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Interactive comment on "Stochastic modelling of spatially and temporally consistent daily precipitation time-series over complex topography" *by* D. E. Keller et al.

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The authors would like to thank reviewer #1 for his/her valuable and detailed comments. We will first give a general reply, then answer the specific comments, and subsequently address the technical comments. For clarification, the referee's comments are repeated first followed by the authors' replies.

General reply

We are happy that referee #1 likes the presentation of our article in terms of readability, clarity, structure and presentation of visual graphics. His/Her specific comments

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are excellent suggestions for improving the current generator. Some of the suggestions require rather fundamental changes beyond the scope of this paper. Therefore, these particular issues must be tackled more thoroughly in future work. However, as suggested by the reviewer, we will discuss these aspects in the text of the revised manuscript version and put the existing approach in this context.

Following the two reviewers' comments, we will sharpen the motivation and goal of the presented study substantially in order to meet the expectations. The overarching goal is to document the specific implementation of a multi-site weather generator suggested in the literature, validate this implementation for the current climate and apply the generator as a statistical downscaling tool for future local climate. The latter will be presented in a separate, additional study currently in preparation. We therefore deliberately chose a simple tool that can be easily adapted in a climate change context, thereby complementing and improving existing climate change scenarios for Switzerland. Perturbing the generator for a future climate is part of the subsequent article. The aim of the article under revision here is to test and evaluate the implementation of the multi-site precipitation generator under current climate conditions in order to understand its capabilities and caveats. In the revised manuscript we will better motivate our study in the abstract, the introduction and through the text.

We agree with the reviewer, that the novelty of the presented article lies not in the generator-tool itself, but rather in its concrete implementation and application to a Swiss catchment including the presentation of the complex pathway to calibrate it. Following the comment of referee #2, we realized that our manuscript title does not well reflect this aspect and might even evoke wrong expectations among the readers. We have therefore changed the title to "Implementation of a multi-site precipitation generator to a Swiss river catchment".

Specific Comments

Comment 1: This model captures the mean behavior relatively correctly, but does not capture extreme events properly. The mixture of exponential variables, which describe precipitation amounts, is not only unable to reproduce observed extremes (see Fig. 9), but it is certainly even worse at predicting future (non-observed) extremes, owing to the light tail of the assumed exponential variable. . .The model advocated by Vrac and Naveau (2007), which uses a generalized Pareto distribution (GPD) for the tail, has an asymptotic justification and thus much better at estimating probabilities for rare events. If the goal of the proposed WG is to be used in impact and risk assessment studies (as claimed several times in the paper), possibly in a climate change context, I think that extreme events should be better captured.

Reply 1: We agree that the proposed mixture model of Vrac and Naveau (2007) better represents daily extreme precipitation amounts. However, fitting this model to daily precipitation amounts at single months introduces a large uncertainty, since the proposed model relies on 5 parameters (gamma shape, gamma scale, threshold, pareto scale and pareto shape). Additionally, although the GPD distribution alleviates the underestimation of daily extreme events, it likely does not improve the reported underestimation of multi-day precipitation sums (Fig. 9). This is because the precipitation amount model currently does not include any autocorrelation. In our view, this aspect is of even greater concern than the underestimation of daily extremes. However, improving our precipitation generator with regard to multi-day extremes goes beyond the scope of this study and will be part of future work. We will add a sentence to Section 5 (Summary and outlook), giving a perspective on this aspect.

Concerning further model development, we plan to refine the presented WG in a future study by using multi-state Markov chains (e.g. dry, wet and very wet) in combination with different probability density functions for wet and very wet days in order to improve its main deficiencies. The use of this approach has the advantage to better capture one-day and multi-day extremes as well as to include a temporal memory in the precipitation amount process.

The reviewer is right that care should be taken when using our simulated time-series as a data basis for any risk and impact assessment studies. We will reformulate those sentences with this aspect.

Comment 2: The proposed WG simulates stationary time series (month by month). Does this make sense over a period of 51 years (and for the near future)? A non-stationary model could perhaps explain part of the inter-annual variability, that is not captured in Fig 4. And also, again, if the goal is to use this WG for risk and impact assessment, does it make sense to assume that climate is stationary? Can the WG be used to extrapolate in the future?

