Response to Reviewer #1:

Comment 1:

The authors in this manuscript discussed the potential utility of applying the Isotopic General Circulation Models (iGCM) product isoGSM in assisting large scale hydrological modelling. It was found that spatial isotope data of precipitation from isoGSM can essentially help to reduce modeling uncertainty and improve parameter identifiability in comparison to a calibration method using only discharge and snow cover area fraction without any information of water isotope. The suggested isotopic-tracer-aided hydrological model showed high values for robustly representing runoff processes in large mountainous catchments with sparse observations in high mountain Asia. This topic is closely matched to the journal and the results can be interesting for hydrological modelling community. Additionally, it is well-written, logically organized, and easy to follow. Reviewer would like to point out two main concerns that may be helpful to generalize the results to improve the paper.

Response 1:

Thank you very much for your appreciation. We have revised the manuscript thoroughly according to your comments. In the following please check our responses to your comments on a point-to-point basis.

Comment 2:

The authors claimed that with spatial precipitation isotope derived from the isoGSM data modeling uncertainty and parameter identifiability can be greatly reduced in the large mountainous catchment. The isotopic data provides additional information to constrain the uncertainty of model parameters controlling water separations into direct runoff, subsurface flow, etc. I think it can be regarded as some kinds of fine tuning without modifying the model structure itself. However, as have been reported in many recent studies, global climate changes are changing streamflow regimes and groundwater storage in cold alpine regions on the TP (e.g., Xu et al., 2019; Lin et al., 2020; Yong et al., 2021). In a warming background, for example, frozen ground on the TP are experiencing significantly degradation, which will modify storage capacity of soil and groundwater and even the flow pathway. Hence, the question is that is it enough to justly constrain parameters, and shall the model structure be simultaneously changed in the study basin?

Response 2:

Thanks for your comment. We acknowledge that the model structure changes in the warming background. However, we would like to clarify the three points. First, some of the changing underlying surface conditions can be reflected by not only model structure, but also parameters themselves. For example, frozen ground degradation can lead to a larger water storage capacity and higher hydraulic conductivity, which can be reflected by the parameters WM, KKA and KKD in the model. Second, the tracer-aided modelling method can actually help diagnose model structure as well (e.g., Birkel et al., 2011), but such work was only conducted in small catchments due to the limited precipitation isotope input data in large scale. This study mainly explored the utility of isotopic GCM data on driving tracer-aided model in large basin, thus provided the potential to conduct the works improving model structure in large basin scale. Thirdly, we applied the model at a relatively short time scale (less than one decade), during which the structure change is not significant. We will explore how the isotope data help with model structure change under climate

change conditions in our future research. Thanks again for the comments.
We have added the discussion about this issue in the revised manuscript (L554-572).

Comment 3:
Results suggested that model driven by the corrected isoGSM data can provide a more reliable ratio in determining the contributions of runoff components, especially the overestimations of glacier melt. The authors have compared the results with other assessments (e.g., Immerzeel et al., 2010). I know that accurate estimation of runoff components in a macro-basin is a tough task due to sparse observations, and thus maybe controversial in high mountain Asia basins. However, the reviewer suggested that more evidences (e.g., isotopic results or sub-basin results or neighboring observed data) besides modelling results should be added and compared to justify their results. And statistical results about glacial retreat in the YTR maybe help as another useful evidence for runoff components determinations. In addition, more physical explanations of adopted assumptions, equations (e.g., equation (1)) should be supplied.
Response 3:
Thanks for your suggestion. We have tried to find more evidences to verify our results through following ways (L615-633):
1. L615-624: Verifying the results of glacier melt estimation by comparing the calibrated DDF with the reported values estimated in a physical manner by glacier mass balance measurements (Zhang et al. 2006).
2. L624-633: This study also estimated the contribution of different runoff generation pathways (surface and subsurface runoff). We compared the contribution of baseflow with the result estimated by groundwater model independently from hydrological modeling approach (Yao et al., 2021). An accurate baseflow estimation can lead to a more reliable estimation of water sources by constraining some of the parameters.
In addition, we have provided more physical explanations of equations used to interpolate the measurement data and correct the isoGSM data in the revised paper (L159-168, L545-553).

