Response to reviewers

We would like to express our gratitude to the reviewer for the detailed review and constructive feedback. We provide below our detailed answers (in blue) to the remarks made by the reviewer (in black). Line numbers refer to those from the submitted manuscript.

GENERAL COMMENTS

This study presents a simulation-based analysis of the climatological snow mass budget and its trends in the years 1962-2019 in the vast Tibetan and Pamir Plateaus in high-mountain Asia. The main findings include the overall smallness of the snow water equivalent SWE (annual area mean only 0.85 mm), the dominance of sublimation over snowmelt as the primary sink of snow (about 62 % of the total), and a near-zero annual and area mean trend in SWE in 1962-2019, although more than two thirds of the area has experienced a decrease in SWE.

Due to the very limited coverage of in-situ observations and the difficulties in remote sensing of snow conditions in rugged terrain, modelling is necessary for deriving credible estimates of SWE and the snow mass budget in the Tibetan and Pamir Plateaus. Modelling always includes uncertainties, both from the input data used and from the parameters and the structural choices in the models used. The latter are especially difficult to estimate without repeating the research with a completely different models, which is unfeasible in practice. Nevertheless, this study makes a commendable effort in deriving input data that are as free as possible from systematic errors (in particular, the undercatch of precipitation), in taking into account the large sub-grid scale topographic variations (by running the VIC model separately for each 100-m elevation band in each grid cell) and in optimizing the parameters of the model using machine-learning methods. The resulting framework is somewhat intractable, but the results appear to be in reasonable agreement with available station observations and earlier studies for individual locations. Some sensitivity analysis focusing on the input data of the VIC model is also provided.

Despite inevitable uncertainties (among which a 62 % change in simulated SWE in response to a 10 % perturbation of precipitation is a striking example), I find this research well-done, important and worth publication. However, I still have many comments on the details, mostly on the presentation but also on the science.

We sincerely thank the reviewer for the insightful assessment of our manuscript and for the constructive scientific and editorial suggestions. The comments will help us improve both the interpretation of the snow mass budget and the clarity of its presentation. In the revised manuscript, we will distinguish snowpack state variables from fluxes, clarify the scope of the forcing-sensitivity experiments, correct the treatment of uncertainty in long-term means, and revise the figures and tables accordingly.

DETAILED COMMENTS

1. L34-64. The beginning of the Introduction is very general. You could reduce the general text in the interest of conciseness and focus on topics that are directly relevant to your study.

   We agree. We will substantially shorten the opening of the Introduction, remove general descriptions and peripheral socioeconomic examples, and focus directly on snow storage, snowmelt, sublimation, and their climatic controls on the TPP.

2. Winter Olympics is somewhat too much a "tip of an iceberg" example.

   We agree that this example is peripheral to the scientific motivation. We will delete the sentence referring to the Winter Olympics.

3. L53-54. The suppression of outgoing longwave radiation is a consequence of low surface temperature, not a cause of it.

   We agree with the reviewer's physical interpretation. The original sentence will be removed as part of the shortened Introduction. If a brief explanation is retained elsewhere, it will be revised as follows: Snow's high albedo reduces absorbed solar energy; because snow surfaces are often colder than adjacent snow-free surfaces, they generally emit less outgoing longwave radiation.

4. Start a new paragraph from "The sensitivity".

   We agree. The discussion of climatic sensitivity will begin with a separate paragraph.

5. L68-69. This list is not internally consistent. Snowpack depth and SWE are measures of the amount of snow, all the rest budget terms that change the amount of snow.

   We agree and will distinguish state variables from fluxes.

6. A better reference for GlobSnow: Luojus, K., Pulliainen, J., Takala, M. *et al.* GlobSnow v3.0 Northern Hemisphere snow water equivalent dataset. *Sci Data* **8**, 163 (2021). https://doi.org/10.1038/s41597-021-00939-2

   We thank the reviewer for suggesting this more appropriate dataset reference. We will replace the GlobSnow citation accordingly.

7. L364-365. Two interannual standard deviations strongly overestimates the sampling uncertainty in long-term mean values. If snow conditions in individual years can be considered independent (which should be a reasonable assumption in areas where snow does not survive over the summer), then the sampling uncertainty in the long-term mean is inversely proportional to the square root of the number of years.

