Review 1

I would like to thanks the authors for the careful revisions made. The manuscript is now of very high standard and ready for publication.

I just had one minor comment for the final version:

Comment1: This is also consistent with the findings of Leclercq et al. (2010) and Huss et al. (2010), who demonstrated that the opposing effects of retreat and surface lowering largely cancel each other out over longer timescales.' I would remove this sentence as this is off-topic and could be misleading. 'longer timescales' don't apply here as you are looking at just a few years. The rest of the justification is already enough.

Response: We thank the reviewer for this helpful comment. We agree that the sentence referring to longer timescales is not relevant to this short-term study and could be misleading. We have therefore removed the sentence: "This is also consistent with the findings of Leclercq et al. (2010) and Huss et al. (2010), who demonstrated that the opposing effects of retreat and surface lowering largely cancel each other out over longer timescales." The remaining justification, based on the short three-year study period, is retained. We have also removed the now-unused references Leclercq et al. (2010) and Huss et al. (2010) from the reference list.

Review 2

The authors put in significant efforts to address all the points I raised and I think the clarity and readability improved a lot.

My last minor comments are as follows:

Comment 1: Fig. 1, you say the glacier is 2.2 km long which seems a bit small when checking the figure and also given the 16 km2.

Response: We thank the reviewer for this comment. We agree that the reported glacier length of 2.2 km could appear inconsistent with the glacier area and may cause confusion. We have therefore removed the length value from this sentence. The revised sentence now reads: “This south-facing glacier spans an elevation range of 5,400–6,104 m a.s.l.”

Comment 2: "GTI was –0.12 in 2022 indicating weakened mid-latitude westerlies." I do not agree that this is a weakening, this is basically normal/noise.

Response: We agree. A GTI of –0.12 is close to neutral and should not be described as a clear weakening. We have revised the text to “Figure 7b shows that the GTI in 2022 was –0.12, a value close to neutral, indicating near-normal rather than substantially weakened mid-latitude westerlies.” and emphasized that the extremity resulted from the synergistic combination with the strongly positive NSI, rather than from the GTI alone.

Comment 3: 2013 NSI basically the same magnitude as 2022!?

Response: We thank the reviewer for this important correction. The reviewer is right: the NSI in 2013 (2.13) is almost identical to that in 2022 (2.12). We have therefore removed the implication that the 2022 NSI was unprecedented or uniquely strong. However, the two years differ in the accompanying circulation conditions. The GTI in 2013 was strongly negative (–1.21), whereas in 2022 it was only slightly negative (–0.12). This indicates that the 2022 extreme melt cannot be attributed to the NSI alone,

but likely resulted from the combined effect of multiple circulation factors.

We have revised the relevant sentences as follows:

- **Original:** "The NSI in 2022 reached 2.12, far exceeding the values in 2020 (–0.03) and 2021 (0.24), confirming a remarkably northward-displaced SAH during summer 2022."
- **Revised:** "The NSI in 2022 reached 2.12, comparable to the 2013 value of 2.13 and much higher than in 2020 (–0.03) and 2021 (0.24). This indicates a strongly northward-displaced SAH in 2022, although a similar NSI magnitude also occurred in 2013. However, the GTI in 2013 was strongly negative (–1.21), whereas in 2022 it was only slightly negative (–0.12), indicating different mid-latitude westerly conditions between the two years."
- **Original:** "the NSI in 2021 (0.244), like that in 2020 (–0.03), remained far below the exceptional 2022 value (2.12), indicating that in neither non-extreme year did the SAH exhibit the strong northward displacement observed in 2022."
- **Revised:** "the NSI in 2021 (0.24), like that in 2020 (–0.03), remained much lower than in 2022 (2.12), indicating that in neither non-extreme year did the SAH exhibit such a strong northward displacement. Nevertheless, because the 2013 NSI was comparable, the 2022 extremity cannot be attributed to the NSI alone."

Comment 4: Why not correlating NSI with Xiao Dongkemadi mass balance to make your point stronger in section 5.4?

