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## **Comment on hess-2021-576**

Anonymous Referee #2

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Referee comment on "Stochastic Generation of Multisite Streamflow for Future Water Resources Vulnerability Assessments: Application over South Korea" by Sukwang Ji and Kuk-Hyun Ahn, Hydrol. Earth Syst. Sci. Discuss.,  
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## **Review for manuscript "Stochastic generation of multisite streamflow for future water resources vulnerability assessments: application over South Korea"**

**Authors:** Sukwang Ji and Kuk-Hyun Ahn

**Journal:** Hydrology and Earth System Sciences

### **Summary**

The authors introduce a model for the simulation of multi-site streamflow under non-stationary conditions. The model relies on a three-step approach which combines an annual with a daily simulation approach. The approach is evaluated for 12 catchments in South Korea and applied to assess reservoir system performance under future temperature scenarios.

### **General remarks**

The study introduces a simulation approach for the simulation of multi-site steamflow under non-stationary conditions using a temperature covariate to simulate annual

streamflow. However, I think that the paper lacks a clear problem statement and research questions and that the method description needs to more clearly introduce the individual modeling steps, several among which seem unclear to me. In addition, the model seems to not capture extreme events very well, which needs to be fixed.

## Major points

- Title: a vulnerability assessment does not seem to be part of the analysis and I therefore suggest to rephrase the title.
- The introduction introduces the new modelling approach but it is unclear what actual research gap the study addresses. It is important to clearly state the actual research questions.
- The first part of the introduction is a bit heavy on highlighting the disadvantages of GCMs even though streamflow is usually simulated using downscaled data as an input to a hydrological model. I think that l.62-69 and l.77-81 are not needed and that instead a short statement about different uncertainty sources involved in hydrological modeling of future streamflow is sufficient (see e.g. Clark et al. 2016; <https://link.springer.com/content/pdf/10.1007/s40641-016-0034-x.pdf>) before highlighting the need for computationally more efficient alternatives such as stochastic modelling. Instead, a more comprehensive introduction to different existing modelling approaches and their advantages/disadvantages should be provided.
- The methods section needs a lot of clarification, as many steps remain unclear to me. For example:
  - how are the regions defined for averaging (l.172)
  - how exactly is the covariate computed (l.180)?
  - how was the order of the AR model determined (l. 192)?
  - how exactly are the different SOM nodes selected (are these the same as classes, l.200-209)?
  - how do you ensure that the temporal dependence between spatial patterns is retained (l. 211-213)?
  - why six spatial patterns (l. 222)
  - what do you bootstrap from (l. 226)?
  - it remains unclear why the copula is needed (l. 243-254). To extrapolate, would it not be sufficient to use an extreme value distribution?
  - what is a 'pivot' variable (l.260)?
  - the coupling step (l. 280-284) also needs clarification.
  - how does this bias correction procedure work (l.322)?
- The model evaluation shows that extreme values are not well captured (Figure 6). This problem might be resolved by using a 3-parameter extreme value distribution instead of the Gamma distribution to model streamflow. Statements such as the one on l.415-416 or the one on l. 423 need to acknowledge the lack of performance in terms of extreme flows.
- The reservoir performance analysis (l. 505-512) comes as a surprise. This aim is neither mentioned in the introduction nor is the approach described in the methods section. What is the goal behind this part of the analysis?
- Conclusions: Based on the results presented in Figure 6, I disagree with the statement: 'Second, compared to climate model-based projections, our simulated streamflows properly reproduce the primary characteristics observed in historical records'. There are two problems with this statement: first, the paper does not show any results for climate model-based projections and second, the model performance is rather bad in terms of extreme events. The discussion section should critically reflect this last issue.

- A discussion section should discuss the generalizability of the approach to other regions and larger datasets as well as the limitations of the approach. That is the fact that extremes are not simulated very well.
- Careful language editing is recommended to improve the reading flow.
- The figure design could be improved. Figure 1: indicate different workflow steps and better establish link to text. Figure 4: unit of legend missing, Figure 5: legend missing, Figure 6: legend missing, Figure 7: legends missing for upper panels, Figure 8: legend missing, Figure 11: legend missing.
- Figure 12: only include this analysis if problem is part of introduction and related to one of the research questions to be phrased.

### **Minor points**

- l. 128-131: statement is not necessarily true in all climate zones.
- l. 306-308: rather belongs to introduction
- l. 565: 'precipitation' -> 'discharge'?