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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Stochastic generation of multi-site daily precipitation for the assessment of extreme floods in Switzerland" *by* Guillaume Evin et al.

Anonymous Referee #1

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1. Overview

In the manuscript, the authors compare three different types of daily precipitation generators for the application in flood hazard modelling. One model is the so-called "Wilks-model", which was published in 1998 in the Journal of Hydrology (Wilks, 1998) and has served as the basis for a larger number of multi-site precipitation generators. The other two are new developments and called GWEX-1D and GWEX-3D. While GWEX-1D is simulating at daily scale, GWEX-3D simulates precipitation first at three days scale and then uses disaggregation based on observations to achieve daily precipitation as output. The three models are then compared with respect to different temporal and spatial

statistics.

2. General comments

- The title of the paper is a bit misleading. The three models may be used for the spatial assessment of floods and hydrological modelling is mentioned not only in the title, but also throughout the manuscript. However, the precipitation models are not applied in an impact assessment in this study and for this reason in my eyes the title should solely contain the comparison of three precipitation models. It is a bit irritating that the authors refer to the importance of several aspects of the precipitation model performance whose importance is not really demonstrated.

- The names of the new precipitation models are a bit misleading. First, "1D" and "3D" give the impression of any type of one- and three-dimensional simulation methodology. However, they represent days ("D"). I would rename the models into something more suitable.

- As far as I understand from the paper, the new GEWX models are actually "Wilks models" but with a new method to simulate the precipitation amounts (using temporally and spatially correlated random numbers from an autoregressive process and using a Student copula for the spatial component). I think this should be stated as such in the paper as the manuscript presents the new models more as a revolution rather than an evolution. So one of the first sentences could be that the paper deals with two modifications of the Wilks approach.

- The motivation behind the study and for the new model developments is the impact assessment. However, without the same the reader will not be able to really understand the sensitivity of certain statistics in regard to the assessment of extreme floods. I think the importance of some of the statistical metrics should be explained in more detail referring to the area of their application, and proof must be given of their relevance. Other literature in such a study (complex mountain region) is not very convincing to me.

- The abstract is incomplete and must be much more detailed and specific. What is an “event”? What is “large”? What are “recent advances”? The abstract should mention the Wilks model, the two new models (maybe also a short sentence how they work) and the basic outcomes of the study.

- The Wilks model could likewise be applied with E-GPD distributions for precipitation intensities and Markov chains of the order 4. That is, revealed weaknesses of the Wilks model can easily be addressed. I recommend adapting the Wilks approach for a more objective comparison. The original Wilks approach is not a given and was just one application for a specific dataset in the US and in my eyes it should always be revised for other study areas and climates.

- For flood modelling, the lagged cross correlations (see Wilks 1998, page 183) can be very important as they represent the progression of weather systems across the study area. Especially at larger scales the progression of weather events may be important. I strongly recommend plotting these statistics for all three models.

- The autocorrelation of precipitation is addressed by MAR(1) models in the GWEX models. I would recommend plots for the autocorrelation of the precipitation intensities for some sites to see potential differences in their performance.

3. Specific comments

- Line 8. I think there is a language issue.

- Line 10. Not only conceptual models. There are more recent studies for coupling WGs with impact models.

- Page 1 bottom/ Page 2 top: In my eyes the classification is not fully correct. All these models are multi-site models. Also resampling methods are multi-site models. I recommend a more suitable classification even though I admit that the variety of the existing multi-site models makes a clear classification more and more difficult (also the authors combined parametric and non-parametric techniques).

- Page 4, Line 8. Are Thiessen polygons suitable for such a complex mountainous study region?

- Page 7 “Marginal distributions”. Can any proof be given that the more complex fitting of a combined distribution is really significantly better for the simulation of the extremes in this region? Also here, the most prominent argument is other literature.

- Page 9, top of the page. If the Gaussian copula is not suitable for simulating spatially dependent extremes but the Student copula is, this could be demonstrated. I am thinking of readers who want to build the code but are not experts in copulas and want to understand the significance.

- Page 9 bottom. Why are Markov chains of the order 4 used? Have there been statistical tests or sensitivity studies to underline this decision? Later on, some remarks are given on the simulation of short dry spells, but I think this should be addressed in a more structured way.

- Page 11, Table 11 (and figures). Red and green are not suitable for figures, please change the colours as some people cannot read them otherwise (https://www.nature.com/nature/journal/v510/n7505/full/510340e.html).

- Page 12, Line 28. I guess it is very difficult to say if an extreme precipitation amount is unrealistic or not as long as they are physically possible?

- Page 16, Line 18-20. If the order of the Markov chain is the issue for short dry spells, this can be easily adapted by using the same order in the original Wilks approach. What was the argument for using the first order Markov chains in the Wilks model? (see comment above)

- Page 21, Line 8-9. Please explain the seasonal differences with explicit reference to the study area and its climatology for better understanding.

- Page 22, section 4.4. and figure 10. To me, the performance looks fair for all three models. The main difference is the simulation of higher extremes with the GEWX

models. The authors mention the difference but it needs further discussion. Also, how can we know that the extremes of one method are more realistic than from another? While we know little about the validity of the simulated extremes, they may have a big impact on simulated floods, especially in small catchments (but as mentioned before, this is not examined in the paper).

- Page 26 Line 10-13. It is not surprising that the non-parametric disaggregation leads to a better performance. I understand its strengths but it may likewise be a limiting factor in generating extremes.

- Page 20, first line 2-9. As already mentioned, I see the motivation behind the study (and it is generally a good one). But without any proof that the differences in the performance of the three precipitation models really have a significant impact on the simulation results of hydrological extremes (also considering all the uncertainties in hydrological models), the significance of the research outcomes remain questionable.

- Page 29, Line 21-22. Please explain why, see comments above.

- Page 29, Line 27-28. The issue of larger spatial scales could be addressed by running more analyses at smaller scales. So the key motivation of the study is probably to examine large flood events and their spatial dependences? If so, this should be better explained. But again, without really simulating the floods throughout different scales the arguments for a particular precipitation model choice is questionable.

- Page 30. Is the underestimation of the inter-annual variability such a big issue in Switzerland and for flood modelling? I would assume it is more an issue in more arid regions and for example agricultural studies? Some more remarks on the relevance in Switzerland and floods in general would be useful.

4. Summary of review

- The abstract needs revision and must be more detailed (see general comments).

- The introduction is not very well structured. The arguments for the construction of

the new precipitation methodologies are mainly based on other literature and reasoning. The context of the paper should (i) either be revised (comparison of precipitation models) or (ii) proof must be given of the advantages using the new models by really coupling them with a hydrological model and examining the estimated flood events in the study region. I think it is the key weak point of the paper: reference is given to an application, which is not really done. Also, the title and abstract are a bit misleading and the reader may expect a flood modelling study and thus more than what has been presented.

- For the three different precipitation models, I would recommend a flow chart with the Wilks model as the central component and then the adaptations that have been done. This makes it easier for the reader to understand all models and what has been changed.

- Although the level of English is very good, some (minor) mistakes can be found in the manuscript and a native speaker should probably have a final look before resubmission.

5. Literature

Wilks, D. S.: Multisite generalization of a daily stochastic precipitation generation model, Journal of Hydrology, 210, 178–191, 1998.

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