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Comment on hess-2021-49

Daniel Wright (Referee)

Referee comment on "Identifying sensitivities in flood frequency analyses using a stochastic hydrologic modeling system" by Andrew J. Newman et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-49-RC2, 2021

Review of HESS-2021-49: "Identifying Sensitivities in Flood Frequency Analyses using a Stochastic Hydrologic Modeling System" by Newman et al.

The authors present a sensitivity study examining the relatively contributions to flood quantiles from precipitation, initial conditions, model structure and parameters, and meteorological sequencing for two watersheds in the western US. The results are interesting and useful, while the manuscript is well-written. Like reviewer #1, I think that only minor revisions are needed. Similarly, like reviewer #1, I think occasionally found it difficult to understand exactly what was done or why. I'll point out those issues that I noticed, but I agree with reviewer #1 that an overall workflow diagram might be helpful if done with care.

Specific comments:

I agree with the authors' discussion of AEP equality assumptions, but the potential problems don't end there. Even the assumption that precipitation annual maxima—which are the values used here and in most studies—are the drivers of streamflow annual maxima is not really correct. In Yu et al. (2019; specifically, Table 3 in that paper), we found that you need to get into 200+ year return periods before that assumption is really reliable, at least for the midsized midwestern watershed we looked at. Clearly, this is less of an issue for really big floods.

L68-70: It seems like some element is missing from this sentence. "higher sensitivity…" higher than what?

Section 3.1: I think you need to provide more explanation on how you used the total

probability theorem. I *think* I understand what you did, but the reader shouldn't have to guess. Out of curiosity, I'm wondering if that approach would be valid when using distributed models. With lumped models (which I assume the authors are using here, but I'm not actually sure; see below), a bigger rainfall event combined with a higher IC will result in a higher peak than a smaller rainfall combined with a drier IC. But with distributed models, that is only true in general but not universally due to routing effects—I've seen cases where this isn't.

Section 3.4: It would have been nice to know how important this assumption of picking a few (high) ICs is, as opposed to letting the ICs vary more widely. My particular concern is that to some degree or another, your rainfall quantiles are probably based in part on some events (probably some big ones!) that are outside of this Feb-July (Altus) and Apr-June (Island Park) periods. The Colorado 2013 floods are a good example of this. I suspect that there is some degree of misrepresentation of the relative importance of ICs and precipitation for this reason.

Section 3.5: I found the explanation of spatial precipitation structure to be unclear-both how it was done, and why it was done. In the latter case, my confusion stems from the lack of description of the models' spatial discretization (or maybe I missed that somewhere).

Section 3.6: I found this section difficult to follow, and didn't totally understand what was being done.

Section 3.7: While neither Peleg et al. (2017) for I (in Zhu et al. 2018) examined model structure, we did use ANOVA (in Zhu et al.) or something like ANOVA (in Peleg et al.) to examine the roles of other things (ICs, for one) in FFA. I won't be offended if you don't, but you may consider whether those prior studies' findings provide relevant contrasts with your work.

L304: Usage of "overrepresentation" is unclear.

L304 and more generally: given all the moving parts here, some section referencing would help, as well as a bit more precision with terminology. For example, "KGE interval metric-based calibration"-it took me a minute to figure out what you were talking about. You mean calibration based on peak flows, right? Furthermore, referring back to Section 3.3 (e.g. "(see Section 3.3)") would help the reader the track down the relevant details they might have missed or forgotten. This section referencing would help in a number of other places too.

Section 5: It would be nice to know if the "shapes" of the flood frequency curves are driven by the shapes of the precip frequency curves, which aren't shown.

L365 and around there: I struggled with this paragraph, in part because I didn't understand the descriptions in Section 3.6. Also, this is another good place to refer back to earlier sections/descriptions.

L376: "Dry to historical meteorological sequence"-I found this wording confusing

L390: You could refer back to the first mention that you're analyzing different streamflow timescales

L428: consider replacing "across" with "between"

References:

Yu, G., D. B. Wright, Z. Zhu, C. Smith, and K. D. Holman. "Process-Based Flood Frequency Analysis in an Agricultural Watershed Exhibiting Nonstationary Flood Seasonality." Hydrol. Earth Syst. Sci. 23, no. 5 (May 7, 2019): 2225–43. https://doi.org/10.5194/hess-23-2225-2019.

Zhu, Zhihua, Daniel B. Wright, and Guo Yu. "The Impact of Rainfall Spaceâ□□Time Structure in Flood Frequency Analysis." Water Resources Research 54, no. 11 (2018): 8983–98. https://doi.org/10.1029/2018WR023550.

Peleg, N., F. Blumensaat, P. Molnar, S. Fatichi, and P. Burlando. "Partitioning the Impacts of Spatial and Climatological Rainfall Variability in Urban Drainage Modeling." Hydrol. Earth Syst. Sci. 21, no. 3 (March 14, 2017): 1559–72. https://doi.org/10.5194/hess-21-1559-2017.