   We agree. Two interannual standard deviations characterize the year-to-year variability of the simulated annual series and do not represent the sampling uncertainty of the 58-year mean. We will use the inverse of the square root of the number of years.

8. Figure 4. Add headings above the figure panels to facilitate faster understanding ("Annual snowfall" in (a) etc). In addition, the area means over the entire TPP could be added to the maps, to make it easier to see the magnitude differences that are hidden by different colour scales.

   We agree. We will add concise titles to all panels and annotate each spatial panel with its TPP-wide area-weighted mean or mean trend.

9. Figure 5. Same comments as for Figure 4 above.

   We agree and will make the same changes to Fig. 5.

10. This is a surprising definition. SWE only contributes to runoff when it melts. For what purpose does it count as an available water resource before melting? Also, if the same snow contributes to the resource both when on ground and when melting, it is counted twice.

    We thank the reviewer for this important comment. We agree that SWE contributes directly to runoff only after melting. However, our definition considers snow water resources in both stored and released forms.

    Here, a state variable describes the amount of water stored within a system at a given time, whereas a flux describes the transfer of water into or out of that system over a specified time interval. SWE is therefore a state variable representing the water equivalent stored in the snowpack at a given time. Annual mean SWE is the temporal average of this standing storage over the snow year; it is not an accumulated annual flux. In contrast, annual snowmelt is a flux integrated over the snow year, representing water released from the snowpack through melting.

The eventual fate of the water represented by SWE has not yet been determined. It may subsequently melt, sublimate, or remain stored. We therefore regard annual mean SWE as an indicator of potential snow-water availability, whereas annual snowmelt represents realized meltwater release. The model does not count the same water twice at a given time as both SWE and snowmelt are separately simulated and output by the model at the same given time step.

We acknowledge, however, that the original wording in Lines 494–495 did not sufficiently distinguish the state variable from the accumulated flux and could be interpreted as a strict annual mass-balance partition. The reported value of 105.57 ± 26.48 Gt was intended as a descriptive indicator combining realized snowmelt supply and potential snow-water storage, rather than as a conservative annual flux or a direct contribution to annual runoff. We will revise Lines 494–495 to clarify these definitions and interpretations.

11. L559-560. Refer to Figure S6 where these results are shown.

We agree. The comment refers to the Discussion statement that the five SWE-evaluation metrics are satisfactory. We will add an explicit reference to Fig. S6 at Lines 559–560.

12. 578-581. What do you mean by "each snow event" in the calculation of the annual accumulated maximum SWE? If SWE decreases after a snowfall event but does not drop to zero before the next snowfall event, then summing the two SWE maxima without subtracting the minimum SWE in-between will overestimate the total water input from the two snowfall events.

Thank you for raising this important point. We agree that the phrase "each snow event" in the original manuscript was ambiguous. However, our calculation does not sum the local SWE maxima associated with every individual snowfall event. We followed Gao et al. (2023), who defined the metric as the "cumulative value of the maximum SWE per snow cycle" (Sect. 2.3). Their Fig. 2 distinguishes a single continuous snow cycle, which may contain several increases and decreases in SWE, from multiple snow cycles separated by snow-free conditions.

Consistent with this definition, we identified each snow cycle as a continuous snow-cover episode bounded by snow-free periods. If SWE decreased but remained above zero before a subsequent snowfall, the two accumulation phases were treated as part of the same snow cycle, and only the maximum SWE of the entire cycle was used. Therefore, our method did not result in two separately summed maxima in our calculation, and the remaining SWE was not counted twice.

We will change the terminology from "each snow event" to "each snow cycle" and add an explicit description of the cycle-identification and accumulation procedure. We will also clarify that this quantity is used as an annual cumulative maximum-SWE index following Gao et al. (2023), rather than as a direct measurement of total snowfall input.

13. L594-595. Explain the meaning of the first column numbers (1-11) also in the table caption.

We agree and will define the site numbers in the table caption.

14. L629-630. Please indicate the section of the Supplementary material where the details are given.

We agree and will add the specific section reference.

15. L632-633. SWE responses to 10 % perturbations in precipitation The word "changes" should be avoided because it is easily mixed

with the long-term trends discussed in the previous sentence.

We agree. We will use "forcing response," "sensitivity," or "elasticity" for the response metrics and reserve "trend" for the historical evolution during 1962-2019.