Reply 2: Our goal is to develop a simple tool that generates time-series consistent with mean conditions. In addition, it should be easily adjustable for future climate mean conditions. In a climate change context (to be published in a follow-up article) we will use standard climatological periods of 30 years. In this study, however, we chose a relatively long time-period of 51 years, in order to accurately assess the added value of a multi-site generator against multiple single-site generators. Reducing the time-window of calibration increases the sampling uncertainty (as shown in Figure 3 with artificial data) and hence the determination of added value becomes more uncertain, too. In the revised manuscript (in Section 2) we will include this reasoning for the particular choice of time-period.

Nevertheless, introducing a non-stationary model would certainly be a valuable extension to the current generator. Although precipitation over the Thur catchment does not feature a trend over the 51 years, one could sample from the observed interannual variability in the WG-parameters and add henceforth an additional stochastic component. This direction of future development is briefly mentioned in Section 5 at page 8762 L25 – page 8763 L4.

Comment 3: The model captures spatial coherency between monitoring stations, but does not describe spatial dependence at ungauged locations. Therefore, it is impossible to simulate precipitation data over the whole catchment, which may be essential for risk assessment (e.g., if the simulated data are needed as input of a hydrological rainfall-runoff model). All pairwise correlations between the different stations are computed empirically, although there is a large geostatistics literature about Gaussian processes, (stationary or non-stationary) correlation functions, etc. Why not fit a correlation function to the data, which would: - allow simulation over the whole catchment, - decrease the number of parameters drastically (therefore also the uncertainty) by exploiting the inherent spatial structure, - automatically yield positive definite correlation matrices (without any adjustment), - easily generalize to many more time series?

Reply 3: We agree that the suggested geostatistical model is an interesting alternative approach but unfortunately it is not compatible with our proposed model for the following reasons: (a) The number of parameters in our model is certainly large and a correlation function would reduce this number drastically. However, from a theoretical point of view, geostatistical models (be it isotropic or anisotropic models) rely on the assumption of stationarity, which is not sufficiently fulfilled in regions with a complex topography (e.g. Schiemann et al., 2010), such as the Thur catchment in this study. In addition, from a practical point of view we see some problems in estimating a correlation function. Since we only use a small number of stations (8) the uncertainty of fitting a correlation function would be substantial. As it can be seen from the Correlogram (see Fig. 1 below) the correlation (p) shows large variations for all inter-station distances (h). (b) The potential to simulate precipitation also at ungauged location is obviously appealing. However, the specification of a correlation function would not be enough. Additionally, further parameters of the precipitation generator (i.e. transition probabilities and parameters of the probability density function) would have to be interpolated consistently in space. This is not straightforward given the complex topography. (c) Concerning the problem with the positive definite correlation matrices, a correlation function could not alleviate this problem. This is because it is the pair-wise optimization process (see chapter 3.3.2, page 8749) that yields non-positive correlation matrices and not the estimation of the observed correlation matrices. We will clarify this aspect in the revised manuscript at page 8750, line 18.

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Nevertheless, we think it is certainly worth mentioning this alternative modelling approach in the discussion part of our revised manuscript together with an explanation why we have decided against it.

Comment 4: The number of parameters in the model is very large, so simplifications should be considered, e.g., by - fitting a correlation function (see point (3)). This would decrease the number of spatial dependence parameters, though spatial heterogeneity might be difficult to take into account. - fitting a global yearly model, for example using splines or sines/cosines (instead of fitting separately one model per month). This would decrease the number of temporal dependence parameters, and yield a coherent model throughout the whole year.

Reply 4: We agree with the reviewer that fitting a global yearly model would decrease the number of parameters, for instance by specifying a GLM with cos/sin harmonic functions as co-variates as e.g. described in Furrer and Katz (2007). However, since our generator is later subject to be perturbed for future climate using a delta change approach, we would rather stick to empirical estimates of these parameters for each month separately. In a GLM-context, the prediction of future WG-parameters would have to be done using a number of co-variates (such as e.g. in Beuchat et al. 2012), assuming stationary link functions between the predictors and the predictand. Furthermore, the selection of the (large-scale) predictors is far from trivial. Especially in regions where local effects dominate the precipitation process (e.g. Alps) there is a risk of model over-fitting. Also, to accurately capture the seasonal cycle in the climatic change of certain precipitation characteristics (e.g. wet-day frequency) would be a huge challenge. For these reasons, we opt here for the simpler solution.

Furthermore, the proposed model development would not substantially improve the model in terms of its main deficiencies: i.e. representation of extremes, too low inter-annual variability, and underestimation of multi-day spell lengths. For future research, we would rather spend resources in improving these main deficiencies.

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Regarding the spatial correlation function we refer to reply 3.

