Authors' **response** to the **comments** by Anonymous Referee #2

We thank the reviewer for the careful comments and criticisms. Reviewer #2 has recommended rejection, essentially based on two criticisms: 1) our model does not represent reality well enough, and 2) our conclusions are not significant. Before providing a detailed response to the reviewers' comments, we present our response on the above two points.

1) Our model is a reasonable one in terms of process representations, and also in terms of data used for calibration. This is evident when the present model is compared with all the 18 cited glacio-hydrological model studies from the region (Table R1 below). The calibration using both runoff and glacier mass balance data simultaneously (sect 3.2.3), and the detailed comparisons of modelled sensitivities of mass-balance (sect 4.3) and runoff (sect 4.2) with existing studies, do not reveal any inconsistencies in our results. Moreover, our process-based explanations for the pattern of sensitivities reported (sect 4.5) support that the effects may be real.

Table R1: A comparison of the characteristics of the glacio-hydrological studies over the High Mountain Asia, with the present study: The entries denote the number of model studies having a particular feature.

	Heavily glacierised Himalayan catchments (6 models in total)	Sparsely glacierised Himalayan catchments (8 models in total)	Regional studies covering High Mountain Asia (4 models in total)	This study
Glacier baseflow	Yes: 0 No: 6	Yes: 0 No: 6	Yes: 0 No: 6	No
Glacier storage	Ignored:4 Linear reservoir: 2	Ignored: 3 Linear reservoir: 5	-	Linear reservoirs
Sub-debris ice melt	4	2	1	No
Snow redistribution model	1	1	1	No
Icemelt model	DDF: 5 Energy balance: 1	DDF: 7 Energy balance: 1	DDF: 2 Energy balance: 1	DDF
Runoff observations	>5yrs: 2 <5yrs: 4	>5yrs: 6 <5yrs: 2	-	>5yrs: Dudhkoshi <5yrs: Chandra
Mass-balance observations	Yes: 5 No: 1	Yes: 2 No: 6	Yes: 4 No: 0	Yes
Input precipitation correction	Yes: 5 No: 1	Yes: 5 No: 1	Yes: 4 No: 0	Yes
Number of calibration parameters	3 to 5: 1 =<2: 4	3 to 5: 2 <=2: 0	-	2

2) We are not aware of any systematic and focused studies of climate sensitivities of the glacierisied Himalayan catchments. In particular, the solely precipitation-independent (temperature-independent) glacier (off-glacier) runoff in these catchments has neither been pointed out nor been explained in terms of the underlying processes before. The corresponding implications for the change and variability of runoff have not been studied as well. Thus, the results presented may be considered significant.

This is a well written manuscript on a very relevant topic regarding the contribution of streamflow generated in the glacier-covered part of a catchment to catchment-scale water resources. It uses two contrasting glacierised

Himalayan catchments, one of which is winter-precipitation dominated, Chandra (the western Himalaya), and the other one summer-precipitation dominated, upper Dudhkoshi (the eastern Himalaya). For these catchments, climate sensitivities of simulated streamflow is obtained by regressing the simulated variability of streamflow to the one its meteorological drivers. The used model is a the Variable Infiltration Capacity (VIC) model, augmented with a glacier melt module.
We thank the reviewer for the critical comments.

The analysis is model-based; , the used precipitation-glacier-melt-streamflow model is very simple for the glacier-covered catchment part; as far as I see, it sums up the ice melt and the snowmelt (and rainfall) and routes it through a single (or perhaps two, unclear) linear reservoir, i.e. the corresponding streamflow response has a single time scale stemming from icemelt and snowmelt and no baseflow, thus the model can most likely not simulate a water carry-over effect from month to month for the glacier part. This model structure might have a different impact on the estimated sensitivities for the different analysed catchments.
We beg to differ with the reviewer’s opinion here.

The baseflow for the Himalayan glaciers was ignored assuming the negligible permeability of the bedrock. As table R1 shows the same assumption was made in most, if not all, of the available studies (Table R1). We do include baseflow for the off-glacier area (including lateral basins/moraines around the glaciers) (Fig 3).

About glacier storage, all the existing studies either ignore the process or use a reservoir model as used in this study (Table R1). Note that the slow (monthly) timescale in glacier runoff is due to the snow storage which is incorporated in our two-reservoir model (L 160).

