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Hydrology and Earth System Sciences
Discussions

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

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General statement

The aim of our paper was to provide an example of the use of a stochastic simulation-downscaling approach for generating local (station-based) high-resolution time series of precipitation for climate change impact studies. The approach consists of a nested point process stochastic model for daily and coarser timescales and a multiplicative random cascade routine for disaggregation to subhourly timescales. In the paper we provide an application of how such a nested approach can be used for climate change impact assessment concerning extremes. More specifically we stress that stochastic uncertainty (internal climate variability) results in future climate scenario signals in rainfall extremes often being contained within the natural variability of present-day climate.

The analyses are conducted for 22 meteorological stations in Switzerland with high resolution data, and are consistent between stations. The main message of our paper is that stochastic uncertainty should be included in climate change impact studies involving precipitation change, especially when extremes at a particular location (raingauge) are the target. Models like the one we present may be used for this purpose. We acknowledge that there are other similar approaches in the literature, which we cite to the best of our knowledge, and we also acknowledge that our approach has some limitations, which in the revision of the paper we will highlight more prominently.

In revising the paper we will directly address all the individual comments of the referee and provide an itemized response letter. Here we provide an overview opinion only on what we believe were the key issues raised and how we will deal with them.

1. Performance of the nested model

The referee asks what is benefit of the composite model, if it appears that the external and internal models have been calibrated separately (yes they have). The added value comes from the application, i.e. disaggregation models like MRC are most widely used for the disaggregation of rainfall down to 1-10 minute temporal scales, with reasonable success considering their few parameters. The parameter estimation consists entirely of estimating the cascade weight distributions by coarse-graining and finding relations between their statistics and intensity/scale/intermittency/etc. The point process models, like Neyman-Scott, on the other hand are most widely used for the stochastic simulation of hourly rainfall, which is the temporal scale where they start having performance problems. The idea of nesting is therefore to use the point process models down to a scale where they perform well, and then downscale further in time with disaggregation models like the MRC. We found this optimal temporal resolution to nest the models to be around 24 hrs. In our view it is not necessary to do a joint calibration of the parameters of both models for this purpose, although this in principle would be possible. We will add more text on the calibration of the models, although we do not wish to duplicate the extensive literature on this subject. The nested model itself was explained in detail in Paschalis et al. (2013).

2. Independent verification

The referee raises the point that an independent validation would require that calibration and validation would not be done on the same period 1981-2010. There is some truth to this statement, however at the same time ours is not a trivial validation as suggested by the referee. First, we are calibrating a stochastic model, and yes the parameters are estimated from the entire period and should reproduce the statistics for the entire period (this is not the same like calibrating and validating say a hydrological model on a different period). Because we are calibrating the stochastic models separately, these statistics will not (can not) be reproduced perfectly by construction for all temporal resolutions. Besides they are not all related to particular model parameters. Second, what we would gain by calibrating and validating on different periods is an understanding of the temporal variability in the statistics themselves, and by extension the model parameters. This can also be achieved by deriving (or assuming) probability distributions of the model parameters and simulating explicitly the uncertainty due to model parameters, on top of the inherent stochastic climate uncertainty. In this case the uncertainty in the extremes would be even larger than what we find. So it is true that out of all possibilities we are presenting a case where we assume that the parameter set for the current climate is fixed and we study the stochastic variability in extremes it can generate, also for the future climate considering FoCs derived by different climate models. We will be more explicit on this limitation in the revised manuscript.

3. Variability in future climate

The referee is unsatisfied with our arbitrary choice of testing the 10-90% confidence band of natural variability. Indeed we had no specific statistical reason to quantify the differences between present and future climate mean extremes with the 10-90% uncertainty bounds in our paper, and we agree that our final Fig 7 is providing limited generality. We will re-adress this comparison in the revised manuscript by explicitly

quantifying the statistical significance of the differences in the probability distributions of simulated extremes for the chosen return periods and temporal resolutions, not just differences in means, and we will summarize the results for all 22 stations in the form of a new Fig 7. At this point we can also test other significance levels.

The referee finally points to the inclusion of FoCs "using a lot of statistical/numerical machinery whose validity is referred to the literature". We understand that this may turn off physically inclined readers, but we would also like to stress that this is a stochastic approach which reproduces observed statistics of rainfall, but does not claim to explain or include any physical reasoning leading to precipitation formation, for which numerical weather prediction models will be more suitable. However, for uncertainty analysis and future climate sensitivity the latter approaches have limitations.

For other comments of the referee which coincide with those of other referees please refer to the more detailed response to Referee 1.

References

Paschalis, A., Molnar, P., Fatichi, S. and Burlando, P. (2013), On temporal stochastic modeling of precipitation: nesting models across scales, Advances in Water Resources, 63, 152–166.

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