Comment 5: The Markov chain of order 1 does not capture well multi-day spells (see Fig. 7, for example). Maybe, a 2nd-order Markov chain (AR(2)) would do a better job. . . Or maybe a model of type 'ARMA(1,1)' would fit better? Of course, the number of parameters would increase if a more complex model is considered, but this would also better capture long-range dependencies. . .

Reply 5: To select the order of the Markov chain model we consulted the BIC. Based on this analysis we did not find a substantial improvement of 2nd-order against 1st-order MC (see Figure 2 below). We therefore opted for the simpler model to also limit the number of parameters. Given a rather short sample size, the risk of introducing large uncertainty would be much higher for a 2nd-order model compared to a 1st-order model. For further details we refer to our reply to your technical comment # 2. Note, that selecting a higher order Markov model does not alleviate the underestimation of long multi-day spells to a full extent. To improve the duration of multi-day spells, the model would have to be conditioned on other atmospheric variables (in particular circulation-related) (Chandler and Wheater, 2002). This however is out of scope of the present study. Finally, if a correct reproduction of long-lasting spells is the main focus, spell lengths generators (e.g. Racsko et al., 1991) might be the more appropriate way forward.

Regarding the suggestion of specifying an ARMA (1,1) model, in our view this would be inappropriate, since we are dealing here with discrete data (dry/wet) rather than with continuous time-series.

Comment 6: In Section 4, the authors validate their WG by looking at different temporal or spatial statistics, such as long-term mean, inter-annual variance, PDF of non-zero precipitation, dry and wet spells, annual maximum sums of consecutive days, etc. However, I guess that there is no validation of space-time interactions. For example, if Z(s,t) denotes the precipitation amount at station s and time t, a possibility would be to see if

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the model and the data agree on statistics of the type 'X = P(Z(s2,t+k)>z | Z(s1,t)>z)' for increasing values of z? Here, the statistic X represents the probability that it rains at least z mm at station s2, given that it rained similarly at station s1, k days earlier. Have the authors checked this kind of space-time dependencies?

Reply 6: We have not validated this aspect. Since the investigated catchment is of rather small size, it is very likely that rain occurrence or non-occurrence will be recorded at all stations simultaneously on the same day (see also Table 1 in the manuscript). Certainly, if we have a larger catchment, e.g. those of the Rhine river, it would be interesting and worthwhile to investigate these time-space statistics. A catchment that is frequently affected by frontal passage of rain, this would be a very relevant aspect of analysis. We will add a sentence to the outlook-section of the revised manuscript.

Comment 7: The title of the paper is 'Stochastic modeling of [. . .] over complex topography', but the topography information does not appear anywhere in the model. Hence, how could the model be modified in order to incorporate information about altitudes, slopes, etc., and therefore hopefully better predict precipitation at unobserved locations?

Reply 7: The consideration of topographic effects is a prerequisite for establishing a weather generator that may be applicable comprehensively over a complex topography area (not just multiple sites).Generalized linear models (McCullagh and Nelder, 1989) or Bayesian Hierarchical models (Gelman et al. 2004) are theoretically appealing frameworks that allow modelling of physiographic dependencies into the amount part of our weather generator. This alone is however not sufficient for a space-time weather generator, as the spatial dependence of (daily) precipitation is also determined by spatial autocorrelation not just the physiographic conditioning of parameters. Clearly, the development of a space-time weather generator that deals with spatial autocorrelation, physiographic conditioning, intermittence and temporal autocorrelation is far from trivial and will require fundamental methodological development, before applications can be attempted.

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Technical Comments

Comment 1: p. 8742, L.7: A reference about space-time modeling of rainfall extremes in Switzerland is the following: Huser, R. and Davison, A.C. (2014, JRSS B), "Space-time modelling of extreme events".

Reply 1: We will insert this reference at the indicated text position.

Comment 2: p. 8743, L.25: The BIC is used to select the order of the Markov chain. However, it is known that it typically over-penalizes complicated models (which might explain why a 2nd order model was not retained). What does the AIC say?

Reply 2: It is true that the BIC criterion tends to be more conservative than the AIC criterion. We chose the BIC as a model selection criterion for two reasons: First, we deliberately aimed for a simple model. Second, at large sample sizes as here with 51 years of daily data, the AIC tends to select over-proportionally higher-order models (e.g. Katz 1981 or Wilks 1998). Both, the AIC and the BIC show a large improvement when we go from a zero-order to a first-order model. However, the difference when going from a first-order to a second-order is almost negligible (see Figure. 2), which is true for both AIC and BIC.

