn. Such conclusions can be sensitive to the accuracy of the fitted model, so it is necessary to check that the model fits well. The main issue concerns the ability of the model to describe variations in the wider population, and this is usually achievable when there

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are additional sources of data against which the model can be judged in terms of the reproduction of the autocorrelations as well as marginal probabilities. By contrast, the Authors seem to judge the accuracy of their model in terms of its agreement with the data that were actually used to calibrate it. This limits the value of their results.

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