Response to reviewer #1 (Simon Michael Papalexiou)

This is a useful paper and deserves consideration. I will avoid a generic summary and list right away several comments that hopefully the authors might find useful.

Response: I thank the reviewer for their many constructive and insightful comments.

As a general comment, the methods described suit more multisite methods, especially for precipitation. At the hourly or finer scales, space-time precipitation has components or advection and anisotropy, and it is not clear if what these methods preserve in this regard (I wil get back to that).

Response: It is true that the model presented does not preserve any attributes regarding advection or anisotropy. The focus of the research was predominantly developing methods that allow for the synthesis of large stations networks (station networks up to 100), as previous versions of the single site alternating model could only achieve this to a limited extent (station networks less than 10 – see Haberlandt et al. 2008). Anisotropy could be considered in future revisions by adding direction as an independent variable within the objective functions used in the simulated annealing optimisation approach. Advection may also be considered by considering temporally lagged values of one or more of the three bi-variate rainfall dependence criteria. This topic will be discussed further in responses below.

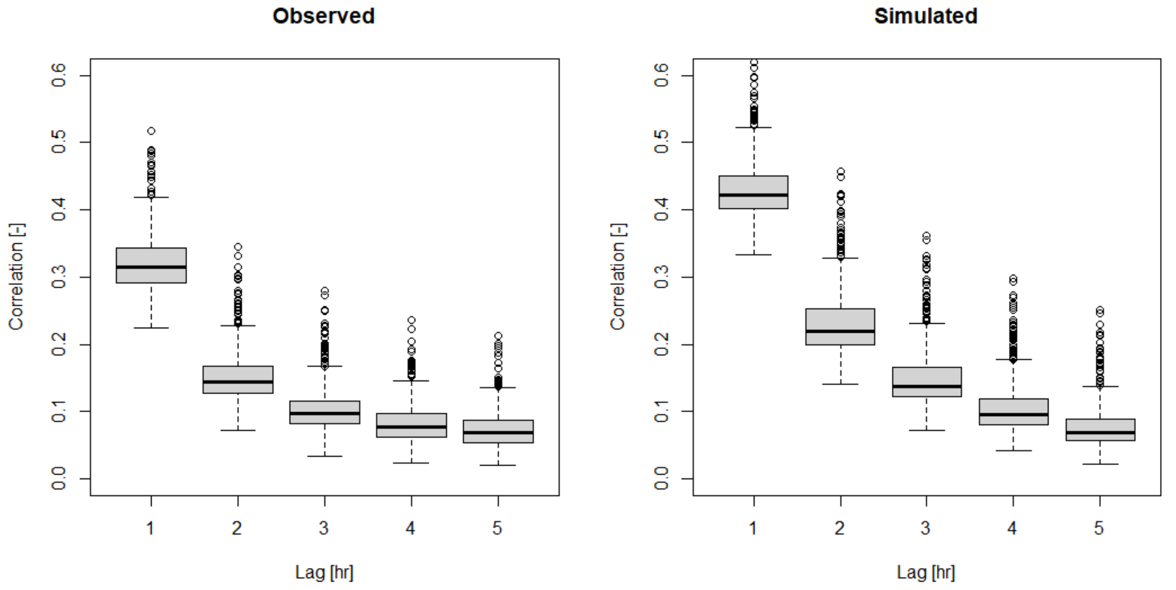
1. please see also Papalexiou (2022) which is dedicated only to precipitation.
   Response: Thank you for the reference. The introduction will be updated accordingly.
2. Rainfall is described by the single-model as an alternating sequence of independent wet and dry spells. How valid this assumption is?
   Response: Observed auto-correlation of the event variables is low. For instance, the median lag-1 auto-correlation across all 699 stations is 0.0269 for wet spell amount, 0.0822 for wet spell duration and 0.0482 for dry spell duration.
3. I can understand the 1 mm limit but based on what rationale the DSDmin = 4 hr was set?
   Response: Choosing 4 hours for the DSDmin value was a somewhat arbitrary decision. The previous study by Callau et al (2017) used a value of 1 hour. The German guideline for calculating return periods of extreme rainfall events (DWA-A 531) requires a dry period of at least 4 hours between events to be considered independent. The text will be updated to include more background as to this choice.
4. if u,v are in [0,1] then where are F_U(u) and F_V(v). Check the notation please.
   Response: Thankyou for the comment. Notation will be revised.
5. What is the justification of this choice? Have you tested e.g., the Gumbel dependence and was not suitable? Did you observe asymmetries? Please justify.
   Response: I assume the reviewer here is referring to the choice of copula? The main driver in the choice of Khoudraji's device, is the presence of asymmetries in the observations (events with large WSA, but small WSD). As to the choice of the Gumbel copula for C_2, many copula families were tested by trial and error, and Gumbel was found to lead to the best performance regarding wet spell intensities. A better explanation regarding the choice of copula will be included in the revised paper.
6. Similarly, what led you to the Weibull choice for DSD and WSA. Great that you've drop the 4-parameter Kappa but why Weibull is a good model for the WSA. If you consider that, e.g., the hourly wet value distribution is a specific distribution then then