Comment 3: p. 8744, L.2: A threshold of 1mm/day was used. How sensitive are the conclusions with respect to this threshold? How many zeros are there?

Reply 3: The threshold of 1mm/day to distinguish between wet and dry is standard practice for station measurements (see e.g. "Peterson et.al: "http://etccdi.pacificclimate.org/docs/wgccd.2001.pdf", WMO, Rep. WCDMP-47, WMO-TD 1071, Geneve, Switzerland, 143pp"). An analysis of the threshold sensitivity was not performed so far. We expect that our main conclusions about the added value of a multi-site WG also hold for another threshold. This is because the precipitation generator, be it in a single-site or multi-site configuration, is calibrated at individual stations with the same WG parameters (amount and occurrence process) for both con-

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figurations. A shift in the threshold would therefore affect both configurations in the same way.

We do not fully understand the question about the number of zeros. In particular, for which season, station, granularity does the reviewer refer to? The number of zeros per given month and station depends on the precipitation regime of the investigated stations. For a high-elevation site such as "Saentis" the wet day frequency ranges from 0.37 in fall up to 0.53 in summer (see Fig. 5 in manuscript). Consequently, the dry day frequency ranges from 0.47 (summer) up to 0.63 (fall), this yields about 15 to 19 dry days (zeros) per month. For a low-elevation site such as "Andelfingen", the wet day frequency is more constant through the year and amounts approximately 0.35 with about 20 dry days per month.

Comment 4: p. 8745, L.22-23, 'they underestimate [. . .] (gamma distribution)': This sentence is misleading, I think, because the exponential and gamma densities decay at the same rate at infinity, so they are likely to give similar probabilities to extreme events.

Reply 4: We agree and we will rewrite this sentence.

Comment 5: p. 8746, L.7-8, 'The parameters [. . .] maximum-likelihood': As already mentioned above, it would be better to have a spatial model linking the parameters, and estimate everything simultaneously (instead of estimating a lot of parameters separately from station to station).

Reply 5: We refer to our reply to your specific comment # 3. We will take up this idea as alternative modelling approach in our discussion part.

Comment 6: p. 8747, L.7-8: The point 4.2.2 is not very clear to me...

Reply 6: The point 4.2.2 simply explains how we randomly sample from a mixture of two exponential distributions. In particular, a random number (lying between 0 and 1) is compared to the quantile-function of the mixed distribution to assign the corresponding precipitation amount at a given day. We will rewrite the sentence to clarify.

Comment 7: p. 8747-8748, S3.3.2: How are the correlation matrices estimated? Empirically? If so, this might induce problems if the number of stations is large, and also it does not ensure that the correlation matrices are positive definite. A better solution is, as explained above, to assume and estimate a correlation function.

Reply 7: Yes, the correlation matrices were estimated empirically on a monthly basis. Indeed, we encountered problems when including more than 12 stations. The reviewer is also correct that the chances are higher that the correlation matrices are not positive definite. We have reported on both these problems in the original manuscript (page 8750, line 17-18). These limitations are certainly a strong downside of the proposed generator. We aim at improving the generator in this direction in future work.

Comment 8: p. 8750, 19-20, 'In absence of [. . .] matrix was chosen': How was this fall-back solution implemented? By minimizing a certain norm? If so, which one?

Reply8: The nearest positive definite matrix was found by applying the R-function "nearPD" from the R-package "matrix". This function uses the algorithm proposed by Higham (2002), which uses a weighted version of the Frobenius norm. In the revised manuscript, we will include an additional sentence to clarify this issue.

Comment 9: p. 8751, S3.4.1: This section shows that the model has a lot of parameters, and that it is crucial to reduce this number to avoid huge uncertainties and optimization issues...

Reply 9: We partly agree with the reviewer. For the replies we refer to your specific comment # 3 and # 4.

Comment 10: p. 8752, L.18, 'roughly 19': In fact the theoretical reduction is (asymptotically) sqrt{10000/30} = 18.3. . .

Reply 10: Yes. Thanks. We will substitute 19 by 18.3.

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Comment 11: p. 8754, L.27, ‘33%’: This percentage seems very low! This might be due to nonstationarities that the model is unable to capture, or simply because the model strongly underestimates probabilities of extreme events.

