Dear Editors and Reviewers,

We sincerely thank you for your insightful and constructive comments on our manuscript, "Satellite-observed surging dynamics of North Kunchhang Glacier I in the Eastern Karakoram" (EGUSPHERE-2025-652). In response to your feedback, we have made comprehensive revisions to the previous submission. We hope that the revised manuscript now meets the publication standards of *The Cryosphere*.

In this round of revisions, we have optimized uncertainty estimation for glacier velocities, reanalyzed velocity changes during the surge of NKG I, reworked the surge mechanism section, and thoroughly reviewed the manuscript's formatting and content. Additionally, we have updated the figures and tables. All comments, suggestions, and questions raised by the reviewers have been addressed and explained in detail.

Again, we deeply appreciate your time and thoughtful comments that are helpful for improving our study and manuscript. Below are our point-by-point responses to the reviewers' comments and questions. Reviewer comments are presented in blue, and our responses follow in black.

Reviewer comments #1:

Overall comments

The authors have done a good job addressing my main concern from the previous version of this manuscript regarding uncertainties in the datasets used. The uncertainty analysis is now much more robust, clearly described in Section 3.6, and consistently reported throughout the results.

This revision represents a notable improvement over the earlier submission. I only suggest a few further, very minor adjustments to strengthen the manuscript. Again, I believe this manuscript provides valuable insights into the mechanisms of glacier dynamic instabilities in the Karakoram and describes methods for glacier mapping using Sentinel-1 SLC images and surface elevation change measurements from Jason-3 altimetry data, which have strong potential for application in other studies of glacier dynamics and for efficiently monitoring glacier change over time.

Response: We thank the reviewer for your positive feedback on our previous revisions and for the positive comment of our work. In response to the reviewer's comments, we have updated the figures and supplemented the figure captions with additional descriptions to enhance clarity. We have also clarified content, including "specific threshold", and have re-analyzed the velocity time series during the surge of NKG I. These modifications have further improved the readability and rigor of our manuscript. We sincerely thank the reviewer once again for providing valuable suggestions for our manuscript.

Specific comments are addressed point-by-point in the following context.

Specific comments

Abstract:

1. L18, L20: Provide uncertainties for the mass transfer estimates (e.g., ~0.45 Gt ± ??).

Response: Based on both reviewers' comments, we have provided uncertainties for the mass transfer estimates in Line 18 (0.53 ± 0.013 km^3) and Line 20 (0.27 ± 0.011 km^3).

Introduction:

2. L49-55: Note that Variagated Glacier and Trapridge Glacier are geographically close, despite their differing mechanisms, showing that other regions also exhibit this heterogeneity in surge behaviours, including Svalbard.

Response: Thank you for your reminder. We have replaced "heterogeneous" with "markedly different" (Lines 52-53).

Study area and data:

3. Consider citing this newly published paper on ITS_LIVE here or elsewhere in your manuscript: https://doi.org/10.5194/tc-19-3517-2025

Response: We have cited this paper in Section 2.2 (Line 113).

Results:

4. L433–434: "In contrast, year 2023 displayed a continuous advance throughout the year, culminating in a ~57 ± 13 m net advance." Be cautious with the wording "continuous advance," since the terminus retreated by >50 m in September.

Response: Thanks for your insightful comment. We have replaced "continuous" with "steady" (Line 433).

5. L529: Clarify what is meant by "critical threshold."

Response: We clarified the specific threshold in Line 527, stating that the driving stress exceeds the basal shear stress or resistance.

Discussion:

6. Section 5.1: Peak 3 appears to have accelerated relatively rapidly, similar to Peaks 2 and 4, but decelerated more slowly. I would therefore not classify it as having a long acceleration phase, as with Peak 1; rather, it seems intermediate between Peak 1 and Peaks 2 and 4. I would also not rule out the possibility that Peak 3's acceleration was hydrologically driven, perhaps reflecting a slower release of englacial or subglacial water over the fall, winter, and spring until the next peak in meltwater input in 2018. As you note, future work could examine the surface characteristics of NKG1 during its surge to determine whether increased crevassing, and the associated development of meltwater

pathways to the glacier bed, coincided with the velocity peaks observed in 2017 and 2018.

Response: Thank you for your valuable comment. We have re-analyzed the velocity changes during the NKG surge. For Peak 3, we found that the abnormally high temperatures in October 2017 likely led to an increase in surface meltwater, which, in turn, caused differences observed in the acceleration and deceleration phases of Peak 3. Section 5.1 has also been rewritten accordingly (Lines 531-567).

Figures and tables

7. Table 1: Near the bottom of the table, in "A Python package contains a series of methods to work with gridded DEM and flow direction datasets": change "contains" to "containing" or "that contains".

