Review for “Reducing Hydrological Uncertainty in Large Mountainous Basins:

The Role of Isotope, Snow Cover, and Glacier Dynamics in

Capturing Streamflow Seasonality”

General comments:

This manuscript focuses on evaluating the value of snow cover area, glacier mass balance, and isotopes in reducing uncertainty and equifinality of hydrological modeling in a large mountainous basin in the Tibetan Plateau. The Bayesian approach and GLUE method are adopted to investigate the research questions. The research topic aligns with the journal scope and the research findings are potentially useful for the readers. I have a few concerns regarding the modeling procedure, the details of the input data, and the interpretation of the results before the paper being accepted for publication.

Additionally, one thing I noticed here is that the time-series simulated and observed discharge does not have a y-axis (Fig.5), which is present on purpose due to data dissemination restrictions mentioned in the caption. However, this is not possible for readers to understand the model performance, and the magnitude of the simulated and observed discharge. A manuscript avoiding showing y-axis of time-series discharge plot in the results could potentially conflict with the basic principle of open science of HESS/Copernicus journals.

Specific comments:

Modeling perspective:

1. The subsurface is overly-simplified represented in the model. The subsurface flow generates from the model is composed of the subsurface lateral flow (“interflow”) in the unsaturated zone and the baseflow from groundwater to surface water in the saturated zone. These two subsurface flow components are simulated as a sum (L105 and Fig.1). It is thus not possible to conclude the role of groundwater in contributing to the streamflow and the groundwater-surface water interactions. The subsurface lateral flow can be high and not negligible in such large mountainous basin (>$2*10^5$ km^2). It is recommended to be cautious in interpreting and concluding the result regarding the baseflow. All mentioning of groundwater baseflow in the manuscript actually refer to the subsurface flow, i.e. the sum of both unsaturated and saturated zone, e.g. on L134, it is subsurface flow, but not baseflow. The presented modeling approach is not able to investigate groundwater alone.
2. Regarding the modeling, are the spatial zones delineated the same for both the surface and subsurface? (this could potentially fragment the aquifers located at the boundaries). Is the subsurface flow allowed to cross the delineated boundaries? The conceptualization of the subsurface processes in the model potentially limits the ability of the model for investigating the surface-subsurface interactions. The model limitation should be clearly discussed in Section 4.3.
3. There are 4 discharge stations, but only the results at Nuxia Station are presented. The results for the other stations should be presented in the Supplementary Information. The authors

should also clarify if the conclusions achieved at Nuxia Station are held the same as the other three stations.

4. Does glacier melt contribute to groundwater recharge? Or is it assumed that all glacier melt goes into streamflow? This assumption should be clear in the text as well.
5. The simple degree-day-factor methods are used to solve snowmelt and glacier melt. Glacier mass balance is estimated with a simple volume-area scaling factor approach. The limitations of these adopted simple approaches for solving snow and glacier melts should be discussed in terms of modeling limitations.
6. L213: how are the Pareto fronts defined? Please justify this threshold used to show the Pareto fronts in Figure 3 and the conclusions obtained from this result relating to this threshold.
7. L248-249: Why does the KKA shows a noticeable convergence, but not KKD? They both are parameters that control the subsurface runoff outflow rates. Please clarify this point.

Data perspective:

Limited information is provided on the input data of this study. This could hamper the readers to interpret the results.

1. L79: The four river gauging are only given by names and no other information and data are available. It is recommended to provide details on the coordinates and elevations of the four river gauging stations in this mountainous basin, also their observed periods, frequency, and measurement method. Any observations errors/failures in the winter low flow and high flow periods? These details are important to interpret the observed and simulated discharge.
2. Section 2.1: What is the modelling period? Please detail the start and end dates of the meteorological data sets and the modelling period. Also add details on which years of DEM, land use data, soil data, snow cover, and glacier data are used in this modelling study.
3. L79-93: Are the gridded meteorological satellite data corrected with in-situ station data? How are the different resolutions of various types of gridded spatial data used in the hydrological model? Please provide details on this.
4. The description of the streamflow sampling is very vague, which is simply stated as “Grab samples of stream water were collected in 2005 at four stations..”. Please provide details on how many samples and in which months the samples were collected. Do the authors have the precipitation (rainfall, snow) isotopes in the same year (2005) or in a different year (2008)? Using streamflow and precipitation isotope data of different years in the same model can be inappropriate.
5. How is the precipitation tracer estimated for rainfall and snow individually? This needs to be clarified in the manuscript.

Interpretation of the results:

1. L256-259: The SCA shows a higher influence on the posterior distribution of T0 than the GMB, which does not show the strongest influence as the authors interpreted. Could the authors please clarify why they see GMB as the strongest from this result figure (Fig.4j)?
2. L306-308: The isotope data have increased uncertainty of the simulated glacier melt runoff (Fig.6d), but they are helpful to constrain other surface runoff components (rainfall runoff, snowmelt). Please clarify this result.

3. L272-277: Including the isotope data leads to a decreased containing ratio. This means a significant under capture of the extremely low and high streamflow. Why including isotope data has decreased the streamflow simulation performance? Please clarify this result.

Technical corrections:

L8: It would be helpful to mention which type of hydrological model the THREW-T is in the abstract. i.e. fully-distributed, semi-distributed, or conceptual?

L78: km^2 should be straight upright, not italic. Please correct all formats of the units for similar cases.

L80, L82: Please add years between which the mean annual precipitation and mean annual temperature are calculated.

L100: distributed -> semi-distributed?

L104: what is bare zone? Bare soil, bare rock?

Figures 2 and 4: avoid using red and green colors together in the same figure to allow readers with colour vision deficiencies to correctly interpret your findings.

Table 1: table caption should be on top of the table.

L165: Please correct the formats of Equations 1-6 by following the journal guideline. e.g. the NSE should be straight upright, not italic. The text subscription should be straight upright as well.

L210-211: NSE, VE should be straight upright, not italic. Please check the format of all such mentioning.

L266-267, 281: Please remove the parentheses around the Section and Figure numbers, and correct all such mentioning in the manuscript.

Figure 4 caption i) covered area -> snow covered area.