Furthermore, we do not have information on how large the (ignored) debris cover is nor on how important snow redistribution is, we simply know that it is ignored.
Only 4-7% of the studied catchments consist of debris-covered ice (Scherler et al., 2018), and their influence on the streamflow was ignored. In fact, a simple inclusion of the melt inhibiting effects of the debris layer, as done in some of the existing model studies (Table R1), may not necessarily lead to improved description of reality. For example, the strong melt enhancements at the ice-cilffs/ponds on the debris-covered surface (e.g., Miles et al., 2022) are usually ignored in the above models. The available estimates of the extent and thickness estimates have large uncertainties as well.

The variability of wind and gravity driven snow-redistribution in the rugged Himalayan topography are also difficult to capture in any coarse-scale model like the present one.

We shall add these details/arguments in the revised version.

Only two parameters of the hydrological model are calibrated, the ones that affect the water balance the most strongly (melt factor for ice and precipitation scaling factor). The calibration is on streamflow and glacier mass balance; there is an empirical weight factor to combine the performance with respect to both quantities; despite a clear lack of giving any formal statistical framework, the parameter estimation approach is called a Bayesian Inference.
High Himalayan catchments, like the one studied here, are data-sparse. So we use a minimal set of two calibration parameters to avoid over-fitting (L 172). All the other parameters were assigned reasonable values, and the corresponding sensitivity was shown to be small (L 210–213). We believe calibrating a small subset of the model parameters is a common compromise in hydrological modelling (eg, Table R1).

The empirical factor was included to give similar weightage to both the objectives while optimising (L 197-198). The factor of half can be absorbed in σ_b, then it is equivalent to using the 80 percentile of the set of uncertainty values (Table S3) instead of the median. We shall expand the relevant discussion, and include this point.

The present approach of using uniform prior distributions for the two fit-parameters (L 185), using Bi-variate Gaussian distribution for the residues associated with the two (independent) observed datasets (L 189), and obtaining a posterior probability distribution for the models (shown explicitly in Fig 4a, 4b), is a Bayesian approach. Here we have followed the formulation used in Rounce et al. (2020), Werder et al. (2020), etc. from the glaciology literature which we are familiar with.

Accordingly, I am rather skeptical about the added value of this model study; I think that this is essentially a modelling exercise without clear indications that it actually corresponds to how nature reacts;
We beg to differ due to the following reasons:

- Given the importance of the data-sparse catchments for us, modelling exercises like the present one are important.
- We have utilised all the available hydro/metero/glacio-hydrological data (even though limited) to improve the representation of natural processes.
- We incorporate reasonable representations of the most important glacio-hydrological processes, following the available studies in the literature (Table R1).
- We have acknowledged the model limitations, which are often shared with most of the studies from the region as shown (Table R1). (though we admit that the reiewer has pointed out a few points where more discussion would have been beneficial).
- The detailed discussions of the consistency of our result with existing modeling studies (runoff: L 318–324, regional glacier mass balance: Table S3, catchment runoff sensitivities: Table S6, glacier runoff sensitivities: Table S7, glacier mass balance sensitivities: Table S8) support the validity of our result.
- The detailed discussions of the underlying processes driving the sensitivity pattern reported, provide support in favour of the validity of the results (Sect 4.5–4.6).

moreover, the conclusion is very general with new insights that can be inferred from general process knowledge such as e.g. the sentence “the temperature sensitivity of the glacier runoff and the precipitation sensitivity of the off-glacier runoff are critical determinants of the future changes of summer runoff and its variability in these two catchments”. I therefore recommend rejection of this version.
Again we beg to differ as,

- We are not aware of any study in the Himalaya where the above pattern of sensitivity has been discussed based on either process understanding or quantitative modeling of glacierised catchments.
- We are not aware of any studies where the implication of the above property on the variability and change in runoff of any Himalayan catchment have been analysed.

The work could become more valuable if it was more critical about the value of the model, if it discussed what we miss with the simplifications and if it provided more insights in what we can learn from the two different types of catchments.
The focus of the work is to understand the pattern of runoff sensitivities of the two Himalayan catchments and the role of glaciers in that. Given the existing glacio-hydrological models for Himalayan catchment, the present model appears to be a rather reasonable model (Table R1). However, as the reviewer has aptly pointed out, some of the assumptions could have been explained better, and there are a few missing details. We shall make appropriate corrections.