16. L637-639. The relative importance of the different variables cannot be evaluated from the idealized 10 % sensitivity tests alone, without knowing the actual magnitude of uncertainty / variability in the four perturbed variables. More representative values (regarding the contributions to interannual variability of SWE) could be derived by multiplying the sensitivity ratios from the 10 % tests with the observed interannual standard deviation of each four input parameters.

We thank the reviewer for this thoughtful comment. We agree that idealized perturbation experiments alone would not be sufficient to attribute the interannual variability of SWE to the variability of the meteorological forcing variables. However, our experiments were not designed as uniform 10% perturbation tests or as an attribution of SWE interannual variability.

We used an elasticity-based trend attribution framework with detrended forcing scenarios. In the reference simulation, the long-term trends were removed from precipitation, maximum temperature, minimum temperature, and wind speed. In each sensitivity simulation, the historical time series of one forcing variable was restored, while the other three variables remained detrended. For example, the precipitation scenario used historical precipitation together with detrended maximum temperature, minimum temperature, and wind speed. Similarly designed simulations were conducted for the other three forcing variables.

The difference between each single-historical-forcing simulation and the all-detrended reference simulation represents the modeled SWE response associated with the historical change in that forcing variable. The classical elasticity formulation of Schaake (1990) was then used to normalize the SWE response by the corresponding forcing change, yielding a dimensionless measure (termed as elasticity) that allows the relative effects of the four forcing variables to be compared (see supplementary material No.3). The difference between our and conventional elasticity calculations is that we used historical trends to perturb the forcing, which is more realistic, whereas conventional elasticity calculation uses arbitrary values to perturb forcings, e.g., 10% change in precipitation or wind speed, 1°C change in temperatures.

Therefore, multiplying the elasticity values by the interannual standard deviation of each forcing variable would address a different question: the potential contribution of forcing variability to SWE interannual variability. Our analysis instead evaluates the relative effects of the historical long-term changes in precipitation, maximum temperature, minimum temperature, and wind speed on simulated SWE.

We appreciate the reviewer's thoughtful suggestion but we feel that the suggested method of multiplying the sensitivity ratios by the observed standard deviations would answer a different question than what we intended and would also provide only a first-order linear approximation. It would not account for correlations and interactions among the forcing variables, nonlinear SWE responses, or possible differences between positive and negative perturbations. Therefore, it would not reveal a robust attribution of SWE variability.

Nonetheless, we agree that the wording in Lines 637–639 should clearly reflect the purpose of the experiment. We will revise the manuscript to clarify the experimental design, the interpretation of the elasticity values, and the distinction between trend attribution and interannual variability attribution.

17. Figure 7 is confusing. Panel (a) shows sensitivities of area mean SWE to long-term trends in different parameters, whereas (b)-(e) show the spatial distribution of the sensitivity to the idealized 10 % perturbations in these variables. It would seem more logical to

show in (b)-(e) the sensitivities of SWE to the local precipitation, Tmax, Tmin and wind speed trends instead. If the figure is retained as it is, at the very least the difference in interpretation between panels (a) and (b)-(e) should be stated explicitly.

We thank the reviewer for this helpful comment. Figure 7 was intentionally designed to present two complementary aspects of the SWE response to meteorological forcing trends. Panel (a) quantifies the TPP-wide mean SWE response to the historical changes in each forcing variable during 1962 – 2019, thereby showing the regional-scale temporal effect. Implicitly, the units of Panel (a) are % per mm, °C and m/s per year. Panels (b)–(e), in contrast, map the elasticity of SWE to long term changes in each forcing calculated using elasticity formulation of Schaake (1990), thereby revealing the spatial heterogeneity of unitless SWE elasticity for spatial comparison.

We consider it useful to retain these panels in the same order because they answer two closely related but logically following questions: panel (a) shows how the regional mean SWE responds to the historical forcing changes over time, while panels (b)–(e) identify where SWE is more or less responsive to a normalized change in each forcing. We therefore retain the current structure of Figure 7. Nevertheless, we agree that the distinction between panel

(a) and panels (b)–(e) was not stated sufficiently clearly. We will explicitly describe their different interpretations in the main text and figure caption.

18. Table 3. Please also give the contributions of the four variables to the SWE trends in the different basins, either in this table or in a separate table.