WSA would be its convolution. Specifically, for the Weibull there were some attempts to justify it theoretically as a rainfall distribution by Wilson & Toumi (2005); it was also used in meta statistical approaches for daily rainfall e.g., (Marani & Ignaccolo, 2015; Marra et al., 2018, 2023) and it seems it does a good job in describing the extremes but if it is suits well for the WSA it will be nice to show some evidence. Also, here you're using the 3-par version which also can end up with ζ quite larger than the min so you might have inconsistencies in low values. Can you explain please? The same point holds for the distribution choice of the WSD. Is the LN supported by the literature? By your own analysis in this dataset?
Response: Like for the previous response, the Weibull was chosen more through trial and error than any rigorous theoretical foundation. Many distributions were explored, then goodness of fit criteria such as the Cramer-von-Mises test and visual tests such as QQ-plots were used to arrive at the choice of Weibull for the DSD and WSA and LN for the WSD. If requested, a section can be included to discuss the choice in more detail and perhaps provide some goodness-of-fit results. Also, sometimes the lower bounds of the distributions falls below the WSAmin or DSDmin, in which case any simulated value below these thresholds are replaced with the threshold value. The text will be updated to mention this.

Equation 7. So this implies that the tail of intensity is exponential? It could for this region but in general this contradicts many global studies indicating that the tails are not exponential but heavier. Exactly for this reason, in the past I explored the Generalized Gamma to allow heavier right tails and recently some Generalized Exponential distributions having similar tails (see Papalexiou, 2022). I believe the choice of model is crucial as we're risking underestimating the potential for extremes. Please explain.
Response: Equation 7 describes the distribution of rainfall within the event. It should be emphasised that the internal distribution of rainfall within an event is considered of lower importance due to the intended end use of derived flood frequency analysis of meso-scale catchments where an exact reproduction of the time series over small time scales isn't necessarily required. The wet spell peak of an event is modelled through a copula that describes the dependence between the ratio WSP:WSA and the wet spell duration (eqs 2, 3, 8) and the Weibull distribution to describe the ratio WSP:WSA. Equation 7 only applies to the timesteps before and after the wet spell peak (see figure 2 for a visual representation). The text will be modified to make this clearer to the reader.

Also what is the correlation structure within the event? Clearly at hourly resolution there is strong autocorrelation within wet values (see Papalexiou, 2022).
Response: Auto-correlation is generally over-estimated in simulated values (see below plot of observed and simulated auto-correlation for all 699 stations up to lag 5). A plot and

discussion of the correlation structure can be included in the final manuscript if required.