Comment 4:
One of the reasons limiting tracer-aided model in applying in larger scale catchment lies in the lumped conceptual model structures. So the reviewer suggest that more information about model structures should be added in the section Introduction and methodology. How to delineate a larger scale basin into response units in your model for fully capturing the heterogenous natures of a basin? And how to organize the model structure to consider the strong spatial variability of runoff generations especially in vertical direction.
Response 4:
Thanks for your comment. This study adopted the spatially semi-distributed model conceptualization of representative elementary watershed (REW) to enable the model to simulate the runoff processes in a large scale. The heterogenous natures of basin were captured by the distributed input data including climate factors, vegetation, soil and topography, which affected the runoff generation processes.
We are not sure whether “vertical direction” means the different processes in vertical direction within a small scale (i.e., the canopy interception, the surface runoff and infiltration), or the different runoff processes in regions with different elevation? If it is the former, the THREW model divided

the REW into two vertical layers (surface and subsurface layers), and further into eight sub-zones including interception, bare soil, river channel, unsaturated groundwater and saturated ground water, which described the vertical characteristic. We have added description about this in the revised manuscript (L222-226). If it is the latter, the elevation determined the meteorological (e.g., temperature, precipitation) and topographical (e.g., hillslope gradient) factors, which influenced the runoff generations. Consequently, the vertical variability of runoff generation processes in both regional and basin scale were considered in the THREW model.

For the development of a distributed tracer-aided model, we should say that developing a distributed tracer-aided model is a tough task when compared to lumped model (because the tracer processes need to be combined with a rather complex description of runoff processes). However, it is not the most critical one. The challenge is that such models cannot be applied to large basin due to data availability issue, which is the focus of our study.

Comment 5:

As pointed by the authors, runoff in this region is highly vulnerable under climate warming, and hence the land covers, soils and groundwater aquifers. How do they consider these changing environmental factors in hydrological modelling?

Response 5:

Thanks for your comment. We think that the changing factors can be represented by the changing model structures, parameters and input data. We need to acknowledge that the changing conditions are far less than adequately represented in current model due to lack of adequate understanding of influence of changing condition on runoff generation mechanism. Some of the changes can be represented by model parameters, so we can represent such kind of changes by tuning model parameters including by using isotopic data. But more studies are required to understand the influencing mechanisms.

However, this study applied the model at a relatively short time scale (less than one decade), during which the problem of changing condition is not a big issue. This study mainly focused on finding more accurate parameters in a given period.

We have added discussion about this issue in the limitation part of revised manuscript (L554-572).

Comment 6:

Could you provide more details about how model parameters be constrained or calibrated in terms of isotopic data?

Response 6:

Thanks for your question. The parameter was constrained by involving the behavior of isotope simulation in the optimization objective of calibration process. We have provided more details about this issue in the introduction part of revised manuscript (L55-58).

Comment 7:

Does the TPSCE data include glacier in snow cover, or not?

Response 7:

Yes. The TPSCE data is generated by mering multisource snow cover datasets (Chen et al., 2018), and includes the glacier area. Actually, we have found that the snow cover area in the KR catchment has a minimum value, which is close to the glacier covered area ratio.

We have made this clearer in the revised manuscript (L147-148).

Comment 8:

As is known precipitation condensing at cooler temperatures tends to be more depleted in the heavier stable isotopes, thus precipitation falling at higher latitudes, at higher elevations, and further inland tends to be isotopically depleted (Yang et al., 2020). So try to explain the physical meaning and extent of the coefficients (e.g., x, y) in Equation (1).

Response 8:

Thanks for your comment. The equation 1 was used to capture the elevation effect and continental effect. The measurement stations were approximatively at the same latitude, and the extent of YBR basin was within a small range of latitude (Fig. 1 in article), thus latitude was not chosen as a variable in regression. Longitude can reflect the distance from the station to the China's mainland border, thus the coefficient y is expected to be higher than 0. The coefficient x reflects the altitudinal lapse of precipitation isotope composition, thus is expected to be lower than 0. The estimated values of coefficient were same as expected (x = -0.003, y = 0.574).

We have added the explanation of the coefficients their extents in the revised manuscript (L159-168 and L331-333). Thanks.

Comment 9:

Isotopic composition of glacier meltwater in this catchment was assumed to be -18.9‰, why a constant value was adopted here. The uncertainty of isotopic data in glacier as well as precipitation for hydrological modelling should be discussed.