We thank the reviewer for this suggestion. We would like to clarify that Table 3 reports the elasticity or normalized sensitivity of basin-mean SWE to changes in precipitation, maximum temperature, minimum temperature, and wind speed, rather than their relative contributions to the simulated SWE trends. To address the reviewer's suggestion, we have used the grid-cell attribution results shown in Fig. S10d to calculate the basin-scale relative contributions by averaging the contribution fractions across all grid cells within each basin (Table 4). Because the attribution framework in Fig. S10d considers precipitation, mean air temperature, and wind speed, the resulting basin-level contributions of these three drivers will be provided in a separate table and summarized in the Results (Lines 400–405). We will also clarify the distinction between the normalized sensitivity analysis in Table 3 and the trend-attribution analysis in Fig. S10d.

Table 4. Mean relative contributions (%) of snowy-day precipitation, mean air temperature, and wind speed to the explained variation in annual SWE in the 12 basins during 1962–2019.

Basin	Precipitation (%)	Mean air temperature (%)	Wind speed (%)
Upper Yellow	42.89	32.39	24.72
Upper Yangtze	31.44	37.48	31.09
Upper Mekong	28.28	37.51	34.21
Upper Salween	35.97	37.27	26.76
Brahmaputra	32.40	40.56	27.04
Upper Ganges	33.04	37.81	29.15
Upper Indus	48.67	33.08	18.25
Upper Amu Darya	49.90	33.55	16.55
Upper Tarim	52.31	27.47	20.22
Inner TP	34.84	36.12	29.05
Qaidam	47.64	30.52	21.84
Qilian Mountain	41.14	34.88	23.97

19.L704-705. Probably true but not rigorously quantified (cf. comment 16 above).

We thank the reviewer for raising this point. We would like to clarify that this statement refers specifically to the elasticity of SWE calculated using the ratios of normalized changes of SWE to normalized changes of meteorological forcings, rather than to the contributions of these forcings to the observed SWE trend or interannual variability. The sensitivity coefficients show that annual SWE responds more strongly to a prescribed proportional change in precipitation than to equivalent proportional changes in maximum or minimum temperature. We have revised the sentence to make the experimental context clearer and avoid possible confusion between sensitivity and contribution.

Revised text: 'Overall, annual SWE is most sensitive to prescribed proportional changes in precipitation and is less sensitive to equivalent proportional changes in maximum and minimum temperature.'

20. Figure S3. Please explain the meaning of the red lines.

Thank you for pointing this out. Red lines are fitted regression lines. When the red line is near the 1:1 line (black), the estimation is found to be matching the observation. Inspection of our plotting code revealed that the regression direction had been reversed: observed SWE was regressed against estimated SWE, although observed SWE was plotted on the x-axis and estimated SWE on the y-axis. We have corrected the regression by fitting estimated SWE against observed SWE and have redrawn the red solid lines in all panels of Fig. S3. The black dashed lines represent the 1:1 correspondence. This correction changes only the fitted regression lines (red lines); the scatter points, correlation coefficients, and RMSE values remain unchanged because the same paired data are used.

21. Figure S7. Please use the same colour scale in all three columns. Furthermore, the scale for Pbias should be symmetric with respect to zero, that is, the change from red to blue should occur at zero

bias.

We agree. We will replot Figure S7 using one common colour range for each evaluation metric across the calibration, validation, and test columns. For Pbias, we will use a diverging colour map centred at zero, with symmetric limits defined by the largest absolute Pbias value across all three datasets. These changes will allow direct visual comparison among the three groups of stations.

22. Figure S10d. Absolute contributions of precipitation, temperature and wind speed to the SWE trends would be easier to understand than the map of relative contributions.

We appreciate the reviewer’s suggestion though we would respectfully argue that showing relative contributions in Fig. S10d precisely serves our purpose of comparing the relative explanatory importance of precipitation, air temperature, and wind speed in affecting the SWE trends within the multiple linear regression framework. The three predictors have different physical units and numerical scales: precipitation (mm), temperature (℃), and wind speed (m/s). Their raw trends or unstandardized regression coefficients are therefore not directly comparable and cannot be plotted on one regional map for comparison. In our analysis, the relative contributions were derived by decomposing the coefficient of determination (R^2) following the Shapley-based method of

Israeli (2007). This procedure places the contributions of the three predictors on a common, dimensionless scale and quantifies the proportion of the explained variation attributable to each predictor.

