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Interactive comment on "Climate change and uncertainty in high-resolution rainfall extremes" *by* B. Kianfar et al.

Anonymous Referee #3

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The authors present a model study to estimate future rainfall intensities at the station level across a wide range of temporal scales, from 10 min rainfall to daily, thus representing the scope that is relevant for the planning of future urban drainage and flood protection systems. To simulate rainfall, a two-step stochastic model is used that is a composition of the Neyman-Scott Rectangular Pulse (NSRP) scheme for daily rainfall and the Multiplicative Random Cascade (MRC) scheme to disaggregate to sub-daily (down to 10 minutes). They apply a simple change factor procedure to the NSRP parameters (leaving MRC unmodified), with factors being derived from the ENSEMBLES simulations, and find that future rainfall variability largely overlaps with the present.

Following are a few major comments I would like to make, justifying a major revision for eventual publication in HESS.

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Discussion paper

The authors emphasize that their simulations are done in a 'composite' mode (NSRP+MRC), which they claim to compare favorably to the single models used previously. I have not understood this distinction. I would have so if the composite model is defined by calibrating NSRP and MRC parameters together. But there is no mention of this, nor is there any mentioning of the intricacies of the calibration at all (what procedure? What parameter convergence? etc.)

A major critique is the lack of independent verification of the model. Calibration and validation is done on the full data period from 1981 to 2010, resulting, e.g., in simulated mean values that are trivially qperfect in the validation. This type of verification does not reflect the model's behavior for future applications.

The main climate change analysis rests on cases that fall outside the 10-90% confidence band of natural variability. The authors seem to miss that this occurs 'naturally' in 20% of the cases anyway. So I suggest to either mention that explicitly, or to choose a significance level that is much smaller, such as 1%. See also my annotation in the pdf pertaining to using a multivariate significance test ("field significance").

The procedures by which the change factors alter the NSRP parameters contain a lot of statistical/numerical machinery, whose validity is relayed to the literature. There is a chance that the physically inclined reader will be turned off by this, especially since the verification is so poorly done. The paper generally lacks physical argumentation, which at this point at least is desirable.

A few more major and minor comments have been annotated in the attached pdf.

I would like to add that apart from these comments I fully agree with the other reviewers' opinion, especially that of rev. #1.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-536/hess-2016-536-RC3-supplement.pdf

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