Review comments on "A coupled stochastic rainfall-evapotranspiration model for hydrological impact analysis" by Pham et al. 2017 submitted to HESSD

Mojtaba Sadegh

This is a nice study that uses copula-based approaches to stochastically generate mutually dependent rainfall and evapotranspiration forcing for rainfall runoff models. This could be used for design purposes where observation is sparse or missing. However, this approach does not seem to be working properly for the extreme events (which are needed for design purposes). Acknowledging this fact, the study is valuable for the areas with no observation.

Overall, paper is well written and well structured. I have some comments (most of them major) that could potentially improve the quality of this paper.

1. Line 46: Authors use stochastic process models to generate precipitation series. My question is:
   How do stochastic process models handle changing characteristics of precipitation? Several studies have shown, for parts of the world, that rainfall events are shrinking in time and expanding in amplitude. Also there is a temporal shift in rainfall events in some parts of the world, let alone the changes in the distribution of rainfall/snow. Addressing these issues could be helpful.

2. Lines 91-94: I don't understand how the number of stochastically generated forcing data could influence the uncertainty of the rainfall-runoff model's response. Uncertainty is a characteristic of the forcing data (let's neglect the modeling uncertainties for now), not the number of generated time series. So if you find a time series that fit your runoff extremes well, this is just a random phenomenon. This cannot be the basis for prediction, as we can't determine the best forcing for future, and need to rely on the ensemble of forcing data.

3. Lines 95-96: Section 2 should precede section 3!

4. I am confused about how sections 2.1 and 2.2 are connected. Historical record of climate forcing are obtained for Brussels, and RR model is calibrated for the Grote Nete catchment. How do you use a model calibrated against one watershed, to predict runoff at another watershed?
   Moreover, 1 year of data for evaluation is not enough. You will need a couple of years to ensure calibrated model can capture different aspects of a catchment.

5. Section 2.3: Copulas characterize dependence structure of different variables. This means there should be a dependence structure. Did you quantify the correlation between evaporation, temperature, and precip? If so, is it significant? At what temporal scale?

My understanding is that you perform your analysis at daily scale, and I fear the correlation might not be significant at the daily scale.

6. Line 150: bivariate -- > It could be multivariate

7. Line 152: I would reference to Joe 1997 too. Joe and Nelsen both played an important role in introducing copula to the scientific community.

8. Lines 171-173: I agree that vine copulas are very flexible, but it comes at a price! A model with 4 degrees of freedom is more flexible than a competitor with 2! However, usually there is not enough information to constrain all parameters. The copula literature usually does not address the parameter uncertainties, and so they neglect the identifiability of parameters. I would address this predicament here. For more info, refer to figure 6 of:
   Sadegh, M., E. Ragno, and A. AghaKouchak (2017), Multivariate Copula Analysis Toolbox (MvCAT): Describing dependence and underlying uncertainty using a Bayesian framework, Water Resour. Res., 53, doi:10.1002/2016WR020242.
   Link: http://onlinelibrary.wiley.com/doi/10.1002/2016WR020242/full

9. Line 191: As a minor issue, when someone talks about a 3-dimensional model, I expect the model to have three parameters. When someone talk about trivariate model, I expect a multivariate model that associates three variables.

10. Line 203: How did you construct the marginal distribution? Empirical? Fitted distribution?

11. Eq. 3: how did you calculate inverse of the vine copula? Analytical or numerical?

12. Line 253: pvalue larger than 0.05 or smaller?!

13. Section 2.4: How did you calibrate the modified Bartlett–Lewis (MBL) model, given the stochastic nature of precipitation prediction models? With stochastic models, usually summary statistics of data and simulation are compared, rather than original time series. For this purpose, approximate Bayesian computation is a great framework.

14. Lines 338-344: I cannot disagree more! Forcing and model uncertainties are intertwined, and interact in a nonlinear manner. It is not as simple as you explained. You cannot simply use a RR model calibrated for one watershed to simulate runoff at another watershed! Tens (Hundreds) of papers are available on the regionalization topic, not many of them really provided a sound ground for transferring model parameters from one watershed to another! Worse is that authors assume this modeling uncertainty does not interact with the forcing uncertainty.

15. Figure 22: 22 figures? Is that many figures really necessary when most of them don't provide any new info?

16. Lines 476-478: I have a hard time accepting this claim. If you generate a much longer synthetic (stochastic) forcing, then let's say predictions at a 100 years return period level improves. I accept this. But I cannot accept the general comment that longer fording data reduces overall uncertainties. What if I had to estimate a 500 years return period flow?