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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Process-Based Flood Frequency Analysis in an Agricultural Watershed Exhibiting Nonstationary Flood Seasonality" *by* Guo Yu et al.

Anonymous Referee #1

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Comments on "Process-based flood frequency analysis in an agricultural watershed exhibiting nonstationary flood seasonality", by Yu et al., submitted to HESS.

The authors explore the utility of hydrological simulations driven by stochastically transposed rainfall fields in deriving flood frequency over a watershed that experiences nonstationarities. Their results highlight the importance of considering changing flood seasonality in flood frequency analysis. While process-based approaches have a fair amount of advantages, their shortcomings are also quite obvious, for instance, mode uncertainty in both parameters and model structure, representation of synthetic rainfall scenarios, etc. As a hydrologist, I would still favor statistical approaches if the gaug-

ing record is good (as is the case in this paper). This being said, I would suggest the authors focus on explaining the importance of changing flood seasonality in flood frequency, but rather demonstrating the superiority of process-based approaches to other FFA methods (which is not, as far as I can see). My specific comments are listed below.

Specific comments:

1. An important part is missing from the present paper is model validation. Evidence needs to be explicitly presented to show the capability of long-term model simulations in capturing, for instance, flood seasonality, as well as other features (distribution of annual maximum discharge). This can be done by adding simulation results into Figure 3b and Figure 5a. The authors show a larger frequency of floods during post-summer season in their simulations, could this be possibly related to the positive model biases in representing rainfall-runoff processes during this season? The reliability of process-based approaches in FFA builds on decent model simulations. The authors should spend additional efforts in demonstrating this in the paper. This can be done by providing a quantitative assessment of the model performance. Another question about the simulation, how is channel flow represented/considered in the analyses. Antecedent streamflow in the channels can be an important element in representing antecedent watershed wetness, in addition to soil moisture, that plays a role in streamflow simulation.

2. The representation of synthetic rainfall fields is another key in process-based FFA approaches. The authors mentioned that they chose 'most intense rainfall events' within a prescribed domain. How exactly do they define "most intense rainfall events"? Please explain. The authors use the word "realistic" throughout the paper which is inappropriate or miss-leading. They are using synthetic rainfall fields, even though based on real storm events. Please modify.

3. The authors show flood frequency estimates in modern times using Stage IV rainfall

fields, and the results match well with gauging records. How about the performance of CPC rainfall in estimating flood frequency?

4. An interesting finding in the paper is described in P17 Line 15-20, but needs to be rephrased. We can see summer floods dominate the upper tail of flood frequency in this region, even though they do not occur as frequent as spring floods. The distribution derived from gauging records is still the 'truth' anyway. Under-representation of summer floods is a pretty common feature of flood peak distributions in the US. I would suggest the authors to provide a brief diagnostic summary of the most extreme flood events in this region.

5. The authors compared simulation results using model with and without snow module, and suggest in the paper that "the modeler must either have sufficient data to diagnose such issues or have sufficient prior knowledge." (P18 Line 14). I would believe a snow module should be needed in simulation hydrological regimes in this region (dominant spring floods in flood frequency). We cannot simply opt out the snow module by simply checking the simulation. What prior knowledge do the authors have? I would suggest the authors to examine the observed snow climatology over this region, and more ideally, carry out detailed diagnostic analyses of flood agents in this region.

6. P22 Line5-7, it is not true that conventional statistical FFA methods underestimate flood frequency. At this stage, I would still believe statistical estimates are the ground truth, which enables the evaluation of the process-based approach. The authors do not show updated Bulletin 17B curves using the 1990-2016 flood records in Figure 5, which I would suggest to update. As I have mentioned earlier in general comments, it is not wise for the authors to demonstrate the dominating superiority of process-based FFA approaches in this paper, at least for this region. Process-based approach, as presented in this paper (hydrological model + SST), can be highly recommended in poorly gauged watersheds. For poorly-gauged watersheds, however, another issue arises as how to obtain a large ensemble of antecedent watershed wetness conditions used in event-based model simulations. The authors need to provide a discussion

about both pros and cons of the proposed approach.

I have a couple of additional comments on word expressions, paragraph organizations, etc., but they can wait till the second round of review. The paper can be a worthwhile contribution to the literature subject to major revisions.

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