Response: We thank the reviewer for this constructive suggestion. We agree that correlating the NSI with the long-term mass balance record of Xiao Dongkemadi Glacier, the longest continuous mass balance series in the central Tibetan Plateau, would strengthen the circulation–mass balance linkage in Section 5.4. We have therefore performed this analysis over the overlapping period 1992–2023 between the Xiao Dongkemadi mass balance record (1989–2023) and the summer NSI and GTI indices (1992–2023).

We used the raw (non-detrended) values to directly assess the relationship between

the circulation indices and the observed mass balance. For robustness, we also examined the detrended results, which are consistent in sign and significance with the raw results. The raw NSI is significantly negatively correlated with mass balance (R = –0.42, $p < 0.05$), indicating that a more northward-displaced South Asian High favors more negative glacier mass balance. In contrast, the GTI shows no significant correlation with mass balance (R = 0.15, p = 0.417).

These results support the role of the SAH in modulating glacier mass balance on the central Tibetan Plateau. However, because the NSI in 2013 (2.13) was almost identical to that in 2022 (2.12) while the 2013 mass balance was less negative, the 2022 extreme melt cannot be attributed to the NSI alone. This reinforces our revised interpretation that the 2022 event resulted from the synergistic combination of a strongly positive NSI, a near-neutral GTI, and other local energy balance anomalies.

We have added these correlation results to Section 5.4 and included the corresponding scatter plots in the main text as Figure 8e and 8f. The revised text now reads:

"To further test the linkage between large-scale circulation and glacier mass balance, we correlated the raw (non-detrended) summer NSI and GTI with the Xiao Dongkemadi mass balance record, the longest continuous mass balance series in the central Tibetan Plateau, over the overlapping period 1992–2023. The NSI was significantly negatively correlated with mass balance (R = –0.42, $p < 0.05$), whereas the GTI showed no significant correlation (R = 0.15, p = 0.417). These results support the role of the SAH in modulating glacier mass balance on the central TP, while also indicating that the 2022 extreme melt involved additional circulation and surface energy balance anomalies."

