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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Assessing the impacts of reservoirs on the downstream flood frequency by coupling the effect of the scheduling-related multivariate rainfall into an indicator of reservoir effects" *by* Bin Xiong et al.

**Anonymous Referee #2**

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General Comments: The manuscript presents downstream flood frequency analysis framework using the annual maximum daily flows (AMDF). Joint cumulative probability of multiple rainfall variables (maximum, intensity, volume and timing) are considered as multiday rainfall input (MRI) and employed in C-vine copula model. Flood frequency model is defined by nonstationary generalized extreme value (NGEV) distribution model including uncertainty deliberation with Bayesian approach. Rainfall-reservoir composite index (RRCI) is proposed and used to quantify the reservoir effects as covariate for expression of distribution parameters. According to the different



metrics, the results of the proposed method outperforms typical reservoir index (RI) based flood frequency model which only accounts reservoir capacity and mean annual runoff. I believe the study is quite interesting for the readership of the journal and contributing to better modeling of downstream flood peak mechanism. The model results give reasonable outcomes and can be useful for regions where large reservoirs are located. The manuscript deserves publication after a major revision considering my below comments: - Language needs some refinements before publication. Also, there are some typos and repeated sentences, which make hard to follow and disturb the readability. It would be nice to revise the manuscript totally by dividing long sentences and eliminating the repeated ones. Same tense should be used (is or was) thought the text. - Studies dealing with downstream hydrograph alterations caused by dams are not discussed enough in the literature. - As stated in Lines 45-49, there are several factors for the generation of the floods. Authors focused on meteorological conditions, but also indicating the importance of hydrological conditions such as snow cover. The elevation range of the study area is quite wide (13 – 3493 m) and most upstream reservoirs (especially Ankang gauge) should be dominated by snowmelt. The response of the basin will be complex compared to lower altitude basins. There is not much information about the assessment of the snowmelt contribution of the catchments and their effects on operational decisions. It is also interesting to see that linear correlations between the timing variable of multivariate MRI and AMDF give lowest (almost zero) Pearson r for AK gauge in Figure 5. Would snowmelt be a reason for this? If this is the case, maybe RRCI is not enough to explain downstream peak floods for the regions where reservoirs fed by snowmelt? Temperature data can also be effective to estimate flood peaks in such cases. I believe this situation should be clarified. - In Data Section, the explanation of reservoir data is based on only their capacities. There is not much information how they are operated. For example, for what purposes they are operated, or how their reservoir pools are divided (flood control, conservation, dead storage etc.)? - It is not clear why inverse distance weighting (IDW) is selected for areal distribution of the rainfall records. The catchments are large and elevation

ranges in between 13-3493 m, so that this method may not be representative especially for mountainous regions. - Maybe it would be better to call “downstream flood frequency analysis” rather than “flood frequency analysis” throughout the manuscript? - Variation of RI and RRCI are quite different for AK gauge station in Figure 6. Please state the reason - Uncertainty of flood estimates are greater in AK stations (Figure 8) compared to the others. The reason should be explained. - Discussion section is comparatively short to conclusion part. In general the paper describes a usable approach but the main weakness is insufficient discussion of the available results. I mean, it is stated that the downstream flood regime should be altered by upstream reservoirs and the magnitude of flood peaks are reduced due to the storage capacity of them. This is expected in such a reservoir system by analyzing long period AMDF values (see Figure 7, observed AMDF). Rather, the author should elaborately clarify GEV model results in Discussion part. Main results should be given under discussion, and conclusion should be briefly summarize them. Considering these, I guess these two sections should be totally revised. - Figure and tables are appropriate. However, I have some doubts about the usefulness of Figure 9 to illustrate the reservoir effects on flood risk. It is not combining the results of the frequency model. It is not clear for what reason this figure stands for especially at the end of the result section. (I suggest removing this figure, as it is a bit confusing in terms of central theme of the paper). If authors would like to include it, I suggest them to re-organize its location through the manuscript and revise the descriptions to make it more clear (in Lines 387-395).

Specific comments: - There are too much abbreviation in the manuscript. Maybe a glossary would be useful for the readers. - Line 49, what is “nature extreme flow”? - Lines 50-52, what about the operational targets and other constraints? - Lines 52-54, requires more up-to-date references. - Lines 76-78, even a small reservoir could be very complex to derive operational strategies and a lot of detailed information might be required. I am not sure about this classification. Please consider revising this part. - Line 96, what type of uncertainty? - Line 84, which “previous studies”? - Line 108, it is a bit vague what do you mean by “more accurate effects on reservoirs?” - Lines 115-117,



please refer to Bayesian method in the objectives. - Line 143, what do you mean by “more precise effects of reservoirs”? - Line 146, please briefly explain “multiday rainfall input”. - Lines 147-150, It is a bit confusing whether scheduling related multivariate (SRMR) and MRI are same or not? Could you give more detail for their explanations. - Line 155, why OR-joint exceedance probability is selected as measure function? - Line 158, what do you mean by “reservoir scheduling is more inflexible”? - Lines 170-172, selected four variable require more explanation. - Line 208, it is not clear “obeys nonstationary distribution”. Please revise. - Line 280-286. The sentence is too long and difficult to understand. Please separate and revise. - Line 301, please revise “Actually, although. . .” - Line 303-304, it is not clear what do you mean by “(e.g., special extreme MRI may limit or reduce the effects of the reservoir).” - Line 314-315, please describe and relate calculated Spearman correlations in the text, otherwise remove them. - Lines 338-339, please clarify “special rainfall events” - Lines 412-413, please mention future studies in Conclusion part, not under Discussion. - Line 429, it is not clear what do you mean by “some rare multivariate MRI still would produce lower values of RRCI than that of RI”. Please revise it.

Technical corrections: - Figure 1. The caption should be “The flowchart of nonstationary covariate-based flood frequency analysis with a rainfall-reservoir composite index (RRCI) - Figure 7. In the caption, “thick blue” should be “thick blue line”. - Table 2. It would be better to not to duplicate “Dangjiangkou reservoir” and remove first row. The details should be given in the text only. - Line 26, please revise “of the previous study” - Line 35, please revise “What’s more” - Lines 62-63, please revise the sentence. - In Line 73, it is stated three model components but not clear which of them are ordered since only two are given? - Line 76-78, too long sentence and hard to follow. Please revise it. - Line 119, please explain AMDF. - Line 114 and Line 120, “RRIC” should be “RRCI” - Line 115, “to calculate” should be replaced with “to develop” - Line 139, “the Eq. (1)” should be replaced with “Eq. (1)”

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