Response 9:

Thanks for your question. The uncertainty that isotope input data brings to hydrological model is an important issue to be discussed. We found that large number of studies indicated that the isotope composition of glacier melt had very small variability, whose value were much lower than that of precipitation (e.g., He et al., 2019; Cable et al., 2011; Rai et al., 2019; Wang et al., 2016). So it is reasonable to assume the isotope composition of glacier melt as a constant value, when there is no available measurement data. However, the value of the assumed composition will affect the model, especially the estimated contribution of water sources. A lower assumed value of glacier melt isotope composition may lead to lower contribution of isotopic depleted glacier melt runoff component.

We have added more discussion about the uncertainty of isotope composition of glacier meltwater in the revised manuscript (L508-519).

Comment 10:

Equation (3) is similar to Equation (1). However, the equation has deprecated the term longitude here. Why?

Response 10:

Equation (1) was used to interpolate the point-scale measurement data to the whole basin, and the term longitude reflected continental effect. Equation (3) was used to correct the output of isotopic GCM model, which tended to have larger error in the regions with higher elevation, because of the complex regional topography, which cannot be well captured by the coarse spatial resolution of GCM. It seems that no mechanism can make the error of GCM change with longitude, thus it was deprecated in Equation (3). However, the choice of regression term in regression and bias correction will undoubtably have important influence on the modelling result. Consequently, we still need to do lots of works to explore a general way to drive tracer-aided model using isotopic GCM data (e.g., to have a better understanding on the bias characteristic of the iGCM data).

We have explained this issue with more details and discussed the uncertainty it might bring in the revised manuscript (L545-553).

Comment 11:

The standard for REW delineation? Why do you sub-divide the whole YTP into 63 units and however 41 in the more smaller catchment KR?

Response 11:

The REW (representative elementary watershed) approach is adopted based on the self-similar characteristics of a watershed and its sub-watersheds (Reggiani et al., 1999). REW is considered as the fundamental component of hydrological processes and modelling, in which series of balance equations are established. The major principle of REW scale is the scale of interest, modelling purpose, and the data availability (Tian et al., 2006; Tian et al., 2008). Previous study (Tian et al. 2020) divided the YTR basin into 63 REWs considering the data availability and simulation objectives. This study established the model on the basis of Tian et al. (2020), and the available isotope data did not support further division of basin, thus the division of 63 REWs was adopted in this study. Similarly, KR catchment was divided into 41 REWs in a previous study (Nan et al., 2021) to characterize the strong variation of altitude, and was adopted in this study. We have added more details about REW delineation in the revised manuscript (L222-231).

However, we need to clarify that the representative scale of REW is not clear, and we will explore it influence on modelling result in the uncertainty analysis in future work. Thanks again for the question.

Comment 12:

Why NSE threshold is significantly larger in maco-YTR than in smaller scale of KR?

Response 12:

We found that it was relatively easier to get good simulation and high NSE (> 0.9) in YTR than in KR (the highest NSE was around 0.85). Many previous studies indicated that the nonlinearity of rainfall-runoff process seems to be lower in large basins than small catchment, likely due to the compensation of several complex processes. We have addressed this in the revised manuscript (L284-285).

Comment 13:

The authors can refer to some reported contemporaneous isotopic data if possible, add some sporadic-distributed data as additional evidences besides the continuous observations in 2005.

Response 13:

Thanks for your suggestion. We tried to use some other isotope data to verify our result, but we found discontinuous and sporadic-distributed isotope data not suitable for this aim. The model performed better on capturing the seasonal variation of river isotope, but not as well on simulating the isotope signature for a given date or a short period. That is why we divided the limited available isotope data into two groups, i.e., the data at outlet station for calibration, and the data at internal stations for validation. Nonetheless, we have used some non-isotopic evidence to verify the model behavior as mentioned in Response 3 (L615-633 in the revised manuscript).

Comment 14:

In which stations the model performance in YTR was shown in Table 3 and Fig. 5-6?

Response 14:

Nuxia for discharge, the whole YTR basin for SCA, and all the four stations for isotope. We have made this clearer in the revised paper.

Comment 15:

Why the dual-objective has obtained the best results, while it produced the worst MAE values on another hand? However, the two scenarios adopting isotopic data as supplements for modelling could get better results of runoff components. More details should be revealed why the latter two scenarios calibrate the model at the cost of precision, and in order to obtain more accurate predictions, part of hydrological processes must have been distorted in the dual-objective to compensate other wrong representation in hydrological process simulation.