1. lower phi is typically used for the gaussian pdf, here you need capital phi Φ
   Response: thankyou for the comment. Will be revised to Φ.

Section 2.2. Operationally, how fast is this optimization approach? When you mention that the occurrence criterion in the hardest to converge does this imply that in many cases it does not converge at all?
Response: Convergence performance is a topic which perhaps could be discussed in the paper at greater depth. The performance of the convergence can be summarised across all catchments by Figure 8, as these three bi-variate spatial rainfall criteria comprise the objective function of the optimisation approach. It is clear however that smaller networks will converge better than larger networks, as each random swap needs to satisfy more neighbouring stations. During writing of the paper I did consider showing plots showing directly convergence performance (by say showing end objective function values plotted against catchment size), but in the end I thought the inclusion of Figure 8 conveys enough information about the spatial performance of the model and the exact mechanics or the convergence are probably of less interest to the reader.

1. I guess the branched non-sequential procedure is describe correctly, yet to be honest as a reader I got lost here and mathematically it is really not clear what exactly spatiotemporal correlation structure this grouping of primary and secondary stations produces.
   Response: a mathematical background for the branched approach is indeed not present. The branching was developed in order to reduce the computational complexity and expense when dealing with large station networks. Branching is an attempt to transfer information through the network as best as possible without requiring an objective function which contains all stations within the network. This reduces computational expense but also makes sense as we should place more computation effort on nearer stations than farther stations, as the spatial dependence of farther stations is less anyway. In summary, the branched method was developed over a long period of trial and error, and has little mathematical background.

2. so this approach will preserve only the lag-1 correlations?
   Response: no lagged correlations are preserved. The simulation of advection and storm fronts is not considered in this work, as it was not considered a priority for the end use of derived flood frequency analysis of meso-scale catchments. For smaller and urban catchments the reproduction of storm fronts is probably useful. The approach developed here however could incorporate lagged correlations within the objective function if storm advection were to be considered a priority in the models end use.
3. Just to be sure, the disaggregation you are using is not stochastic? Right? Each daily value in transformed to the hourly ones by using the deterministic functions if I understand well. If this is the case then there aren't any fluctuations. Please clarify and state that in the text.
   Response: I am unsure exactly to which disaggregation the reviewer is referring to here? Within the rainfall model, the only disaggregation which occurs is to generate the hyetograph of each rain event (lines 137-146, equation 7) where the event rainfall depth (WSA) is disaggregated to hourly timesteps. The hyetograph profile is deterministic (equation 17), however the wet spell peak intensity and timing are derived stochastically. If the reviewer is referring to the disaggregation of the non-rainfall climate variables, this is indeed purely deterministic.
4. So the process preserves correlations within each catchment and not in the whole network of the 699 stations, right? Up to how many stations can this method applied effectively. For example, in our latest work (Papalexiou et al., 2023) we can go up to 10,000 stations easily preserving marginals and corrections. What are exactly the theoretical components that your approach preserve?
   Response: Correct, the spatial consistence is preserved on a per catchment basis. In this study, the largest catchment contains 87 stations. In theory, nothing prevents the method being applied to the entire study area simultaneously. The limitation is generally the memory requirement of the computer, as the hourly time series for all stations must be loaded in memory. Depending on how many years are simulated at once, the numeric matrix required will be very large. To overcome this issue, one could apply the method to shorter time series. This has not yet been attempted, but is possible. As our intended use is derived flood frequency analysis, where each catchment is anyway modelled independently, so the benefit of running the method across the entire study is diminished. It should also be noted that modelling at the hourly timestep (as opposed to daily timestep in Papalexiou et al., 2023) adds both complexity and a non-trivial increase in computational cost. The simulated annealing optimisation used is by no means fast, as millions of swaps are trailled and by swapping events and not timesteps, recalculating the objective function across all timesteps between swapped events (and for all relevant station pairs) can be slow. The three and only theoretical components that are preserved in the model extension into space are the three bi-variate rainfall criteria described by equations 9-11.

