Response to anonymous reviewer #1

Overall comment:

I appreciate the opportunity to review the manuscript, entitled "Runoff component quantification and future streamflow projection in a large mountainous basin based on a multidata-constrained cryospheric-hydrological model." The topic of this study is of great importance not only to the earth and environmental science community but also to policymakers and practitioners such as hydropower companies and water resource managers. This study presents an attempt to systematically analyze streamflow variations and runoff components in the Yarlung Tsangpo River (YTR) basin using a physically-based hydrological model validated by streamflow and multiple datasets related to cryospheric processes. Despite some limitations, the proposed method is capable of reconstructing the sediment yield over the past decades with satisfactory performance.

Overall, I like the study and would recommend a moderate revision before publication. Below are my major and specific comments.

Response:

Thank you very much for your high evaluation on our manuscript and the constructive suggestions. We will revise the manuscript thoroughly according to your comments.

Major Comments

Comment 1: Model Validation and Result Presentation

Based on the modeling scheme, model validation should target multiple hydrological processes, including streamflow, snow, and glaciers. The authors used multiple datasets to validate the model, which is commendable. However, in the results section, the authors seem to focus primarily on streamflow validation. It is suggested that the authors provide a more detailed presentation of the validation results for snow water equivalent (SWE), snow cover area (SCA), and glacier mass balance (GMB) to comprehensively evaluate the model's performance.

Response:

Thanks for your comments. Actually, when presenting the results obtained by four calibration variants with different calibration objectives (i.e., D, DS, DG, and DSG), the simulated SWE, SCA and GMB are presented. But we didn't show these results of upstream regions produced by the DSG calibration variant when comparing DSG and ALL variants, only showing the evaluation metrics. We will add more details of simulated SWE, SCA and GMB in the revised manuscript.

Comment 2: Simulation of Extreme Events

Typically, extreme hydrological events contribute significantly to annual runoff and can have severe socio-economic impacts. However, daily-scale models often underestimate these extreme events. Have the authors considered the model's performance in simulating extreme events? If there is underestimation, can the model structure or parameterization be improved to enhance simulation accuracy?

Response:

Thanks for your comments. Indeed, our study focused on the overall model performance on hydrograph simulation, paying less attention to the extreme events. Besides, based on the comparison of observed and simulated hydrograph, we can observe a generally underestimated peak flow, even in the D calibration variant where NSE is higher than 0.9. Results are similar in some hydrological modelling studies in other basins on the Tibetan Plateau (e.g., Su et al., 2023, Xu et al., 2019). In addition to the limitation of daily-scale modelling, this could be also due to the uncertainties in precipitation dataset. The mainstream precipitation datasets generally underestimate the precipitation amount, due to the lack of validation toward observation data in high altitude regions, where precipitation amount is generally high (Xu et al., 2017). We will add this issue in the limitation section in the revised manuscript.

Comment 3: Uncertainty Analysis in Future Projections

The authors used data from 10 CMIP6 global climate models for future projections, which is a robust approach. However, significant differences may exist between different climate models. It is recommended that the authors provide a more detailed analysis of these inter-model differences and discuss how they affect the uncertainty in future streamflow projections.

Response:

Thanks for your comments. For now we only show the uncertainty bands produced by different GCMs in the figure of streamflow projection results. We will describe the uncertainties in the GCMs and the projected future streamflow with more details in the revised manuscript.

Comment 4: Focusing on the different time periods

The authors adopt different time periods in different parts, which makes me really confused. For example, 1980-2018 is used for model calibration and validation, 1960-2020 is used for the historical trend analysis, 1960-2014 is the historical period of CMIP6 (L202), and 1980-2009 was used as the baseline of historical simulation. I suggest authors to summarize the different time period adopted in different analyses, and clarify the reason why different time periods are adopted.

Response:

Thanks for your comments. Different time periods were adopted in different part due to the availability of streamflow and meteorological datasets. In specify, we selected the past 6 decades (1960-2020) to analyze historical streamflow changes based on the start time of the measurement at hydrological stations, while the most applicative precipitation data over the YTR basin to build the model covered 1979-2018, which was divided into calibration/validation periods (1980-2009/2010-2018) in our study. The CMIP6 data was divided into historical and future periods by 2014 and we chose 2 periods in the near/far future (2020-2049/2070-2099) to compare with the historical 30 years (1980-2009). This indeed makes the analysis confusing. We will add several sentences in the revised manuscript to clearly clarity the adoption of different time periods and the reason for such difference.

Specific Comments

Abstract: The abstract can be substantially shortened to one paragraph. It should focus on the study's innovation and main findings.

Response:

Thanks for your suggestion. We will shorten the abstract to focus on the innovation and main findings in the revised manuscript.

Introduction: The introduction lacks acknowledgment of existing literature on multi-decadal sediment observations in other high mountain areas and cold regions such as the Andes and the Arctic. Supplementing this literature would enhance the comprehensiveness of the research background.

Response:

Thanks for your suggestion. We will add some references related to water and sediment processes in other could mountainous regions (e.g., Slosson et al., 2021; Zhang et al., 2023) to enhance the paper's relevance to a wider audience.

Methods: The methods section should detail how multiple datasets were integrated into model calibration and how the weights of each dataset were determined.