Response 15:

Thanks for your comment. This is indeed an important issue to illustrate the role of isotope data to improve the model behavior.

The MAEiso was not involved in the optimization objective in the dual-objective calibration, thus worst MAEiso values were obtained. We analyzed the relationship between the behaviors of discharge and isotope simulations (NSE-dis and MAE-iso) obtained by dual-objective calibration, and found that there was a trade-off between the two objectives (Fig. R1a). The highest NSE-dis can reach around 0.93, but the MAE-iso is not good at the same time. When MAE-iso reach relative best values, the NSE-dis is around 0.9, which is still a high-level performance. We further found that when the highest NSE-dis was obtained, the contribution of glacier melt was estimated as around 0.35~0.4, which was however estimated as around 0.2 when best MAE-iso was obtained (Fig. R1b and c). The isotope composition of glacier melt was assumed to be lower than the precipitation, thus an overestimated contribution of glacier melt can lead to lower simulated river isotope than measurement. Consequently, calibration focusing only on discharge may result in overestimated glacier melt, which can be rejected by the behavior of isotope simulation.

It is notable that the performance of isotope simulation is more sensitive than discharge simulation to the runoff component and internal processes. For example, when the contribution of glacier melt is in a large range of 10-40%, the NSE-dis can all be calibrated to a high value (>0.9) by adjusting other parameters, whereas the MAE-iso gets worse significantly when the proper contribution of water source is deviated.

We have discussed this issue with more details and provided corresponding figures to illustrate the value of isotope data for aiding hydrological modelling in the revised manuscript (L576-593).

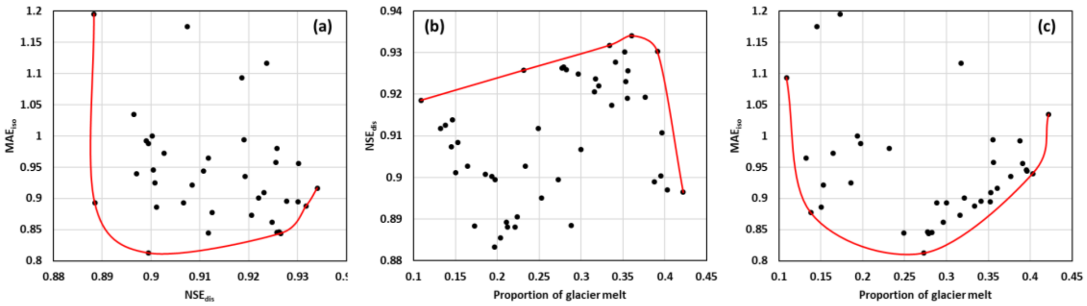

Fig. R1. The relationships between (a) MAEiso and NSEdis, (b) NSEdis and glacier melt contribution and (c) MAEdis and glacier melt contribution.

Comment 16:

Provide a spatial distribution map of precipitation isotope.

Response 16:

Thanks for your suggestion. We have provided the spatial distribution map of precipitation isotope in YTR basin (Figure 5) and added some description on the map in the revised manuscript (L339-344).

Comment 17:

Why the dual-objective has obtained good results in predicting discharge in the outlet station, while the other two scenarios adopting isotopic data could get better results in internal stations?

Response 17:

Thanks for your question. The model was only optimized according to the NSE at outlet station in dual-objective calibration, thus cannot necessarily result in a proper representation of internal processes. However, the fact that model can simultaneously satisfy multiple calibration objectives gave confidence in the model realization (McDonnell and Beven, 2014). Better performance for internal stations is not necessarily consistent, but the fact that most of them perform reasonably shows the robustness of the modelling results.

We have added some discussion on the role of isotope data on improving model performance at internal stations in the revised manuscript (L602-605).

Comment 18:

What is the meaning "consistently estimated lower proportions of glacier melt than the dual-objective calibration, which can be attributed to the role of isotope data in regulating the contribution of strong-evaporated surface runoff component fed by glacier melt to streamflow"? And what is the proportion of glacier evaporation in glacier melting?