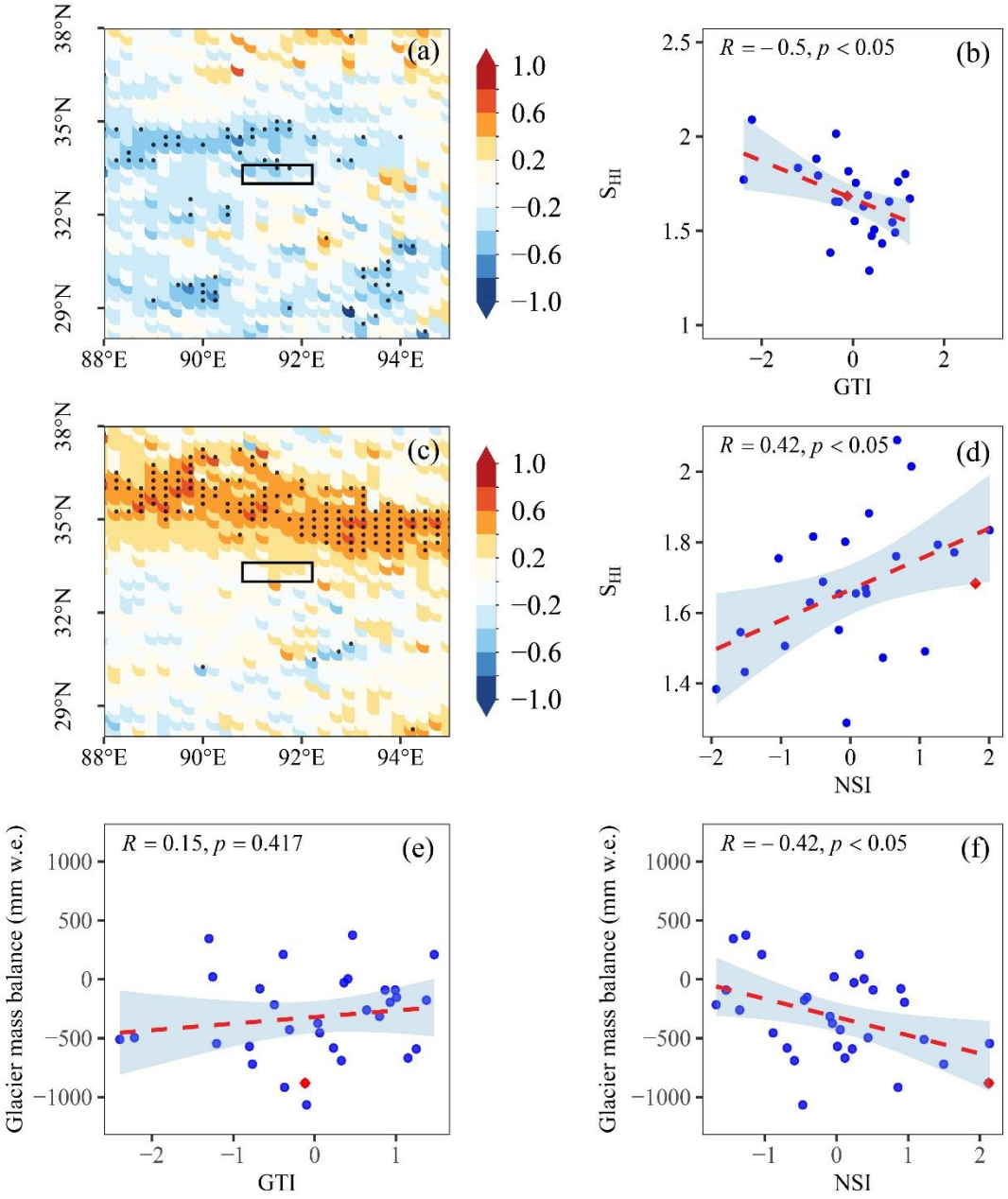


Figure 8. Correlations between GTI, NSI and SHI during June–August from 1992 to 2022. (a, c) Spatial distribution of correlation coefficients of GTI and NSI with SHI based on ERA5 gridded data; stippling indicates significance at the 95% confidence level ($p < 0.05$). (b, d) Scatter plots and correlation coefficients between GTI/NSI and the SHI calculated from the Tuotuohe meteorological station. (e, f) Scatter plots and correlation coefficients between the raw (non-detrended) summer GTI/NSI and the Xiao Dongkemadi mass balance record, the longest continuous mass balance series in the central TP, over the overlapping period 1992–2023. In (b, d, e, f), the red diamond marks 2022.

Comment 5: Fig 3g, are the rows correctly labelled? 1–9 June, 3–16 July, and 3–29 August I thought are the Tuotuohe station heat wave dates according to the text but in the figure they are labelled as Sangqu

Response: We apologize for the labeling error. In Fig. 3g, the rows were mislabeled. The dates 1–9 June, 3–16 July, and 3–29 August are from the Tuotuohe station, not Sangqu. We have corrected the row labels in Fig. 3g and double-checked all other panels for consistency.

Comment 6: double check the rounding consistency flagged by the other reviewer

Response: We thank the reviewer for this careful check. We have re-examined all numerical values in the manuscript, tables, figure captions, and supplementary material. In our previous response, we stated that decimal places were unified to one; after rechecking, we realized that a single fixed rule is not appropriate for all quantities. The required precision depends on the variable, its uncertainty, and its use. We have therefore adopted the following consistent rounding rules:

1. Mass balance, melt/accumulation amounts, and energy fluxes: one decimal place where the value is model- or observation-derived and the uncertainty is larger than the decimal precision, e.g. −920.0 mm w.e., +317.8 mm w.e., +28.2 W m^{-2}, +20.3 W m^{-2}, +17.0 W m^{-2}, and +14.0 W m^{-2}. Values that are inherently integer-scale or whose uncertainty clearly exceeds the decimal precision are reported as integers.
2. Calibrated parameters and dual-threshold boundaries: two decimal places, because these values define the thresholds and require higher precision, e.g. k_{min} = 1.06, k_{max} = 1.13; thresholds = −690.93 and −717.98 mm w.e.
3. Statistical metrics: R^2 and correlation coefficients are reported to two decimal places, e.g. R^2 = 0.99 and 0.96; r = −0.42. p values are reported to three decimal places, e.g. p = 0.417. Where a p value is below the significance threshold, we retain the conventional notation $p < 0.05$; where it is below 0.001, we report p

< 0.001.