Response: We have revised it to "A Python package that contains …".

8. Figure 8: In the caption or preceding text, explain why "Distance to the 1984 terminus" was chosen for the y-axis. Without Figure 9 for context, it could be unclear whether increasing distance implies advance or retreat.

Response: Thank you for pointing out this issue. We have supplemented the following explanation to the caption: For panels (a) and (b), since the glacier length in 1984 was shorter than in all subsequent years after 2000, the glacier termini extracted from Sentinel-1 images (2015–2024) are all ahead of the 1984 terminus. Therefore, an increase in distance from the 1984 terminus indicates glacier advance, while a decrease in distance indicates glacier retreat (Lines 423-425).

9. Figure 9: Add a scale bar, ideally to panel (f).

Response: We have added a scale bar to panel (f) as suggested (Fig. R1).

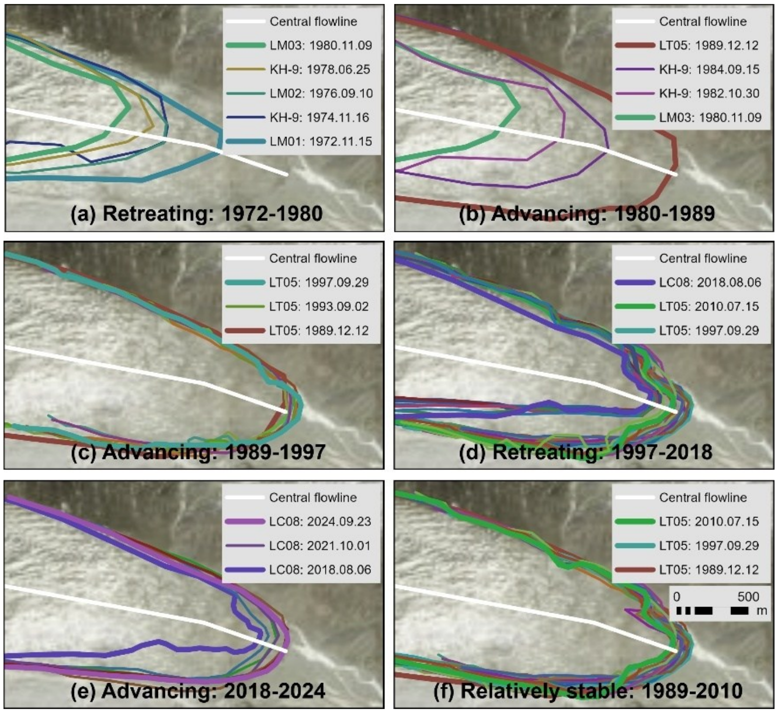


Figure R1: Figure 9 with the added scale bar.

10. Figure 10: In the caption, briefly describe what the black circle highlights in Fig. 10c.

Response: We have added the following information to the caption of Fig. 10: The black circle in panel (c) marks the deep trough that appeared near the outlet of ice-dammed lake in the 2024 GF-7 DEM (Lines 476-477).

11. Figure 11: Add scale bars to both (a) and (b).

Response: We have added scale bars to both Fig. 11 (a) and (b) as suggested (Fig. R2).

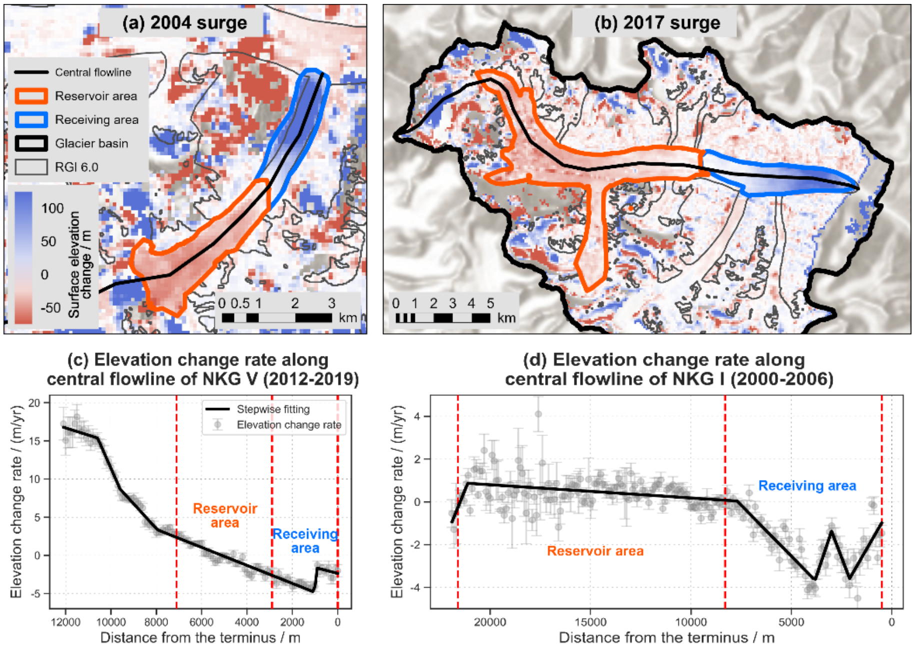


Figure R2: Figure 11 with the added scale bars.

