Hydrology and Earth System Sciences Discussions

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Comment on hess-2021-576

Anonymous Referee #1

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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A small note for everyone before we get to the nitty-gritty. This paper was submitted to JoH previously and got a rejection. I was the one who requested it. As luck (let's call it luck) would have it, the paper was submitted to HESS and came to me. Again. What are chances? Are there so few people in this field? Anyway, I dug through my previous reviews and compared this manuscript to the one that was submitted to JoH. To my surprise they are exactly the same. I had heard about authors submitting to other journals upon rejection but never experienced it firsthand. I must say the feeling is not nice mainly because all the effort that I put the last time was a waste. Nobody cares. What am I to do? Submitting my old review is what I do. It's only fair. Cheers.

General comments:

I regret to say that I am very unsatisfied with the contents of this paper. The various approaches and assumptions used here are not backed up by proper evidence. Vine-copula construction is very flimsy. Using temperature as a covariate to predict discharge is not tested properly before used for simulations. The results show considerable bias almost everywhere. Finally, I do not find the results of this study to be of any importance for this journal given how much is done already on the effects of climate change on the future. Regardless, the constructions used here are unacceptable, in my opinion. The data points are too few. Complex spatial correlations require more than a regional average and a transition matrix for representing them.

I was going for major revisions but given the results of this study, even after the

revisions, we won't be seeing anything useful. I mean, we have been hearing about the effects of climate change for what? The past 30 years? Tell us something that we don't know. Hence, a rejection. My comments will seem harsh. That is because I felt that I was wasting time writing this review. Seriously, much more effort should be put in to preparing a good manuscript. The whole time it seemed like methods from here and there were stitched together without giving any thought to the outputs and their meaning. I (almost) never reject a paper but this one took the cake (again).

Specific comments:

L180: Just out of curiosity, do we know for sure that components can be modeled using an AR process? Shouldn't there be a test of some sorts? AR processes are useful if the dependence is normally distributed i.e., follows a Gaussian copula. And I have yet to see a natural variable that is anything remotely close to Gaussian.

L191-194: I don't know about Ahn (2020), but daily discharges have long memories i.e., more than 1 day. In my experience, mesoscale catchments can have memories of more than 1 week (around two weeks is the norm and the annual and seasonal cycles are always there). This should be taken in to account. BUT, if first order is justified then please show it. Citing another study as a justification (in this case) is a bit weak in my opinion. It could be that the decomposed series have no/short memories but we don't know this for sure.

L242-277: All of this would be correct if calibration and validation on independent datasets showed good results. Which are not mentioned this far in the text. According to Joe (2014), we can do a range of complex dependencies, but how do you know if the construction is correct? How many copulas were tried here?

L333-336: Is the South-Korean climate similar to that of Australia? Based on Fig. 3 alone, I won't call it a strong relationship. For example, for a scaled Tmax of zero we can have a range from about 500 to 1200 mm. Quite a range, I think. And this is the best case?

L402-406: For Node (1,2), there is just one value that is mostly influencing the trend. I would hold the judgment that the lows have significantly changed for this node. Also, the sample size is extremely small to make the call that flows are trending downward. Do we know for sure that the stream flows are free from anthropogenic influences? I have heard that South Korea is very active in water-resources-related projects.

L408-420: Dear Lord. Fig. 6 looks bad. Pardon the nit-picking but the standard deviation, skewness and maxima are way off. What was the point of fitting the distribution when the simulations never ever went out of the observed bounds anyway? It would be okay if the simulations extended to both sides of the 1-1 line but always being on the same side represents bias. And a big one in this case. How could one say that "overall, the results describe that the stochastic simulations properly represent…including daily average". The daily average looks acceptable but the higher-order statistics are off. The phrase "although there are some underestimations" took a whole new meaning here. The simulations underestimate the skewness massively and yes, there is bias. That has to be corrected.

L422-431: Coming to Fig. 7, the skewness seems to be doing its own thing here as it is always the same regardless of the basin considered. Except for the mean, the rest looks pretty biased too. The lower figure is acceptable.

L433-443: Based on what is considered acceptable in this paper, I don't know what to believe now. Fig 8. shows consistent bias. Some of the cross-correlations look pretty good but many have bias in Fig. S1.

L445-458: Why is the difference of medians considered? Discharge is (to a large degree) exponentially distributed that means that the median will be a very low value as compared to the rest that make up the larger sum of the runoff. I don't know what to make of this figure. The authors could at least consider total volume error. As an example, consider rainfall. Median rainfall is going to be very close to zero but that is not something that brings river flow, does it? It is the upper tail. Same thing for discharge but to a lesser degree. Compare the Lorenz curves of discharges, maybe?

L460-476: Regarding Fig. 10 (right), the chosen sample is way too small to make a solid judgment. Also, there is no proper explanation about why the partial model did not show a significant change? Regardless, the sample is too small.

L528-529: The approach used here is not compared to any other method, then how can one say if it is good or bad? How do we know that a simpler approach may have sufficed in the first place? Or any other existing one? I wonder now, how do you see non-stationarity in a series? I know about the long-term waves but how big are those compared to the rest of the time series? I didn't see any figure for that. It was just assumed that non-stationarity is there. It is always there, I guess.

L530: I don't find the simulation results to be "proper" at all.

L535-552: The new approach proposed is tested here on one region only. The results are not impressive by any means and no comparisons or validations are provided that show a

decisive improvement. Just claiming that such an approach is not used before by combining various methods from here and there is rather weak. If I had a dollar for every time I heard this justification, I would have a few hundred dollars. Using such a short period of observation as input is not enough to account for non-stationarity. There are wet and dry years.

L554-568: Ah, shift the burden of validation to the future generations. Let them deal with the matter of the approach being right or wrong. But hey, at least we have a paper. When you already know the short coming that the Gamma distribution leads to an underestimation then why wasn't a better one searched for? You could try the Pareto distribution. Nothing too complicated to test. The results of fitted distributions are also not shown anywhere.

L570-590: Instead of using higher temporal resolution data, I am more concerned about the length of the time period. 23 or 8 years is too short to predict long-term future.