4. Temperature anomalies: one decimal place, e.g. 4.9°C, 1.6°C, and 2.0°C.
5. Percentages: one decimal place when the percentage is a model-derived contribution, e.g. 60.2%, 49.7%, and 18.0%; integers are used for simple observed proportions where additional precision is not justified, e.g. 73%, 85%, and 29%.

We have also checked that the same variable is reported with the same precision across the abstract, main text, tables, figure captions, and conclusions. Where necessary, values have been re-rounded and inconsistencies corrected. In particular, all specific p values have been updated to three decimal places throughout the manuscript. We hope this clarifies and resolves the reviewer's concern.

Comment 7: Fig. 5 & section 5.2 & Fig. S4: I would recommend stating in the caption why 2 red dashed lines, besides it might be better to just show ALL glaciers in one figure in the main text. Moreover, I feel that the text could benefit with presenting the translation of the k values to the melt thresholds (in brackets alongside for example)

Response: We thank the reviewer for these constructive suggestions. We have addressed all three points as follows.

(1) Explanation of the two red dashed lines in the caption.

We have revised the caption of Fig. 5 to explicitly state that the two red dashed lines represent the relaxed and strict thresholds derived from the dual-threshold framework. The relaxed threshold is used to identify potential extreme mass loss events, while the strict threshold provides a more conservative criterion for high-confidence extreme events. The revised caption now reads:

"Figure 5. Annual mass balance time series and extreme mass loss events for (a) Xiao Dongkemadi Glacier, (b) Qiyi Glacier, (c) Guliya Glacier, (d) Ningchan No. 1 Glacier, and (e) Xiao Anglong Glacier. The data were sourced from Yao et al. (2012), Zhu et al. (2026a) and Zhu et al. (2022a, 2022b). The two red dashed lines indicate the relaxed and strict thresholds, respectively, derived from the dual-threshold framework

for identifying extreme mass loss events. Red dots mark years identified as extreme mass loss events."

(2) Showing all glaciers in one main figure.

We agree that presenting all five glaciers together improves comparability. We have therefore merged the five glaciers into a single multi-panel figure in the main text (new Fig. 5a–e), and the original Fig. S4 has been removed from the supplementary material.

(3) Translating k values to melt thresholds in the text.

We have added the corresponding thresholds in brackets next to the k values in Section 5.2.