Response 18:

Nan et al. (2021) provided a more detailed explanation for this. The THREW model assumed the glacier melt contributed to river channel through surface runoff, together with other surface components (precipitation occurring in saturated area and impermeable areas). The impermeable area in KR catchment is large due to the large glacier covered area, resulting in a large contribution of surface runoff. The evaporation of surface water was highly related to the surface runoff, and consequently related to the contribution of glacier melt. The surface evaporation process resulted in a higher isotope composition of surface runoff component due to the isotopic fractionation effect.

Our results indicated that only when the proportion of evaporation to total surface runoff was around 30%, the model can perform well on isotope simulation.

Comment 19:

The largest differences in the winter season can only explain that isotopic constrain functions. But the predictions have also been improved?

Response 19:

Thanks for your question. We attributed the large differences in winter to the extremely small total water input, because the contribution of water sources was calculated by dividing the amount of individual water source by the total water input amount. Nonetheless, the difference had negligible effect on the prediction of total runoff, because of the extremely low contribution of winter water to the total annual amount (<1%).

Comment 20:

The uncertainty of isotopic data for hydrological modelling should be discussed quantitatively and deeply. For instance, the distribution map of precipitation isotope is coarse and vertical effects may be not considered in present scenarios in details.

Response 20:

Thanks for your suggestion. However, we thought that a quantitative evaluation of isotopic uncertainty requires much more work to do. We are going to prepare a separate paper about this issue following this work. We have added more discussion about the uncertainty sources, including the isotope composition of water sources, the interpolation and correction equations, and the changing conditions that has not been addressed adequately in the revised manuscript (section 4.1).

TECHNICAL CORRECTIONS

P2L26: was first corrected changes as was firstly corrected.

P3L52: Zongxing et al., 2019 changes as Li et al., 2019? The following same below can also be revised.

P3L74-79: Quite a long sentence it is and suggest to adopt short sentence to follow the gist easily.

P11L364: variant changes as scenarios?

P27: keep x-, y-axis in the same scale.

P39: calibration scenarios instead of calibration variant makes sense?

Response:

Thanks for your corrections, and we have revised these in the newest version of paper. The term "calibration variant" was referred to Tong et al. (2021) which similarly conducted several calibration scenarios to explore the value of soil and snow data.

References

Birkel, C., Tetzlaff, D., Dunn, S. M., & Soulsby, C. (2011). Using time domain and geographic source tracers to conceptualize streamflow generation processes in lumped rainfall-runoff models. Water Resources Research, 47(2). doi:10.1029/2010wr009547

Cable, J., Ogle, K., & Williams, D. (2011). Contribution of glacier meltwater to streamflow in the Wind River Range, Wyoming, inferred via a Bayesian mixing model applied to isotopic measurements. Hydrological Processes, 25(14), 2228-2236.

Chen, X., Long, D., Liang, S., He, L., Zeng, C., Hao, X., & Hong, Y. (2018). Developing a composite daily snow cover extent record over the Tibetan Plateau from 1981 to 2016 using multisource data. Remote Sensing of Environment, 215, 284-299.

He, Z., Unger-Shayesteh, K., Vorogushyn, S., Weise, S. M., Kalashnikova, O., Gafurov, A., . . . Merz, B. (2019). Constraining hydrological model parameters using water isotopic compositions in a glacierized basin, Central Asia. Journal of Hydrology, 571, 332-348. doi:10.1016/j.jhydrol.2019.01.048

Mcdonnell, J. J. , & Beven, K. . (2014). Debates—the future of hydrological sciences: a (common) path forward? a call to action aimed at understanding velocities, celerities and residence time distributions of the headwater hydrograph. Water Resources Research, 50(6).

Nan, Y., Tian, L., He, Z., Tian, F., & Shao, L. (2021). The value of water isotope data on improving process understanding in a glacierized catchment on the Tibetan Plateau. Hydrology and Earth System Sciences, 25(6), 3653-3673.

Rai, S. P., Singh, D., Jacob, N., Rawat, Y. S., & Arora, M. (2019). Identifying contribution of snowmelt and glacier melt to the Bhagirathi River (Upper Ganga) near snout of the Gangotri Glacier using environmental isotopes. Catena, 173, 339-351.

Reggiani, P., Hassanizadeh, S. M., Sivapalan, M., & Gray, W. G. (1999). A unifying framework for watershed thermodynamics: constitutive relationships. Advances in Water Resources, 23(1), 15-39.

Tian, F., Hu, H., & Lei, Z. (2008). Thermodynamic watershed hydrological model: Constitutive relationship. Science in China Series E: Technological Sciences, 51(9), 1353-1369.