12. Figure 12: Change "green line" to "orange line" for the monthly mean glacier velocity.

Response: We have redrawn Fig. 12 using the updated results, and the line colour of the monthly mean glacier velocity has been changed to orange (Fig. R3).

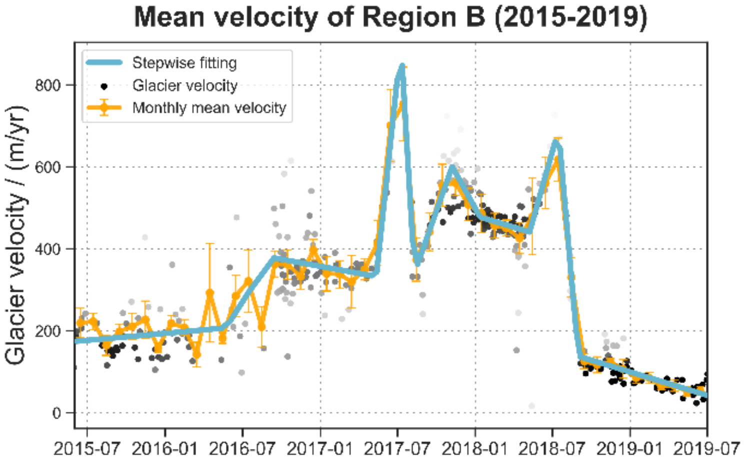


Figure R3: Figure 12 with updated results and colour.

Reviewer comments #2:

Comments on Egusphere-2025-652

I want to commend the authors for their diligent and thoughtful incorporation of the points raised in my previous review. The revisions demonstrate a clear commitment to improving the clarity, rigor, and overall quality of the manuscript.

That said, while the progress is evident and highly appreciated, I believe the manuscript would benefit from a bit more refinement in certain areas to fully realize its potential. I am confident that with these final adjustments, the work will achieve the impact and precision it deserves.

Greg Guillet,

University of Oslo

Response: We sincerely thank the reviewer for your thoughtful and encouraging feedback. Your positive comments about the progress made are much appreciated.

We have carefully considered your comments and have made the necessary revisions to improve the clarity and precision of the manuscript. Specifically:

(1) We recalculated uncertainty in glacier surface velocity using Hugonnet et al. (2022)' method and updated the results and figures;

(2) We reexamined velocity changes during the surge of NKG I, avoiding the oversimplification of attributing the surge to hydrological or thermal controls. We also verified that the enthalpy balance theory provides a better explanation of the NKG surge than classical theories, using available data; and

(3) We updated incorrectly formatted numbers, compared the influences of different temporal intervals in ITS_LIVE product, and reviewed additional studies for comparison.

Thank you once again for your detailed and valuable feedback, which has played an important role in enhancing the quality of our work.

Specific comments are addressed point-by-point in the following context.

General comments

1. Several integers in the paper are formatted as X,XXX (L119 and 136, at least). I think the Copernicus format requires integers as XXXX:

https://publications.copernicus.org/for_authors/manuscript_preparation.html#math

Response: Thank you for pointing out this issue. Based on the reviewer's suggestion, the Copernicus format, and the published articles, we have made the following changes:

(1). For numbers greater than 1000 and less than 10 000, we have removed the thousands separator and represented them as XXXX (e.g., Lines 80, 83, 92, 120, and 137).

(2). For numbers greater than 10 000, we have replaced the thousands separator with a space (Line 82).

(3). For the special number 10 000, we have changed it to "10 thousand" (Line 37).

2. I still think that a lot of the points made by the authors in section 5 are problematic and should be reworked or at least reformulated.

Response: In response to the reviewer's comments regarding Section 5, we have made further revisions. We reprocessed and analyzed velocity changes during the surge of NKG I, and based on this, we examined the mechanisms influencing the surge. We have avoided the oversimplification of attributing the surge solely to either hydrological or thermal controls, as was done previously.