Reply 11: Indeed, the percentage is low but comparable to other studies applying a similar WG model (e.g. Gregory et al. 1993) as mentioned in the manuscript. We agree, that the underestimation of inter-annual variability in the monthly sums is attributed to the stationarity assumption in the annual cycle of the WG parameters. We consider to highlight the existing discussion of this issue more prominently. Also, in the outlook-section we have outlined how this problem could be circumvented (page 8762, line 26). Regarding the underestimation of extreme events, we suppose there are several issues contributing to this problem. The precipitation generator not only misses the large observed precipitation sums, but also misses to capture the small observed amounts of precipitation sums (compare violet and grey shading in Figure 4). The variability underestimation is therefore more likely caused by forcing our generator solely by mean conditions, whereas in real-world there are large fluctuations from year-to-year.

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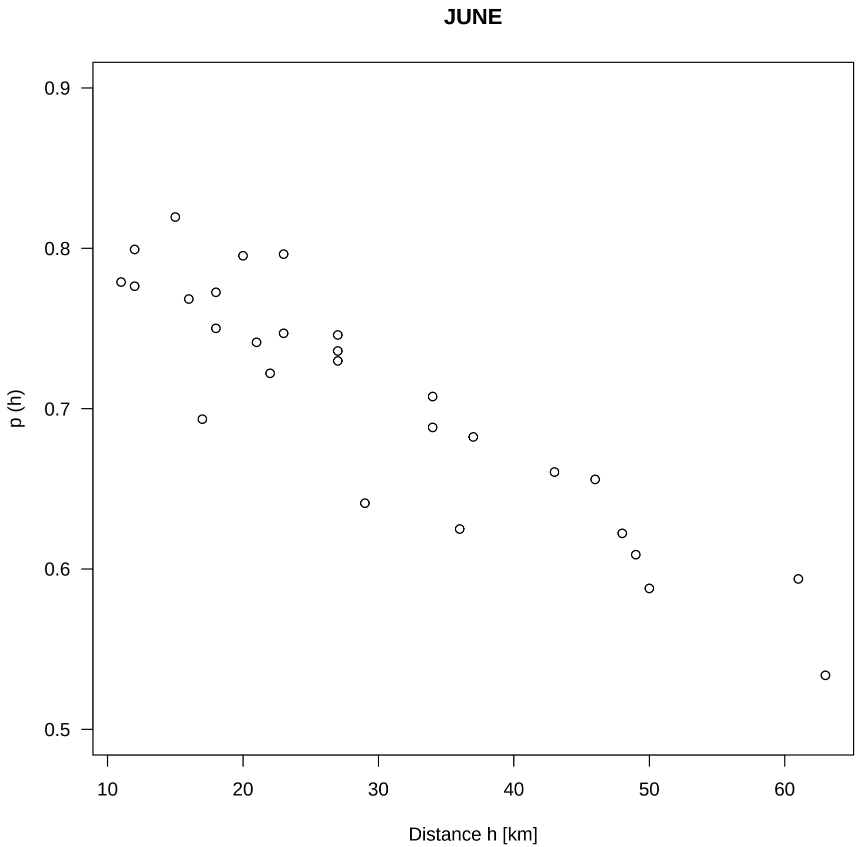

Fig. 1. Correlogram for the investigated eight stations of the "Thur" catchment. It describes the spatial correlation p as a function of the inter-station distance h. Each point refers to a station pair.

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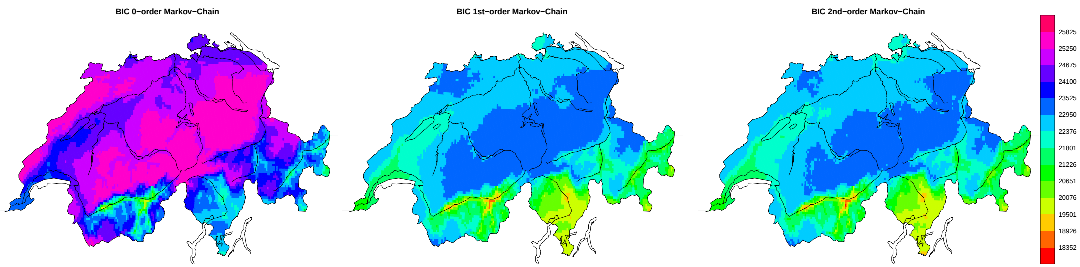

Fig. 2. Bayesian information criterion (BIC) for 0-order (top), first-order (middle) and second-order (bottom) Markov chain over Switzerland. The BIC was calculated over a time-period of 1961-2011.

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