We again thank both the reviewers and the editor for their careful comments and suggestions. Below we provide our point-by-point response in **blue** text color.

Authors' **response** to the **comments** by Reviewer 1

The authors have thoughtfully revised the paper and I think it can be published.
Thank you for the positive comment on the manuscript.

But I think the figures and tables should be improved before publication. Here are the details:
1. The legends of all the shapes should be pointed out in the figure and the legend for all subplots should better place outside rather than inside an individual subplot (e.g., Fig 1b-d, Fig 7, Fig, 8, Fig 9,), making the reader easier to get the information from the figure.
We changed the legend's position as suggested.

2. The font in Table 1 should be uniform.
We used the format prescribed in HESS latex template (https://www.hydrology-and-earth-system-sciences.net/submission.html#templates) to prepare the table.

Authors' **response** to the **comments** by Reviewer 2

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.
We thank the reviewer for the careful comments. Please see our replies below.

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)
Thank you for pointing out this ambiguity in our statement. We have modified it as (L163): "*The snow melt, ice melt, and rainfall on the glaciers routed using a linear reservoir model (Hannah and Gurnell,2001), obtained the glacier runoff. The model used two parallel reservoirs: a slow reservoir with time constant K_{slow} for routing the snowmelt, and a fast reservoir with time constant K_{fast} for routing the sum of the icemelt and the rainfall (Hannah and Gurnell, 2001).*"

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).
Thank you for pointing out this critical limitation of our formulation in your earlier comments, which helped improve our understanding. We have now explicitly stated the assumption in the main text L206: "*The conditional probability p(d|θ) of the observations d given the model parameter θ was assumed to be a bivariate normal distribution (e.g., Rounce et al., 2020; Werder et al., 2020), i.e., a normally distributed residuals for both discharge and glacier mass balance...*"
We confirm that both of the cited references, Rounce et al., 2020 and Werder et al., 2020, assumed normally distributed mass-balance residuals.

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.
We now make this point explicit L215: "*…incorporated the errors in the model (σ^{mod}) and the observation (σ^{obs}). Each of these errors was assumed to be a constant having the following values. σ_Q^{obs} was…*"

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.
We re-written the 'Summary and conclusions' section' with additional statements related to the implications of the observed pattern of sensitivities on runoff variability.

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.
We now clarify that (L159): "*A minimal temperature-index model (Hock, 2003) was chosen to simulate the ice melt over the corresponding snow-free areas. This one-parameter model is easy to calibrate and is expected to work well for ice cover than for snow cover, due to a relatively low seasonal variability of ice albedo (Hock, 2003).*"

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.
The developed glacier model and glacier routing model are available in this public domain: https://osf.io/7une2/.
All the observed hydrometeorological will be made public after publication.

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.
We apologise for the unclear statement, which is modified to L273: "*On the glacierised part, we estimated the mass-balance sensitivities to the corresponding temperature and precipitation forcing over the period of 1980–2018. The sensitivities were computed by linearly regressing the modelled anomalies of the mass-balance to those of the annual precipitation and summer air-temperature."*

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.
We thank you for raising the point.