Re-review: I carefully read the response and the revised paper. I have some additional comments. Below, the red parts are my original comments. The black are the author's answers. In yellow are my new comments.

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 glacierisedHimalayan 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.

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 that our glacier model is inadequate. As shown in Table R1, the present model compares favourably with those used in the existing studies in the region.

I would like to apologize for imprecise reading; the use of two linear reservoirs was indeed stated in the original paper, but I overlooked it. This could be made clearer in the model scheme. Besides: are glacier melt, snowmelt and rainfall summed up before entering the two linear models or are they routed separately? The reference probably says it but would be good to have here)

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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 186). All the other parameters were assigned reasonable values, and the corresponding sensitivity was shown to be small (L 352–358). We believe calibrating a small subset of the model parameters is a common compromise in hydrological modelling (eg, Table R1).

We modified the conditional probability (Eq 2) by including both the model and observation errors (Eq 2) and by removing the empirical weight of 1/2. Please refer to the revised Sect. 3.2.3 for the details. The present approach of using uniform prior distributions for the two fit-parameters (L 191), using a Bi-variate Gaussian distribution for the residues associated with the two (independent) observed datasets (L 206), and obtaining a posterior probability distribution for the models (shown explicitly in Fig 4a, 4b), is a Bayesian approach to the best of our understanding.

Thanks for having clarified and corrected the conditional probability formulation, accounting now for the model residuals (instead of simply the observational error) and removing the factor ½. Because of the omission of the model residuals and the empirical factor, the chosen approach was unclear and appeared to me an ad-hoc calibration criterion; it is clearer now. As far as I see, it is not mentioned in the revised version that you make an explicit assumption about the distribution of the model residuals (only in the response), which might be good for non-expert readers. It is indeed common to make the normality assumption for discharge residuals; I am not sure if there are any examples of assumption about mass balance residuals (this is a detail of course).

Furthermore, I now understand that you sampled the full 2D parameter space but kept the corresponding residual variance constant to a fixed value to compute the full posterior probability. In my experience, the residual variance is often sampled along with the parameters. The chosen solution is pragmatic (especially given the few mass balance observations) but not the standard procedure.

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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 quantified using glaciohydrological modeling and explained in terms of the underlying processes.
- 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.

I still maintain my comment. The take home messages of the analysis read as follows (full relevant text of the paper copied below):

"The precipitation sensitivities of the summer runoff of the non-glacierised parts of the catchments are high, but those of the glacierised parts are negligible. In contrast, the temperature sensitivities of summer runoff of glaciers are high, but those of the non-glacierised parts are negligible. As a consequence, 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. "

We could have guessed that glacier runoff is temperature sensitive (in a model that uses only temperature as driver for melt) and that the off-glacier parts are sensitive to precipitation input. The abstract gives some more interesting text but the conclusion is definitively not yet there.

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

Due to the 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).

Ok, but the snowmelt model of VIC already uses an energy balance approach, i.e. the energy balance approach is set-up for snow melt, this means that all data is available? Or do I get this wrong? Accordingly, my question still holds: why is the energy balance approach feasible for the snow on the glacier but not for the ice melt under the snow? If this is simply for practical reasons (coding), it is perhaps worth mentioning.

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.

I asked the question because it is unclear if the code is available, which is always interesting for other users.

Methods: the computation of glacier mass balance sensitivity is not clear to me; did you run the model with modified precipitation input?

As already stated in L 270, 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), L 270: "*Apart from the sensitivities of summer runoff, we also computed the precipitation and temperature sensitivities of glacier mass balance using the* *corresponding simulated interannual variability over the period of 1980–2018*.

I still do not understand. Please not that my question arose due to the sentence following the one above.

Apart from the sensitivities of summer runoff, we also computed the precipitation and temperature sensitivities of glacier mass balance using the corresponding simulated interannual variability over the period of 1980–2018. The precipitation sensitivity of glacier mass balance was defined to be the mass-balance change due to a 10% change in precipitation following the convention used in the literature (e.g., Wang et al., 2019).

how high is ET?

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

Ok, is this in the main text now, I think this is highly relevant.

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