Response:

Thanks for your suggestion. Actually, the datasets for calibration in Table2 was used separately to calculate the evaluation indicators with the model outputs in model calibration. For instance, we used the observational and the simulated streamflow data series to calculate NSE and lnNSE indicators. Then, when selecting the optimal parameters, these calculated indicators owned the same weight, while also taking into account manual judgment. We will explain it more clearly in the revised manuscript.

Discussion: The discussion section could delve deeper into the mechanisms by which climate change affects runoff components, particularly the reasons for the reduction in contributions from snowmelt and glacier melt. Additionally, comparing the findings with studies from other cold regions would enhance the depth of the discussion.

Response:

Thanks for your suggestion. The reason for the reduction in meltwater contributions was explained in the result section ("The decreasing snowmelt runoff was due to the reduced snowfall caused by climate warming, while the reduced glacier melt runoff indicated that the effect of shrinking glacier areas was more dominant than the acceleration of glacier melting caused by global warming."), and the discussion section mainly focus on the influence of runoff component estimation on future runoff projection.

It is a good idea to compare the findings with studies with other cold regions. We find that the streamflow is commonly projected to increase in mountainous river basins across the world (e.g., Slosson et al., 2021; Zhang et al., 2023), but the reason for the increasing trend could be different. In the YTR basin where rainfall dominates the runoff, the projected runoff is mainly determined by the trend of precipitation. On the contrary, in the basins where meltwater plays important role in runoff generation, the runoff trend is more related with that of temperature, and the runoff might increase even if the precipitation decreases (Slosson et al., 2021). The contribution of meltwater could be especially significant in the regions where precipitation and heat are asynchronous, such as Pamir Mountains and Pan-Arctic regions (Pohl et al., 2015; Zhang et al., 2023). We will add these discussions in the revised manuscript.

Conclusions: The conclusion should more clearly summarize the main findings and point out the study's limitations and future research directions.

Response:

Thanks for your suggestion. We will simplify the conclusion section and point out the limitations and future directions.

Figures and Data: The clarity and informativeness of the figures need improvement. For instance, include the summer discharge trends in Figures 7c-d and ensure consistency with the main text.

Response:

Thanks for your suggestion. Improving the informativeness of figures is indeed a good idea, but since Figure 7 is a hydrograph figure and there is no significant trend in summer discharge (as shown in Table 6), including trend lines is not suitable for it. Nonetheless, we will add the trend for the future streamflow projections in Figures 9 and 10.

References: It is recommended to supplement relevant references to place this study in a wider framework of when discussing the impact on hydropower and comparing with other related studies. https://doi.org/10.1038/s41561-022-00953-y; https://doi.org/10.1002/hyp.14633; https://doi.org/10.1016/j.geosus.2024.01.001

Response:

Thanks for your suggestion. These references are indeed highly related to our research, and we will add them in the revised manuscript.

L24-25: This statement is rather strong. Maybe better to remove it from the abstract and mention it somewhere in the discussion section.

Response: Thanks. We will remove this sentence from the abstract.

L108: There are totally more than 20 GCMs in CMIP6, so how did you select these 10 GCMs?

Response: We selected these 10 GCMs based on the stability of these data in our model and the rationality of simulation results. Different GCMs have different starting times and perform variously in specific basins and hydrological models. We tried about 20 models and finally chose these 10 models with more reasonable results and stable performance, and we will add this in the revised manuscript.

L117: Provide the full name of PMV.

Response: The full name of PMV is passive microwave, and we will add it in the revised manuscript.

L127: “CGM” should be “GCM”.

Response: Thanks for correction.

L133-134: Difficult to follow. Maybe provide the specific equation here.

Response: We will provide the equation in the revised manuscript.

Table 5: In the notes for DG and DS, “calculated” should be “considered”.

Response: Thanks for correction.

L188: The historical trend analysis seems to be a separate part from the manuscript. Consider adding some transitional text when describing the methods and results.

Response: Thanks for your suggestion. As mentioned in the introduction, this study focuses on the streamflow change in YTR basin during the whole period including both historical and future period, and we aimed to conduct a systematic analysis on the streamflow change and runoff component.

Consequently, the historical trend analysis is the basis for this study. We will add several transitional sentences in the method section in the revised manuscript.

Table 7 and 8: There are two "1980-2009" for discharge NSE; add the unit for the RMSE of SWE and GMB.
Response: Thanks for correction. We will correct the period and add the unit of SWE and GMB.

L300-301: Delete this sentence, since "ALL" is the most reliable variant, as mentioned several lines below.
Response: We will delete this sentence.

Figure 7 and 8: Consider adding the simulations obtained by "DSG" variant (if this makes the figure too large, maybe add them in Supplementary Materials). We cannot know the specific performance on these elements (e.g., overestimation or underestimation) solely based on the NSE and RMSE.
Response: Thanks for the suggestion. We will add the related results in the supplementary materials, since there are already up to 16 figures in the main text.

L319-320: Is this "insignificantly" referring to visual or statistical significance?
Response: Both. The P value is <0.01 in all time periods under SSP585 scenario while this is not the case for SSP126/245, and we will consider adding it in the revised manuscript.

Overall, this study addresses an important topic with significant implications for understanding hydrological processes in the Tibetan Plateau. The use of multiple datasets to constrain the model is commendable. However, there are areas where the analysis and presentation could be improved. I hope these comments will be helpful in revising and strengthening the manuscript.
Response: Thanks again for your appreciation on our study. We will revise the paper thoroughly according to your suggestions.