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Comment on hess-2021-381

Anonymous Referee #2

Referee comment on "Forecasting green roof detention performance by temporal downscaling of precipitation time-series projections" by Vincent Pons et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-381-RC2, 2021

This paper investigates the use of downscaling model to forecast green roofs performance in the context of climate change. It uses a downscaling approach based on multiplicative cascades. The topic is interesting and relevant for the community. However I would not recommend to publish this paper in its current state and suggest major revisions. Indeed it requires significant clarifications on the downscaling model. Indeed, its presentation is hard to follow and should be more detailed. It notably seems that different distributions of weights are used according to the cascade step suggesting they are not scale invariant. The calibration process of the numerous parameters (up to 19 !) needs to be explained. Please also clarify that what is called “observed data” for the various figures is actually simulations with observed rainfall. Am I correct ?

Detailed comments :

- l. 40-44 : It should clearly be stated that canonical cascades ensure conservation on average only while micro-canonical ones ensure exact conservation of intensity at each step.

- l. 54 : should MC be MRC ? In general, the use of numerous abbreviations does not really help the reader. I would suggest to limit their use to words really often used in the paper.

- Section 2.2.1 : I think there is a need to be more specific, notably for the reader not specialist. Index i and j should be consistent between equations 1 – 2 and Fig. 1. Please also clarify the range of possible values (if "i" refers to a time step then it belongs to 1… 2^n where n is the cascade step and j * 2^n = total duration ?). Eq. 2 : S is said to measure a proportion while it has only 3 possible values. Please clarify.

- Section 2.2.2 : Please clarify how the fitting of the models was done. Are the probability distribution used the same at all cascade steps (only for P(W=0) if I understand well table 2) ? Was a scaling break identified in the data ? What would be the consequences of such break. l. 101 : "all included 5", may be say all included the use of eq. 5 to help the reader. Please explain how the depth or temperature dependency was included. It would also be needed to clarify in Table 2 to what refer the parameters mentioned.

- l. 144-145 : why limiting to lag-1 ?

- l. 152-153 : how do you define "small", "major" and "extreme" events ?

- Section 3.2 : How to you interpret physically the differences of behaviour in Fig. 3.a ? Is the shape of Fig. 3.b the same for other time scales ? How the fitting of the model was done from this analysis ?

- l. 212-213 and comments on Fig. 4. c and d. Why is the discharge considered to be only slightly underestimated while the observations do not fall in the 5-95% percentile ?

- l. 216-218 and fig 4.e : the discrepancies between models and observations for the lag-1 autocorrelation should be discussed more.

- l. 335-338 : I have trouble to find the figures mentioned in Fig. 5.

- Fig. 5 : last row (extreme events). May be the vertical scale could be split to enable a zoom on the lower part which concentrate most of the information which is not visible now.

- Fig. 6 : "observed data" is not visible in the graphs

- Section 3.5 : without explaining everything, I believe that some details the variational approach are needed for the non-specialist reader. How do authors interpret the fact that the differences between the two approaches are much more pronounced extensive roofs than the detention based ones ?