Tian, F., Xu, R., Nan, Y., Li, K., & He, Z. (2020). Quantification of runoff components in the Yarlung Tsangpo River using a distributed hydrological model.. Advances in Water Science, 31(3), 324-336.

Tong, R., Parajka, J., Salentinig, A., Pfeil, I., Komma, J., Széles, B., ... & Blöschl, G. (2021). The value of ASCAT soil moisture and MODIS snow cover data for calibrating a conceptual hydrologic model. Hydrology and Earth System Sciences, 25(3), 1389-1410.

Wang, C., Dong, Z., Qin, X., Zhang, J., Du, W., & Wu, J. (2016). Glacier meltwater runoff process analysis using δD and δ 18 O isotope and chemistry at the remote Laohugou glacier basin in western Qilian Mountains, China. Journal of Geographical Sciences, 26(6), 722-734.

Yao, Y., Zheng, C., Andrews, C. B., Scanlon, B. R., Kuang, X., Zeng, Z., ... & Li, G. (2021). Role of Groundwater in Sustaining Northern Himalayan Rivers. Geophysical Research Letters, 48(10), e2020GL092354.

Zhang, Y., Liu, S., & Ding, Y. (2006). Spatial variation of degree-day factors on the observed glaciers in western China. ACTA GEOGRAPHICA SINICA-CHINESE EDITION-, 61(1), 89.

Response to Reviewer #2:

Comment 1:

The tracer-aided hydrological model was recognized to have value on improving the rationality of model structure and parameter, which was especially important for mountainous catchments. However, the application of tracer-aided model was limited to small and middle scale because of the low availability of isotope data in large scale. The authors developed a procedure to correct the iGCM data and force tracer-aided model in a large basin on the Tibetan Plateau, and had good effect on improving the model behavior. The results open a new window to expand the application of tracer-aided model to larger scales. Meanwhile, the paper is well structured and the language is well written. I recommend its publication on HESS after a moderate revision properly addressing some specific comments.

Response 1:

Thank you very much for your appreciation. We have revised the manuscript thoroughly according to your comments.

Comment 2:

In the multiple-objective calibration methods, the evaluation indexes of different objects are added together directly. I am concerned about the reasonability of this procedure considering that the NSE, RMSE and MAE have different measurement units.

Response 2:

Thanks for your comment. This is indeed an important issue, and the objective function should be carefully determined when developing a general calibration strategy. But this study only aimed to illustrate the benefit from the calibration of isotope, and adding three objective functions together just meant that good simulation for the three objectives were produced simultaneously. Besides, although the three functions have different units, their values are in the similar order of magnitude (0 to 1) when the model performances were behavioral. Our result showed that when three objectives were all simulated well, the uncertainty of parameter and runoff component contribution was significantly reduced compared to the condition when only one objective was satisfied. Thus the influence of objective function choice was not stressed in this study.

We have clarified this issue in the revised manuscript (L277-279 and L287-288).

Comment 3:

Why did the authors interpolate the measured data using the terms longitude and elevation, but correct the isoGSM data only using the term elevation?

Response 3:

Equation (1) was used to interpolate the point-scale measurement data to the whole basin, and the term longitude reflected continental effect. Equation (3) was used to correct the output of isotopic GCM model, which tended to have larger error in the regions with higher elevation, because of the complex regional topography, which cannot be captured well by the coarse spatial resolution of GCM. It seems that no mechanism can make the error of GCM change with longitude, thus it was deprecated in Equation (3). However, the choice of regression term in regression and bias correction will undoubtably have important influence on the modelling result. Consequently, we still need to do lots of works to explore a general way to drive tracer-aided model using isotopic GCM data (e.g.,

to have a better understanding on the bias characteristic of the iGCM data).
We have added more explanation and discussion on the choice of regression terms and its potential uncertainty in the revised manuscript (L159-168 and L545-553).

Comment 4:
Is it enough to only use the average measured isotope data to correct isoGSM data? How about the seasonal characteristic of the bias?
Response 4:
Thanks for your comment. This study aims to develop a strategy for establishing a tracer-aided model in large basin, especially in the regions where little measured precipitation isotope data is available. Consequently, we tried to use as less information from measurement data as possible to correct the isoGSM data.
We have added description about this in the revised manuscript (L647-654).