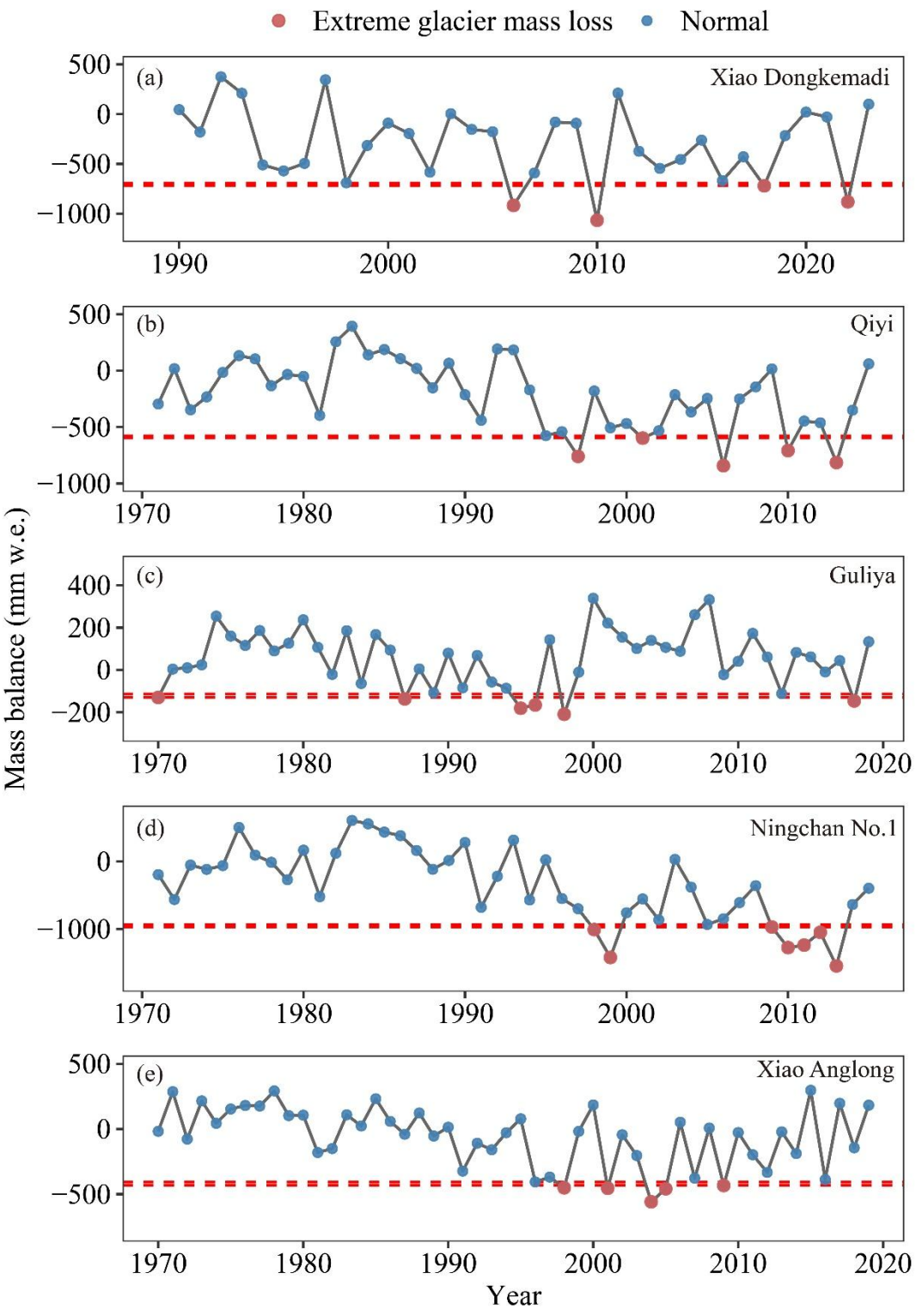


Figure 5. Annual mass balance time series and extreme mass loss events for (a) Xiao Dongkemadi Glacier, (b) Qiyi Glacier, (c) Guliya Glacier, (d) Ningchan No. 1 Glacier, and (e) Xiao Anglong Glacier. The data were sourced from Yao et al. (2012), Zhu et al. (2026a) and Zhu et al. (2021, 2022a, 2022b). The two red dashed lines indicate the relaxed and strict thresholds, respectively,

derived from the dual-threshold framework for identifying extreme mass loss events. Red dots mark years identified as extreme mass loss events.

Comment 8: Fig. S8 please add units

Response: We have added the units to the caption of Fig. S8.

Comment 9: Fig. S1 not really clear whether lines are overlaying or what one can see there as one only sees the yellow line for b & c & d

Response: We thank the reviewer for this comment. We agree that the overlapping lines in Fig. S1 were not clear, especially in panels b, c, and d, where the two series nearly coincide and only one line appears visible. To improve clarity, we have replaced the time series line plots with scatter plots of modeled versus measured values. Each panel now includes a 1:1 reference line and the coefficient of determination (R^2), which more clearly demonstrates the strong agreement between the downscaled estimates and the in-situ observations. This change makes it easier to see that the two datasets are highly consistent, rather than appearing as a single overlapping line. The revised figure is provided as Fig. S1 in the supplementary material.