An absolute contribution expressed as an SWE trend, for example in mm/decade, would represent a different attribution quantity. Its calculation would require combining the regression response with the observed trend of each predictor or conducting separate counterfactual simulations. Such values cannot be obtained directly from the present (R^2)-decomposition analysis. Moreover, the absolute SWE trends are already presented in Fig. 3b, while the corresponding trends in precipitation, temperature, and wind speed are shown in Figs. S10a–c.

To avoid ambiguity, we will retain Fig. S10d but clarify in the Methods and figure caption that the mapped values represent dimensionless relative contributions to the explained variance of SWE for comparison, rather than absolute contributions to the magnitude of the SWE trend.

23. Figure S11. Add area mean values to the maps.

We agree. We will calculate the TPP-wide area-weighted mean snowfall for each month and add the corresponding value to every panel.

24. Table S2. For Daily maximum, minimum and mean temperature,

bias in absolute (C) units would be more informative than percent bias.

We agree. We will replace the percentage bias for daily maximum, minimum, and mean air temperature in Table S2 with the corresponding absolute bias in °C.

TECHNICAL COMMENTS

1. L202 and later. Replace "warming-up period" with "spin-up period".

   We agree and will replace "warming-up period" and "warm-up period" with "spin-up period" throughout the manuscript and Supplement.

2. The sensitivity of VIC to the values of snow parameters was investigated?

   We thank the reviewer for pointing out this imprecise wording. Yes, we investigated the sensitivity of VIC-simulated SWE to variations in the values of the snow-related parameters, rather than the "sensitivity of the parameters" themselves.

3. L280 and later. The impactful snow parameters / the most important parameters? "A sensitive parameter" means a parameter whose value is strongly sensitive to assumptions etc., rather than a parameter whose value has a large impact on the model results.

   We thank the reviewer for raising this terminological point. We respectfully retain the term “sensitive parameter.” In variance-based sensitivity analysis, including the Sobol' method used in this study, a sensitive parameter commonly refers to an input parameter whose variation produces a relatively large response in the model output or contributes substantially to the variance of the selected model-performance metrics. Our use of this term therefore refers to the sensitivity of VIC-simulated SWE to variations in the parameter values; it does not imply that the parameter values themselves are sensitive to assumptions.

This usage is also consistent with the terminology adopted in previous snow-model sensitivity studies, including Houle et al. (2017), which is cited in the manuscript. Nevertheless, to eliminate any possible ambiguity, we will explicitly define “sensitive snow parameters” at its first occurrence and clarify the note below in Table S5. We will retain the term throughout the manuscript because it directly corresponds to the Sobol' sensitivity-analysis framework.

4. L431 and later. decreasing trend

   We agree and will correct the wording.

5. There is no evaporation in Figure 4 (and probably should not be, since only liquid water evaporates).

   We appreciate this comment. We agree that evaporation refers to the phase transition from liquid water to water vapour, whereas the direct loss of solid snow or ice to the atmosphere is sublimation. However, liquid meltwater can be retained at or near the snowpack surface during melting periods. Therefore, the term "evaporation" used in our study refers specifically to the evaporation of this liquid meltwater and not to the direct evaporation of solid snow.

More precisely, the quantity presented in Fig. 4 represents the net water-vapour loss from the snowpack surface, including sublimation from solid snow or ice and, when liquid meltwater is present at the surface, evaporation from the liquid phase. These two contributions are treated as a combined surface vapour flux and are not presented separately in Fig. 4. We agree that our original terminology could lead to misunderstanding and we will clearly describe the snowmelt water evaporation in the text.

6. Supplementary text S1, just above Equation (4): SWE is converted from snow depth?

   We agree that the original wording was inaccurate. Equation (4) estimates SWE from routinely observed snow depth by using the pentad snow-pressure-to-depth ratio as a density proxy; SWE is not converted directly from snow pressure.

7. Supplementary text S3, just above Equation (5). spin-up period

   We agree and will replace "warm-up period" with "spin-up period."

8. Caption of Figure S4, L2-3. Snow band numbers of 11-20 dominate, with an average snow band number of 18

   We agree and revise the sentence for clarity as below

Figure. S4. Snow bands. Number of snow bands in each 0.25° grid cell on the Tibetan and Pamir Plateaus (TPP) (a), and frequency distribution of number of snow bands on the TPP (b). Majority of cells have 11-20 snow bands, with an average of 18 snow bands on the TPP.