Please see also the works of (Peleg et al., 2017) and (Paschalis et al., 2013).
Response: I thank the reviewer for these interesting papers regarding high resolution rainfall models. The introduction will be updated accordingly.

1. This means that monthly variations within this summer and winter period is smoothed out? If you assess the simulation monthly within this period will it match the observed monthly characteristics?
   Response: correct, all months of summer show the same behaviour, as do all months

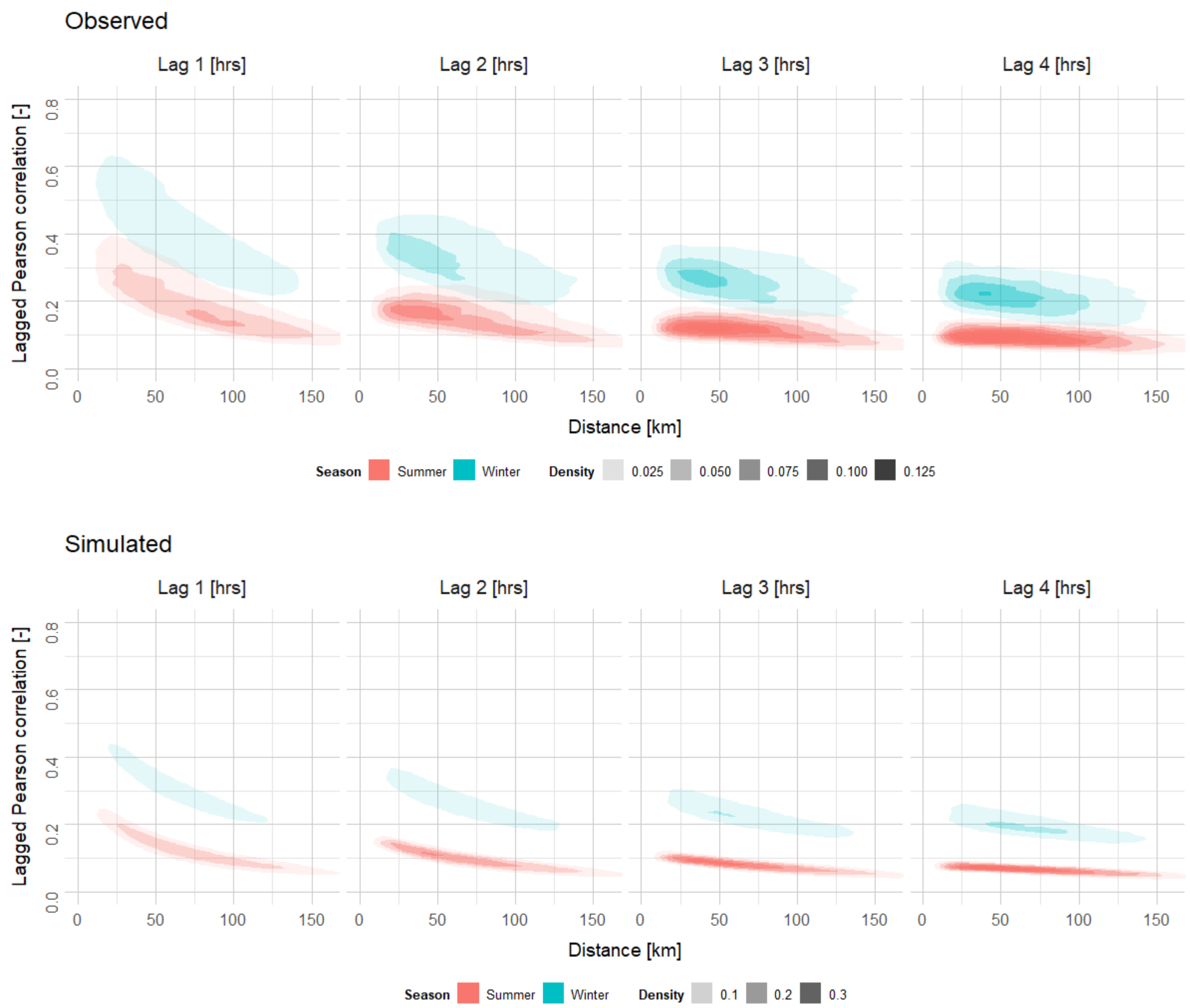
of winter. A further conditioning of the model by calendar month is of course possible, however there is often the problem of a lack of observations for model fitting, as hourly observations are generally not widely available across Germany until around 2006 onwards.