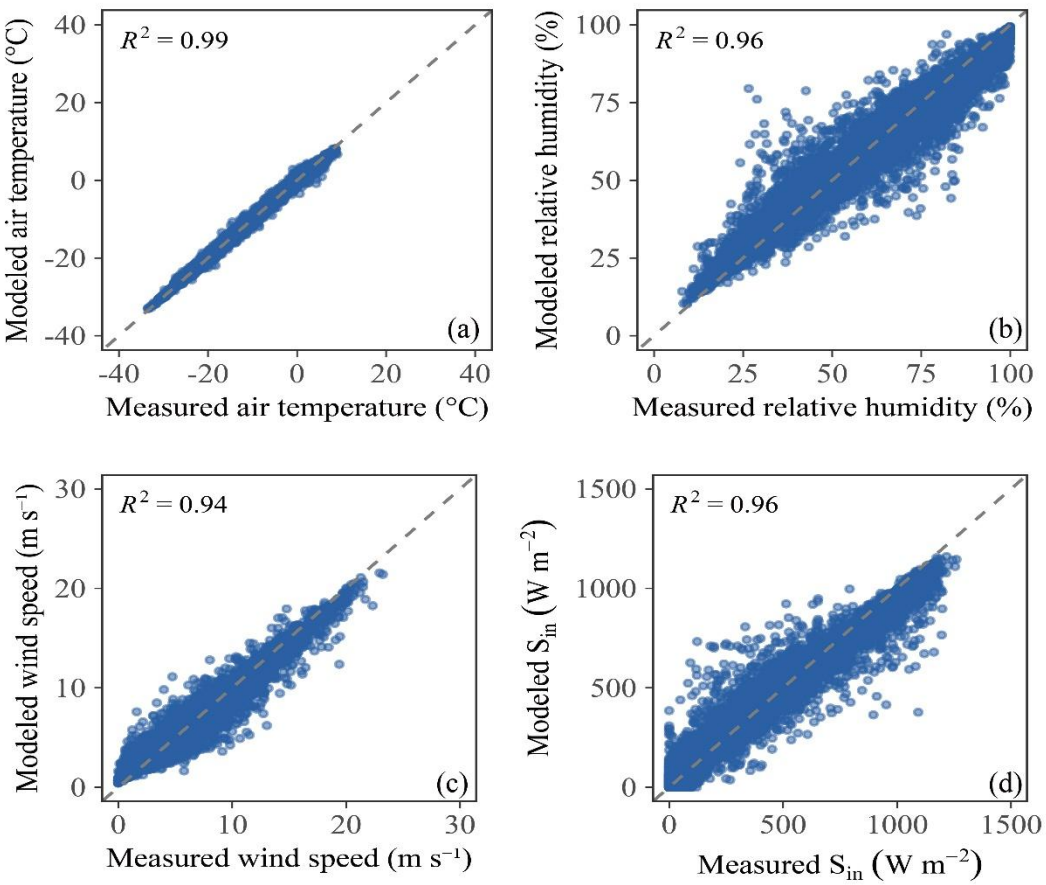


Figure S1. Comparison of observed and ERA5 downscaled (a) Air temperature, (b) Relative humidity, (c) Wind speed, and (d) Incoming shortwave radiation at the hourly scale.

Comment 10: Sec 5.3 and the abstract, you present Lin (+20 W m^{-2}) as a main driver, but Lnet (+14 W m^{-2}) is the honest comparison and QH (+17 W m^{-2}) is then the largest term. This gives a wrong impression of the drivers. Could you add a table with the contribution of each term to the +30 W m^{-2} (in W m^{-2} and %), with net shortwave split into the incoming radiation and albedo part, and rank the drivers from it?