Comment 5:
The runoff is divided into rainfall, snowmelt and glacier melt. How did the authors consider about the groundwater?
Response 5:
This study quantifies the runoff component in two aspects of definitions. The first definition is the contribution of water sources including rainfall, snowmelt and glacier melt to the total water input into the catchment system. The THREW model adopted in this study focuses on rainfall-runoff, thus the processes of deep groundwater are not described. The groundwater in our model is fed by the infiltrated rainfall or snowmelt, thus it has been included in the three water sources. The second definition is based on the runoff generation pathways including surface runoff and subsurface flow (baseflow), and the result was not reported in the manuscript. The contribution of baseflow was estimated as 29.23km^3/yr by the isoGSM-forced triple-objective calibration. We have reported the estimated baseflow and compared with published results in the revised manuscript (L624-633).

Comment 6:
How did the authors determine the isotope composition of snowmelt and glacier melt?
Response 6:
Snowpack and snowmelt were considered similarly as other water storages and fluxes in the model, thus the isotope composition was simulated similarly with the water isotope based on complete mixing assumption.
According to many extant studies, the glacier meltwater usually has depleted isotope composition with a very small variation. Consequently, the isotope composition of glacier melt was assumed to be a constant value, which is lower than the average isotope composition of precipitation.
Determination of glacier melt isotope composition has been addressed in the original manuscript (L168-170), and we have added a brief description about the determination of snowmelt isotope composition in the revised manuscript (L242-244).

Response to Reviewer #3:

Comment 1:

Measurement Interpolation work. When the vapor transport at the global scale, the latitude is a factor controlling the precipitation isotopes rather than longitude (Latitude effect, Dansgaard, 1964; Bowen & Willkinson, 2002). So could the authors add more explanations on why they use longitude instead of latitude in Eq.1, which would bring large uncertainty to the hydrological model results. Moreover, the evaluation of interpolation work should be quantified like R 2 or other indexes.

Response 1:

Thanks for your comments. The choice of term longitude instead of latitude for interpolation was mainly due to distribution of sampling stations. The latitudes of stations are similar, thus cannot reflect the latitude effect on precipitation isotope. Nonetheless, the north-south range of the basin is much smaller than the west-east range, thus the continental effect rather than latitude effect was considered as the main factor controlling the spatial pattern of precipitation isotope, which could be reflected by the longitude. The R2 value was high as 0.98, because data at only four stations were available. We have added above descriptions and discussions in the revised manuscript (L159-168, L532-533).

Comment 2:

Selection of i-GCM model. As the authors claimed, there are a lot of i-GCM model currently. So why do you select the isoGCM model? In Line 182-184, the authors say the isoGCM model product showed the best performance on simulating global spatial pattern of δ18O by citing a reference of Wang et al., 2017, but I did not see it in the cited paper.

Response 2:

Thanks for your comments. Wang et al. (2017) evaluated ten iGCM products in five aspects: 1) simulation of average isotope, 2) seasonal difference of isotope in spatial distribution, 3) relationship between isotope and temperature in spatial distribution, 4) relationship between isotope and precipitation in spatial distribution, and 5) global meteoric water line. IsoGSM was ranked as 1, 2, 1, 2, 2 on these five aspects, respectively, and had a best integrated ranking. We have added the details of main conclusion by Wang et al. (2017) in the revised manuscript (L189-192).

Wang et al. (2017) was a paper on Chinese journal, and we are sorry that the webpage of the journal is not always accessible. Please find the URL (https://kns.cnki.net/kcms/detail/detail.aspx?dbcode=CJFD&dbname=CJFDLAST2017&filename=DXJZ201709011&uniplatform=NZKPT&v=peznCsKc2ci%25mmd2FapV3eSiIUlEQaHDxe0K8fdTsLgaEAp1Sltwklxi2RTTJF5cDOHn%25mmd2B) to find the English version of this paper.

Comment 3:

Correction of i-GCM. Fig 2 and Fig 3 shows that the correction is not so good except the KR catchment compared to the measurement. Some peak bias can exceed over 20‰，which would certainly influence the isotope modelling results sometimes can’t capture the peak signal (Fig 9). My suggestions is that you could compare other i-GCM data which may be closer to the measurement in your study region or you had better improve your correction method. See also the comment above.