Detailed comment:
what do you mean by runoff? there are usages of this term where it does not include groundwater-fed baseflow; accordingly: if we mean total flow leaving a catchment, we might want to use streamflow;
We are happy to review the usage of runoff, and replace it with streamflow wherever needed. However, we have encountered similar usage of runoff in cited literature (e.g. Khadka et al 2014, He and Pang 2015, Bhattacharya et al 2019).

it needs to be very clear also what is meant by “glacier runoff”: runoff generated in the glacier-covered part of the catchment? including baseflow? Including runoff from lateral moraines that are not glacier covered?

Glacier runoff is unambiguously defined in L 159-160. We shall add here that baseflow is ignored (see TableR1, L 164). Lateral moraines, lateral basins, etc are not usually considered as part of the glacier.

Methods, calibration: I do not think that 5% or 10% error on summer streamflow observations is a realistic value; since summer is the period of high flow, this values is certainly, much higher;

The uncertainties in the observed runoff could not be accessed. We assumed twice the value that was reported in another Himalayan catchment (L 193). We shall acknowledge the possibility that the error could be higher.

furthermore, in the chosen formulation, the error should correspond to the total model error and not just to the observational error (see e.g. an Bayesian inference paper by Dmitri Kavetski);

We agree that ideally $\sigma^2_{tota} = \sigma^2_{model} + \sigma^2_{observation}$. However, as we don't have estimates of model errors, incorporating those using, say, an uniform prior (eg, Rounce et al (2020)), would expand the dimension of the model space to 4. To avoid that complication, we ignored the model error (e.g., Warder et al 2020). We shall discuss this point in the revised text.

The paper referred to, discusses methods for considering noise in individual rainfall events. In this paper, we do acknowledge that input rainfall is a major source of error, and employ a simple constant multiplicative factor to correct for it. Given that we do intend to capture the total summer runoff and not the catchment response to the individual rain events, this simplification may not be entirely unreasonable, and is in line with the existing studies (Table R1).

the Bayesian formulation for the mass balance is based on very few obs. values;

We have used all the available mass balance estimates (L 194).

what assumption do you make about the distribution of the residuals for streamflow and mass balance? i.e. what motivates the chosen form of the likelihood?

They are assumed to be from a Bivariate Gaussian distribution (eq 2), which is a commonly used assumption (e.g., Raounce et al 2020, Warder et al 2020). We also assume them to be uncorrelated, which we have confirmed using the sampled posterior distribution. We shall add these details.

How did you compute the posterior (you did not use any sampling method that would yield a sample for the posterior; I guess you did some kind of rescaling?)

The posterior distribution is the truncated bivariate Gaussian shown in Fig 4a,b (We apologise for mislabeling the colormap label as P(θ|d), it should be P(d|θ)). Since we have a two-dimensional model space, we could map out the distribution over the entire domain with 11X29 model runs, and no sampling algorithm was not needed.

Methods, other: i) what is the used temporal time step of the VIC model on the glacier part ?

As stated in L 116, we ran the VIC model at hourly time steps in both glacerised and nonglacierised parts of the catchment.

why is it reasonable to keep the bias correction constant in space? I would expect that biases depend on elevation?

We corrected ERA5 precipitation with an elevation-independent scale factor, which was calibrated using the observed runoff and the decadal regional glacier mass balance. Due to complex topography in the Himalaya, even within a single grid box, an elevation-dependent correction may not be a better representation of the precipitation pattern (e.g., Johnson and Rupper 2020). Please see the discussion in L 105-108.

Why does the glacier melt model not use the energy-balance approach?

Due to scarcity of field data, we chose a minimal DDF model for the ice melt, following most of the hydrological studies in the region (see Table R1).

Is the glacier melt coded by the authors of the study or someone else?
We do not understand the relevance of the question.
We have given due credit to any piece of code used in this paper that is not written by us, if that is what is being referred to.

Methods: the computation of glacier mass balance sensitivity is not clear to me; did you run the model with modified precipitation input?
As stated in L 248–249, we did not perturb the precipitation, but used the interannual variability of mass balance to compute the sensitivities (just as we do for the runoff). We shall make this explicit in the revised text.

