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Hydrology and Earth System Sciences Discussions

Interactive comment on "Multiplicative cascade models for fine spatial downscaling of rainfall: parameterization with rain gauge data" *by* D. E. Rupp et al.

D. E. Rupp et al.

david.rupp@oregonstate.edu

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Reply to Referee #1 We would like to thank Referee #1 for his constructive comments and general contributions to the discussion of this topic. We have repeated the original comments, with our response immediately following each comment.

Referee #1 Received and published: 19 September 2011

This study introduces a new method to estimate the parameters of a multiplicative random cascade (MRC) model (devised for the spatial disaggregation of rainfall fields) when the rainfall observations are collected by a sparse network of gauges. In this context, the approach based on the moment-scale diagrams is deemed to be no suitable, as the gauge density is low relative to the desirable grid cell density. Therefore, the Authors exploit the properties of the -stable distributions to derive a set of analytical expressions that allows estimating the model parameters, resorting to the ratio between the rainfall values at the two limit scales of the cascade without using the intermediate scales. The proposed method is applied to calibrate eight versions of a beta-logstable model, in which the scale parameter of the distribution of the weight generator is assumed to be constant or varying with the scale and/or the large scale rainfall, whereas the intermittency parameter depends on the large scale rainfall. These models and the estimation method are tested on a rainfall data set recorded by 25 gauges located around Warsaw, Poland (the data are aggregated at 15-min temporal resolution). The performance is assessed by comparing the cumulative distribution functions, and the semi-variograms of the observed and simulated rainfall values. The approach seems to be very interesting, hypotheses and possible weaknesses are clearly stated, and, in general, the paper is clear and well organized. In my opinion, the paper is suitable for publication in HESS after some minor changes. Some technical comments are provided in the next sections. Please, note that some of my remarks should be considered as a contribution to the open discussion rather than requests of changes.

COMMENT: Page 7276 (10-11): The Authors state "We believe this is an artifact of the discrete nature of the cascade procedure that was applied". It could be interesting to try the continuous in scale universal multifractal (UM) model, in which the parameter α can be obtained by using the method discussed in the study under review, H can be set up equal to 0 (or a suitable value taken from the literature), and C1 may be seen as a tuning parameter. The model can be more parsimonious than a scale dependent approach and allows avoiding the simulation of the rainfall values at intermediate scales, when they are not required.

REPLY: The referee makes an intriguing suggestion. The UM model (Shertzer and Lovejoy, 1987; Lovejoy and Schertzer, 2010a;b) would address the issues of blockiness

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of the simulated rainfall. Whether it generates essentially equally good or improved simulations with fewer parameters is worth exploring (specifically the use of a threshold cutoff to generate zero rainfall comes to mind as an issue). The referee has shown it can in the context of temporal rainfall (Serinaldi, 2010). We would be pleased to work out the details of his suggestion (the parameter estimation approach he proposes above) with his cooperation if he were inclined to work with us. For the current paper under review, however, we hope that providing it as a suggestion for further research will satisfy the referee.

COMMENT: Pages 7278 (15-20): As mentioned by the Authors, the model is based on the assumption of stationarity; however, the seasonality is a type of cyclic non-stationarity. For instance, convective or stratiform events can characterize some seasons, resulting in different degrees of spatial intermittency (rain/no rain) and spatial dependence. Merging together all data can influence to some extent the calibration/simulation, and the resulting covariograms. Thus, it may be worth describing briefly the seasonality of the Warsaw climate.

REPLY: We will discuss the seasonality of the Warsaw climate and its possible implications in the revised manuscript. It would be interesting to analyze the rainfall record separated by storm type, e.g., frontal from convective (or hybrid?). As a first approximation, we could simply separate the record by season. However, summer events are a mix of types. For example, an analysis of precipitation and circulation patterns at Kraków Poland, some 268 km south of Warsaw (though within the same river basin) showed that about 34% of daily precipitation events were associated with thunderstorms during the summer, whereas in the winter only 1% of daily precipitation events were associated with thunderstorms (Twardosz et al., 2011). While this difference is substantial, 66% of precipitation events during the summer were not thunderstorm-related. Moreover, daily precipitation events in the summer are nearly evenly divided between frontal precipitation and non-frontal precipitation, while in winter frontal rainfall is twice as frequent as non-frontal rainfall (in the case of snowfall, non-frontal snowfall

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is about 50% more frequent than frontal snowfall). The implication is that breaking up the data set into distinct seasons is not sufficient to differentiate the data by process, and a detailed evaluation of the Warsaw record would have to be done. This latter activity would be a new research project in itself: coding of precipitation types requires detailed information about weather phenomenon, which requires round-the-clock visual observation. We currently do not have such information for our existing dataset, as it was beyond the interest of the owner of the precipitation gauge network.

COMMENT: In the context of time series disaggregation, Lombardo et al. (2011; http://itia.ntua.gr/en/docinfo/1132/) have recently proved that the set of weights provided by the dyadic generation process is not stationary, as the autocorrelation of the weight series depends on the position of the simulated weights along the time series. I think that this result can be also applied to the spatial case, and can affect to some extent the spatial correlation. On the other hand, the simulation procedure of the UM model does not suffer this problem, as it is based on transformations in the spectral domain.

REPLY: We see this as another reason it would be worthwhile to compare the discrete model to the UM in a follow-up study.

COMMENT: It could be interesting to show how the simulations reproduce the properties embedded in the model structure by introducing, for instance, the counterpart of Figures 3 and 4 for the simulated data.

REPLY: We agree that this would be interesting, and would help illustrate the extent of bias in the parameters estimation. However, given we have simulated data from 8 distinct models, we will need to be judicious in how we present the results (i.e. repeating Figures 3 and 4 for all eight models, for a total of 16 new figures, would be excessive). We will present the model parameters as estimated from the simulated data in the revised manuscript. We expect that model parameters derived from observations and simulations will not be identical, with results similar to Veneziano et al. (2006).

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Technical corrections Page 7262 (11): The number...needs. Corrected.

Page 7262 (19): probability density function. Changed.

Page 7264 (9): The number...needs. Corrected.

Page 7264 (12): spatial variability. Corrected.

Page 7265 (9): more damped. Changed.

Page 7265 (15): The model is introduced in terms of rainfall per unit area ("rainfall rate"). However, throughout the paper the Authors use the terms "rainfall amount" and "rainfall depth". Perhaps, referring to R0 as "rainfall rate" may be more correct, as Eq. (1) yields rainfall per unit area. We will clearly define "rainfall rate" and be consistent with its usage in the revised manuscript.

Page 7265 (18): is given by Corrected.

Page 7265 (21): Fig. 2. Corrected.

Page 7265 (24): Perhaps, "various types" can be better. Changed.

Page 7266 (12-13): $0 < \alpha \leq 2$, $-1 \leq \beta \leq 1$ This was correct in the original manuscript, but got changed in the proofs, even after I provided the correction again.

Page 7270: Perhaps, using SIσ should be more consistent with RIσ and RDσ, as the scale dependence refers to σ. Changed as suggested.

Page 7271 (13): all eight models. Changed

Page 7272 (4): In four of the models. Changed

Page 7274 (22): Eq. (17). Corrected.

Page 7287 (4, caption): αÎŰ = 2 Changed.

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