Comments on " Potential application of hydrological ensemble prediction in forecasting flood and its components over the Yarlung Zangbo River Basin, China"

The manuscript provides a study investigating the ensemble flood forecasting based on VIC macroscale hydrological model and ECMWF numerical weather prediction in a large snow-present basin located in Tibetan Plateau. The interesting part is that the authors involve an evaluation of impact of parameter uncertainty on streamflow components, to be exactly, the snow-induced and rainfall-induced components. Overall, the objective of the paper is clear and the results are interesting. However, several issues are unclear and/ or not sufficiently considered/ described. I firmly believe it deserves to be published in the Hydrology and Earth System Sciences subject to several moderate revisions. Please find my main comments and detailed comments below.

1. The main issue in this manuscript is that the authors need more efforts to outline the advantages and disadvantages by using multiple parameter sets (N-simulations in the manuscript). It seems that more attentions are focused on the influence of multiple parameter sets on entire streamflow. I would suggest the authors added more contents in discussion to clarify the merits of N-simulation for streamflow components forecasting and evaluating.

2. The main part in current manuscript is the evaluation of N-simulations for entire streamflow. In my opinion, the evaluation of entire streamflow is the foundation of evaluation of streamflow components. For there is no direct reference data for evaluation of streamflow components. The accuracy in entire streamflow is to some degree the evidence of model ability. This is the reason for usage of simulated streamflow components driven by observation as reference in components evaluation. I suggest the authors clarify this in the manuscript.

3. A brief data description used for hydrological modelling should be supplemented though it is similar for the previous publication.

4. I would suggest the authors to simplify the contents about hydrograph separation. Detailed description can be found in Li et al. (2017), and it is better for the authors to just list the differences for the method used in this manuscript with the original one.

5. There is no need to verify the forecasts with two reference data. Evaluation based on simulations is irrelevant to the objectives mentioned in introduction. I suggest omitting the related parts. If it remains, I would like to see it more strongly justified

6. I could understand most of the manuscript without difficulty, and the methods are well documented. However, there are many minor errors in spelling, grammar and English style that I have not corrected. I recommend that the authors have the manuscript proof-read by a native English speaker before publication.

Specific comments:

7. Page 6, Line 4: as snow and glacier melt water is considered together, term "meltwater" is better representative than "snowmelt".

8. Page 9, Line 31: "Lead times of 3, 5, 7, 10, 12 and 14 days" make it "Lead times of day 3, 5, 7, 10, 12 and 14"

9. Page 9, Line 32-33: "Generally, flood volumes tend to be better captured with the increase of "duration", especially for lead times from 7 day to 12 day" should be explained.

10. Page 10, Line 32: "It seems that N-simulations scheme works in poorly-calibrated regions." from what results the authors draw this conclusion.

11. Page 13, Lines 2: "but" make it "while"

12. Page 13, Lines 27: "We believe that the phenomenon captured by most of the parameter sets would be the most possible truth." Change it to "From the view of ensemble, the phenomenon captured by more parameter sets is regarded as the most possible occurrence."