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Hydrology and Earth System Sciences Discussions

Interactive comment on "Exploring the physical controls of regional patterns of flow duration curves – Part 2: Role of seasonality and associated process controls" *by* S. Ye et al.

Anonymous Referee #2

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1. The paper of Ye et al. applies different models to simulate the regime curves of 197 catchments located in CONUS. The model development process follows a top-down approach, where model complexity is introduced in response to model failures. The objective of the paper is to associate different model structures to different catchments, and investigate and interpret the regional patterns that may arise.

2. I am favourable to this work and I think that the research topic is interesting. However, I have the feeling that the work could be much better structured and refined, and that the Authors should spend some more time to reorganize their material. The paper

is lengthy, 14 figures are too many for a scientific paper, and the presentation is at times chaotic. Reading this paper and browsing through the other 3 papers of the series, I recognize that the Authors have gone through an amazing body of work, which I fully respect. However, I had the feeling that the Authors did not want to discard anything of the work they had done, and that the process of synthesis and refinement has been overlooked.

3. Regarding the methodology of the paper, the Authors have used regime curves for model calibration. Regime curves synthesize some aspect of the catchment response, at the expense of a loss of information. The question is why using data aggregates that are less informative than the data themselves. Indeed the Authors themselves state in section 4.6 that "a model focused on predicting the regime curves only cannot be expected to predict well the high and low flows". In my opinion, the Authors should have used the data series as they are for model calibration, and then evaluated the models independently on regime curves and flow duration curves.

4. The mapping of model structure to catchments is unclear. How was this done? Which metrics have been used? I suspect the most complex model was fitting best all catchments. What made them prefer a lower complexity model for certain catchments?

5. Absence of validation. I think the Authors should split the data between calibration and validation, and see if results hold.

6. No presentation of uncertainty estimates. As the Authors have used Bayesian methods, they could present uncertainty estimates of model parameters and predictions.

7. Introduction: I think the authors should state clearly that the focus of this paper is not the flow duration curve, but the regime curve. The first sentence of the introduction is misleading. The FDC and regime curves are 2 different signatures, and cannot be transformed one in another.

8. Methodology: the Authors distinguish between "satisfactory" and "non-satisfactory"

models based on an acceptance threshold, which is MSE=0.53. Catchments with performance < .53 where left out of the analysis. Clearly this threshold is quite important in determining the outcomes of this study. How was it determined? Similarly, in motivating model improvements, the Authors refer to “non satisfactory” model performance. How was this assessed? Where similar threshold adopted? More generally, I think these types of “absolute” thresholds are quite dangerous, becasuse model performance can be affected by many aspects other than model structure, such as data uncertainty. The Authors should think of a better way of motivating model improvement. Perhaps the performance relative to the most complex model could be a better alternative.

9. Model description: note that equations 5, 8 and, line 7 of page 7044 are dimensionally wrong (they equal storages to fluxes).

10. Model description: please mention the numerical methods used to solve model equations (e.g. explicit Euler?)

11. Parameter calibration: I did not fully understand why the Bayesian approach was used if no uncertainty estimates of model parameters and model predictions are shown. The purpose of MCMC method is the evaluation of uncertainties, not calibration, for which much more efficient methods can be used. To my understanding, only optimal values of model parameters and predictions were used.

12. Equation 14: the Authors should specify what N denotes. If N(z|mean,var) is the pdf of a Gaussian deviate z, the equation should be corrected accordingly.

13. Page 7050. It is not necessary to explain how the MCMC works.

14. Progression of model development. Considering the results presented by the Authors, I found that the only model modification that made a significant difference was the inclusion of snowmelt. The inclusion of other processes did not provide a considerable improvement. Is this a correct interpretation, and if yes, how can these model improvements be justified?

15. Regional distribution of model parameters. Can the Authors show uncertainty estimates of model parameters? Where model parameters reasonably constrained through their calibration on regime curves?

16. Figures 1 and 2. It strikes that the pattern of PET is always so smooth. How was this calculated?

17. Most figures could be improved. Figure 3 and 4: maybe use some more specific software for making these figures, represent reservoirs as reservoirs, and have different colour codes for model parameters, states and fluxes?

18. Figures 7 and 8: model improvements are not apparent. Maybe use different metrics, and also show model improvements in other catchments. A different way of summarizing results is probably necessary.

19. Figures 11 and 12: the attribution of model structures to different catchments is not clear to me. It seems a bit speculative if not properly justified.

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