Response 3:

Thanks for your comments. We should acknowledge that the behavior of isoGSM is not so good even though being corrected. However, we found that the seasonal fluctuation played more important influence than the signal of individual events on the isotope simulation, which was captured relatively well by the product. Besides, we aimed to develop a general procedure to establish tracer-aided hydrological model in regions with little available precipitation isotope data, so we used as less information as possible from measurement (i.e., only the average value) to correct isoGSM, resulting in relatively bad performance on capturing the isotope signal at event scale. Lastly, we cannot expect too much of the iGCM products. According to Yao et al. (2013), a work reviewed the observations and simulation on precipitation isotope over the Tibetan Plateau, the iRCM product Zoomed LMDZiso, with relatively best capacity to simulate the isotope signal, still produce large bias on monthly scale, which could still be larger than 10‰ in some months. Nonetheless, the influence of iGCM/iRCM product and bias correction method is indeed a question which requires further explorations and is not a trivial task. We will try to explore this issue more deeply in future works. We have added these discussions in the revised manuscript (L647-656).

Comment 4:

In this work, the modelling results by corrected-isoGSM is compared to that by interpolation from measurement other than measurement. So the authors had better replace 'measurement-forced' with 'interpolation-forced' throughout the manuscript as well as the figures. Otherwise, the readers may misunderstand the work because we think the in-situ measurement is always more reliable than the modeling results whatever physical processes are included in the model.

Response 4:

Thanks for your suggestion. We have replaced all the "measurement-forced" with "interpolation-forced" in the revised manuscript.

Comment 5:

The whole model did not consider the groundwater discharge to the river. However, at the seasonal/monthly scale, groundwater is an important part recharging the river especially in the mountainous region. Please reconsider your model.

Response 5:

Thanks for your comment. The groundwater discharge was actually considered in the THREW-t model. We have added more descriptions about the model conceptualizations of the hydrological processes, especially the groundwater discharge in the revised manuscript (L222-226).

Meanwhile, we should clarify that the model quantified the runoff component in two aspects. First is based on the individual water sources in the total water input forcing runoff processes including rainfall, snowmelt and glacier melt. Second is based on the runoff-generation processes including surface runoff and subsurface runoff (baseflow). Consequently, the contribution of groundwater was not reported parallelly with the rainfall and meltwater, because they were not quantified in the same definition. However, as a rainfall-runoff hydrological model, THREW-t model only considered the role of shallow groundwater which can be recharged by the rainfall, but did not simulate the contribution from deep groundwater storage. We have added this in the revised manuscript (L241-251), and we found some evidence to verify our estimation of baseflow (L624-633).

Comment 6:

The last key word is too long and has to be shorter.

Response 6:

Thanks for your suggestion. We have shortened it as “iGCM correction with sparse measurements”.

Comment 7:

You’d better add N and E to the latitude and longitude in Fig. 1.

Response 7:

Thanks for your suggestion. We have added N and E to the latitude and longitude in Fig.1.

Comment 8:

The subscript i in Eq.2 and Eq.4 represents different meaning, so you have to change one subscript in case of misleading the readers.

Response 8:

Thanks for your suggestion. We have replaced subscript i with k in Eq. 4, and changed the description on the two subscripts in the main text. Subscript i represents the number of sampling site, while k represents the number of the hydrological model unit.

Comment 9:

Line 187: before being used.

Response 9:

Thanks for your correction. It has been changed.

Comment 10:

The scale of Y-axis should keep in uniform for the same station in Fig.6. Also, the Y-axis of Fig 9. (a)(b)(c) had better to be the same so that the results can be easier to compare.

Response 10:

Thanks for your comment. The scale of Y-axis has been kept same.

Comment 11:

The format of reference should be unified throughout the whole manuscript, e.g., line 52 and line 56 are not in the same citation format. Besides, the journal name should also be checked carefully in the reference list, e.g., Wang et al., 2017.

Response 11:

Thanks for your comment. We have unified the format of reference. We found that some of the references in Chinese journal (such as Wang et al. 2017) are not accessible, but they could be searched in the CNKI website (https://www.cnki.net/).

References

Yao, T., Masson‐Delmotte, V., Gao, J., Yu, W., Yang, X., Risi, C., ... & Hou, S. (2013). A review of climatic controls on δ18O in precipitation over the Tibetan Plateau: Observations and simulations. Reviews of Geophysics, 51(4), 525-548.