Methods: how did you compute the deltaP and deltaT values (anomalies)?
As stated in L 236–238 : "*We also considered the runoff from glacierised part of the catchments* $Q^{(g)} = Q_0^{(g)}+\delta Q^{(g)}$*, and that from the non-glacierised part of the catchments* $Q^{(r)} = Q_0^{(r)} + \delta Q^{(r)}$ *. Here, the notations* Q_0 *and* δQ *denote the long-term mean and the anomaly for a given year, respectively.*"

Results: please reword "the present calibration strategy resolved the equifinality problem that is usually encountered while calibrating glacio-hydrological models using only discharge data"; using two data sets does not remove equifinality; you built a single performance metric with an empirical factor to sum up two performance measures and then you report only the best value; it does not mean that there is no Equifinality
We apologise for the confusing sentene.
We used equifinality in a restricted sense where the same discharge is produced in a heavily glacierised catchment, say, by either melting more ice (via a higher DDF) or by having a higher precipitation (via a higher α_P). This is just as discussed in the cited ref. Azam and Srivastava (2020), where the modeled runoff fitted the observations equally well even as the relative contributions of precipitation and glacier melt varied. This is the specific problem that is resolved with our approach.
In the revised text we shall calrify that we are talking about the equifinality, i.e., the family of non-unique solutions to the problem of fitting the observed runoff, in the context of the 2-d model space (DDF, α_P) that we (or Azam and Srivastava (2020) etc.) have considered.

Fig. 1: the legend (not the caption) should also include what the dashed lines are
We shall update the legend.

Fig. 3: the glacier scheme is probably wrong, the text states that there are two linear reservoirs, rainfall is missing
We shall revise the flowchart to correct this error. Rainfall was indeed considered as given in the text (L 159).

Fig. 6: what is the y-axis (equation 1 is the calibration equation, something is wrong?)?
We shall revise the figure.

Fig. 7: should be improve, I cannot see much about the circles
We shall revise the figure.

Table 1: is there snowfall occurring in summer and if yes, what is the amount of sommer snowfall? Caption could say how summer is defined (it is in the text though);
The monthly snowfall is shown in Fig 1c.
Snowfall constitutes 14% and 5% of summer precipitation in Chandra and upper Dudhkoshi catchments, respectively.

how high is ET?
ET is about ~⅓ of the annual precipitation in these catchments (Fig S7).

The following reference which is certainly relevant is missing: van Tiel et al : https://hess.copernicus.org/articles/25/3245/2021/; probably their review on glacier

modelling is also relevant:
https://wires.onlinelibrary.wiley.com/doi/10.1002/wat2.1483
We shall add the above reference in the revised version.

References

Azam, M. F., and Srivastava, S. (2020). Mass balance and runoff modelling of partially debris-covered Dokriani Glacier in monsoon-dominated Himalaya using ERA5 data since 1979. Journal of Hydrology, 590, 125432.

Bhattacharya, T., Khare, D., and Arora, M. (2019). A case study for the assessment of the suitability of gridded reanalysis weather data for hydrological simulation in Beas river basin of North Western Himalaya. Applied Water Science, 9(4), 1-15.

He, R., and Pang, B. (2015). Sensitivity and uncertainty analysis of the Variable Infiltration Capacity model in the upstream of Heihe River basin. Proceedings of the International Association of Hydrological Sciences, 368, 312-316.

Johnson, E., and Rupper, S. (2020). An examination of physical processes that trigger the albedo-feedback on glacier surfaces and implications for regional glacier mass balance across high Mountain Asia. Frontiers in Earth Science, 8, 129.

Khadka, D., Babel, M. S., Shrestha, S., and Tripathi, N. K. (2014). Climate change impact on glacier and snow melt and runoff in Tamakoshi basin in the Hindu Kush Himalayan (HKH) region. Journal of Hydrology, 511, 49-60.

Miles, E. S., Steiner, J. F., Buri, P., Immerzeel, W. W., and Pellicciotti, F. (2022). Controls on the relative melt rates of debris-covered glacier surfaces. Environmental Research Letters, 17(6), 064004.

Rounce, D. R., Khurana, T., Short, M. B., Hock, R., Shean, D. E., and Brinkerhoff, D. J. (2020). Quantifying parameter uncertainty in a large-scale glacier evolution model using Bayesian inference: application to High Mountain Asia. Journal of Glaciology, 66(256), 175-187.

Scherler, D., Wulf, H., and Gorelick, N. (2018). Global assessment of supraglacial debris‐cover extents. Geophysical Research Letters, 45(21), 11-798.

Werder, M. A., Huss, M., Paul, F., Dehecq, A., and Farinotti, D. (2020). A Bayesian ice thickness estimation model for large-scale applications. Journal of Glaciology, 66(255), 137-152.