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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 raises the point that the nested model does not reproduce all of the observed statistics perfectly, referring to Figs 3 and 4, especially concerning auto-correlation and skewness, and that this is "overlooked or downplayed" in the paper (Comment 7). The referee suggests to "provide more tangible evidence that the approach reproduces realistic rainfall distributions and temporal structures". It is true that our model does not reproduce auto-correlation and skewness in summer as well as it does in other months. In the revision we will provide more error statistics that quantify this discrepancy. We would also like to add that: (a) The auto-correlation problem in the microcanonical MRC model is known and inherent to the additive weights. In our development of the nested model we have shown full correlograms for 10-min 1-hr and 24-hr rainfall where the correlation performance over many lags is good, and certainly much better than using only the external model without disaggregation (see Paschalis et al., 2013). It is however true that a bias in the auto-correlation may be responsible for miss-representing extremes with longer durations, so it is an important statistic to reproduce accurately. (b) The skewness problem is also related to the mass conservative nature of the microcanonical cascade and the distribution of cascade weights, among others. However we caution in over-interpreting the discrepancy in the skewness coefficient, because skewness is (and should be) very large in high resolution rainfall intensity, and most importantly its estimation by the skewness coefficient is highly uncertain. Regarding demonstrating performance with additional variables and statistics, we already provided tangible evidence of the performance of the nested model in Paschalis et al. (2013) with variables like the rain intensity, duration of wet spells, auto-correlation, intermittency. We are of the opinion that repeating those in the current paper is not needed.

2. High-resolution rainfall and climate change

The referee is critical of the simulation of sub-daily rainfall by our approach (Comments 3-4-5). Indeed it is correct that the small-scale convective rainfall is generated by different mechanisms than large-scale long-duration precipitation. In fact that was one of the main motivations for developing the nested approach to stochastic precipitation modelling which allows for the parameterizations of the scale-related mechanisms separately. We will stress this more in the revision. It is also true that convective rain intensity variations are indeed difficult to capture also by RCMs, as the referee points out. The referee invites us to elaborate on two aspects related to our stochastic approach: (a) the climate change signal in small scale extremes (especially the temperature effect), and (b) the missing diurnal cycle.

With regard to (a) the basis for the climate change signal in small scale extremes is that convective rain may respond stronger to air temperature than stratiform rain, thereby generating higher extremes. Indeed this sensitivity of extreme rain intensity to air temperature following the rate at which the water-holding capacity of the atmosphere increases with air temperature (Calusius-Clapeyron rate CC) has recently been studied by many authors, including ourselves in Molnar et al. (2015). It is a fact, that stochastic rainfall models (like our current approach) do not take into account this effect on rainfall extremes directly, also because this would require an independent simulation of the temperature time series as well as rainfall. We are of the opinion that our nested

approach reflects the state-of-the-art in stochastic rainfall modelling even without this addition. However, as we stress in the conclusions of our paper, we are currently working on additions of temperature sensitivity. As far as we know only Buerger et al. (2014) have presented an ad-hoc correction to the microcanonical RCM model which reproduces air temperature sensitivity of extremes in a rather subjective manner. We have instead approached the problem by directly parameterizing the scaling exponent in the cascade generator weights of the canonical RCM model with air temperature and are currently exploring the robustness of this method with data at the Swiss stations. First results were presented at EGU (Molnar et al., 2016). This work is not yet complete and it is not ready to be included in the current manuscript.

With regard to (b) the inclusion of a diurnal cycle would over-parameterize the current model. The diurnal cycle may indeed be important if the nested model is used to generate precipitation for flood impact studies involving other hydrological processes with diurnal timing (e.g. snowmelt), but in most impact studies focused only on rainfall extreme intensity, we believe its timing during the day is less of a concern. It is important to keep in mind that this is a stochastic model, and it is counterproductive to attempt to include more physical processes in it, such as precipitation timing, at the expense of over-parameterization. Depending on the application in mind, it may then be advisable to use a numerical weather prediction model with the additional limitations that poses.

3. Model validation

The referee asks for a more quantitative validation of the model with goodness-of-fit metrics and detailed exploratory analyses, claiming the arguments in the current manuscript are qualitative and not convincing, especially for summer when most of the extremes occur (Comments 6-7). In the revised manuscript we will report error measures for the key statistics. It is important to stress that the purpose of the nested model was to have a stochastic rainfall simulation tool which generates good performance across a wide range of temporal scales, and at the same time allows for straight-forward calibration with high-resolution station data and is numerically efficient for stochastic simulation and uncertainty analysis. Our aim was not to develop new stochastic models, but rather put together existing, well-established and tested approaches for rainfall simulation, to get the best out of them at a range of temporal scales. In this sense, the performance will have limitations that are inherent to the external models and the internal disaggregation models used. In other words there is a limitation to what can be achieved with the existing framework, and the relatively poorer performance in reproducing intense summer rainfall may be one of those limitations. Beyond stressing this fact we cannot do much more about the comment of the referee on this subject.

4. Factors of Change

The referee points out that Factors of Change (FoC) are derived from climate modelling experiments at a grid scale and used at a station scale also at temporal resolutions that are finer than the original climate modelling timescales, and asks for more details on these steps (Comments 8-9). We will provide these in the revised manuscript. Because of the physical constraints in the climate models, it is very unlikely that FoCs change dramatically in space over the relatively small spatial extent of our domain (Switzerland). The differences between FoCs derived from different models is expected to be much greater, and we have found this to be true in other regions. At the same time it is true that we are assuming that the FoCs derived for the spatial grid scale can be applied at the station (point) scale, as others have done and we cite these works in the manuscript. Regarding the use of FoCs at different temporal resolutions we will explain this point better in the revised manuscript. FoCs are applied to statistics for a future climate in order to re-estimate the parameters of the external precipitation model (Neyman-Scott in this case) which are dependent on these statistics at different temporal resolutions. Because the FoC-affected statistics are only available at coarser than daily scales, we have to extrapolate them to finer temporal resolutions. We do this by scaling functions between statistics at different resolutions which we estimate from data: this works very well for variance and intermittency with fuctions published in

the literature. Because the skewness coefficient is so highly uncertain, we chose not to make it scale-dependent. It has to be mentioned that the apparent overestimation of skewness in sub-hourly resolutions in Figs 3 and 4 is not due to the FoC scaling, because the fit for daily data is good, but only due to the MRC disaggregation procedure which does not include this statistic in its calibration.

5. Rainfall extremes

The referee is concerned that the statements about future climate predicted extremes lying inside the bounds for the current climate are not quantified properly (Comments 10-11-12). Indeed we had no specific statistical reason to quantify the departures 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. We agree with the referee that the conclusion that a future climate change signal in rainfall extremes may be in-detectable given the natural variability in extremes pertains only to our model, and we will stress this in our revision.

6. Methodology

On methodology, the referee suggests to show only the results for the MRC B model version where the cascade weights are parameterized with intensity and scale (Comment 1-2). We will consider removing MRC A from the revised manuscript.

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