Response: We thank the reviewer for this insightful and constructive comment. We fully agree that presenting L_{in} (+20.3 W m^{-2}) as a main driver was misleading, because L_{out} also increased (+6.3 W m^{-2}), so the net longwave contribution is L_{net} = +14.0 W m^{-2}. In terms of net contribution to melt energy, Q_H (+17.0 W m^{-2}) is the largest single term, followed by L_{net} (+14.0 W m^{-2}) and the net shortwave contribution S_{net} (+5.1 W

m^{-2}). We have corrected this throughout the manuscript.

Following the reviewer's suggestion, we have added two new tables:

- Table S6 lists the contribution of each individual energy balance component to the melt energy anomaly, with net shortwave split into incoming shortwave (S_{in}) and the albedo/Sout effect.
- Table S7 presents the net contributions and ranks the drivers from largest to smallest.

We also rechecked the total melt energy anomaly. The sum of the individual components is +28.2 W m^{-2}, rather than the +30 W m^{-2} stated in the previous version. We have therefore corrected the total anomaly to +28.2 W m^{-2} in the abstract, Section 5.3, the caption of Fig. 6, and the Conclusion. The +317.8 mm w.e. melt amount anomaly remains unchanged.

In the revised text, we now clearly distinguish between the energy-source perspective and the net-contribution perspective. From the energy-source perspective, the largest increase in energy input is from L_{in} (+20.3 W m^{-2}), followed by Q_H (+17.0 W m^{-2}) and the albedo effect (+8.0 W m^{-2}). However, because L_{out} also increases (+6.3 W m^{-2}), the net longwave contribution is L_{net} = +14.0 W m^{-2}. Therefore, in terms of net contribution to the +28.2 W m^{-2} melt energy anomaly, Q_H is the largest single term (+17.0 W m^{-2}, 60.2%), followed by L_{net} (+14.0 W m^{-2}, 49.7%) and S_{net} (+5.1 W m^{-2}, 18.0%).

We have also clarified that the +28.2 W m^{-2} and +317.8 mm w.e. values refer to the three identified heatwave windows (1–8 June, 7–15 July, and 4–29 August 2022; total ~40 days), not the entire June–September period.

We believe these revisions fully address the reviewer's concerns and provide a transparent and accurate attribution of the melt energy drivers.

Table S6. Contributions of individual energy balance components to the +28.2 W m^{-2} melt energy anomaly during the three identified 2022 summer heatwave windows, relative to the October 2019–September 2021 reference period. Percentages may not sum to 100% due to rounding.

Component	ΔComponent (W/m^2)	ΔQ_M Contribution (W/m^2)	Percent (%)
L_{in}	20.3	+20.3	72.1
Q_H	17.0	+17.0	60.2
S_{out}	−8	+8	28.4
Q_G	−3.1	−3.1	−10.8
S_{in}	−2.9	−2.9	−10.4
Q_L	−4.8	−4.8	−17.1
L_{out}	+6.3	−6.3	−22.3

Table S7. Net contributions of energy balance components to the +28.2 W m^{-2} melt energy anomaly during the three identified 2022 summer heatwave windows, relative to the October 2019–September 2021 reference period. Percentages may not sum to 100% due to rounding.

Component	ΔComponent (W/m^2)	ΔQ_M Contribution (W/m^2)	Percent (%)
Q_H	17.0	17	60.2
L_{net}	14.0	14	49.7
S_{net}	+5.1	5.1	18.0
Q_G	−3.1	−3.1	−10.8
Q_L	−4.8	−4.8	−17.1

Comment 11: Fig. 6 & Sect. 5.3, please state explicitly which days were used for the +30 W m^{-2} and the +318 mm. The text and the caption say June to September, but the numbers only seem to fit if they are the accumulated three shaded heat wave windows (about 40 days) so I am not sure this is clear to all readers.

Response: We thank the reviewer for pointing this out. The +30 W m^{-2} and +318 mm w.e. values indeed refer to the three identified heatwave windows, not the entire June–September period. We have revised Section 5.3 and the caption of Fig. 6 and S5 to state this explicitly and to avoid any ambiguity.