Section 4.3. Can you show a graph of a synthetic time series and an observed, and maybe for a station the probabilities of the length of wet and dry spells vs the observed ones?
Response: I think here the reviewer is referring to section 4.2 (rainfall) and not 4.3 (non-rainfall climate variables)? As mentioned above, the internal structure of the rainfall model is not intended to exactly mimic the observed behaviour of rainfall events. As such a plot showing synthetic vs. observed time series may be counter productive. However a plot showing probabilities of wet and dry spells is a good idea and can be included in the final manuscript.

Section 5.2.

As I mentioned I feel that this is better described as a multisite model. Does this approach have any control over advection (linear or described by generic velocity fields) or anisotropy that characterize fine scale precipitation (please see Papalexiou et al., 2021). These are important points that need to be clear discussed for precipitation even as limitations of this approach.

How about the lagged correlations of precipitation?
Response: the performance regarding reproduction of lagged correlations of precipitation is generally poor (see plot below), especially for lag-1, which is not surprising considering that it is not considered within the objective function of the simulated annealing procedure. As advective properties are not a focus of the study (see responses above), discussion of lagged correlation has been omitted, but can be included if deemed necessary.



Section 5.3. I wonder again if the grouping in winter and summer, e.g., Fig 14 is too coarse, especially for the variables such as temperature where there typically strong monthly variations.

Response: This grouping in summer and winter only applies to rainfall. The selection window *w* in the k-NN resampling process is what enforces seasonality for the non-rainfall climate variables. Figure 14 is showing summer and winter seasons only in order to reduce the amount of information shown. This figure could be expanded to show the four regular seasons if requested.

Overall, this is an interesting and useful paper that improves and extends the authors previous works and has its place in the literature. There are many methodological choices that can be better justified, several points that need clarifications, some algorithmic descriptions were hard to follow, the assessment of the generated time series can be improved, and finally, I felt that it is was not clear what theoretical properties this approach exactly reproduces and what are the limitations. I believe also a discussion section will benefit the paper were the authors should summarize limitations, maybe future extension, and put their work in context with other works. My comments are optional, and the authors can ignore them, yet I deem that this work needs amendments to became clearer and more accessible.

Response: I again would like to thank the reviewer for his detailed and constructive comments and I believe most points made are valid. As the model presented lacks advection or direction properties, this detail should definitely be discussed to make clear the limitations of the model but also to discuss possible future model revisions. The theoretical properties reproduced will also be outlined better to the reader. I would also agree that the algorithmic descriptions needs to be improved, which will be a focus of the final manuscript revision.

Sincerely,

Simon Michael Papalexiou

References used within responses:

Callau Poduje, A. C.; Haberlandt, U. (2017): Short time step continuous rainfall modeling and simulation of extreme events. In *Journal of Hydrology* 552, pp. 182–197. DOI: 10.1016/j.jhydrol.2017.06.036.

Haberlandt, U.; Ebner von Eschenbach, A.-D.; Buchwald, I. (2008): A space-time hybrid hourly rainfall model for derived flood frequency analysis. In *Hydrol. Earth Syst. Sci.* 12 (6), pp. 1353–1367. DOI: 10.5194/hess-12-1353-2008.