Additionally, we analyzed the monthly mean enthalpy for NKG using monthly mean velocities, thickness changes, slope, monthly temperature, and precipitation data. Using enthalpy as a threshold, we were able to accurately identify the glacier surge. We also input the accumulation rate, slope, and other parameters of NKG into the enthalpy balance model proposed by Benn et al. (2019), successfully reproducing the glacier's periodic surges. These results demonstrate that the enthalpy balance model provides a good explanation for the surge of NKG I.

Moreover, we provided an explanation for our choice to compare the duration of the glacier advance after the surge initiation.

Relevant content has been revised and expanded in the main text; please refer to our responses to comments 11-16 for further details.

3. Throughout my comments, I have made numerous reference to my own paper Guillet et al., (2025). I am aware that it sounds like plugging my own work in here, however, I think both studies address a similar problem, with similar methods and I do think there is great value in discussing your results with regards to my earlier findings. I leave this choice to the discretion of the authors.

Response: We thank the reviewer for providing a wealth of relevant papers, which has deepened our understanding of the dynamics and influencing mechanisms of glacier surges. Based on the papers and feedback provided by the reviewer, we have further refined the uncertainty analysis and the surge mechanisms section. We also briefly compared our results with those from the reviewer's studies. Although there are differences in the methods and data selection, both studies show similar velocity changes and surge durations during the surge, which supports the validity of our results to some extent.

Specific comments

Section 3

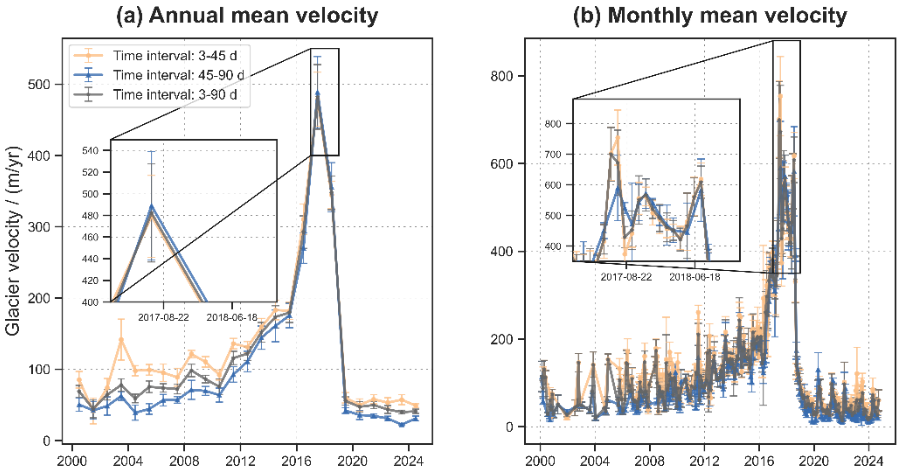
4. L138-142: We found similar problems in Guillet et al., 2025 - ideally we used as surface velocity baseline threshold of 110 days, but I do remember that there is not much information added by increasing from around 50 to 110 days.

Guillet, G., Benn, D. I., King, O., Shean, D., Mannerfelt, E. S., & Hugonnet, R. (2025). Global detection of glacier surges from surface velocities, elevation change and SAR backscatter data between 2000 and 2024: a test of surge mechanism theories. Journal of Glaciology, 71, e88.

Response: Thanks for pointing out this issue. We further analyzed the ITS_LIVE data using different time intervals (i.e., 3–45 d, 45–90 d, and 3–90 d). The results (Fig. R4) show that for annual and monthly mean velocities, different time interval has only a minor influence on velocities during active phases, but a relatively larger impact on velocities during quiescent phases. Longer image-pair intervals lead to lower estimated velocities in quiescent phases, but also reduce short-term fluctuations in the velocity time series.

In addition, for the peak velocities during the surge (point velocities), our results indicate that the maximum velocity of NKG I exceeded 1100 m/yr, whereas Guillet et al. (2025) reported a maximum of 1000 m/yr. Similarly, for NKG V, our maximum velocity exceeded 1700 m/yr, compared to 1500 m/yr reported by Guillet et al. (2025). The inferred surge durations also differ, with our results indicating a somewhat longer duration.

These findings suggest that although the overall trends in glacier velocity are similar across different time intervals (baselines), the selection of interval does influence the results. Moreover, because glacier velocity exhibits intra-annual variability, using overly long intervals may smooth out seasonal fluctuations, while very short intervals inevitably introduce noise. Selecting an appropriate interval therefore involves a trade-off between minimizing fluctuations in the velocity time series and capturing peak velocity accurately. In this study, we continue to use a time interval of 3–45 days.


(a) Annual mean velocity

(b) Monthly mean velocity

Figure R4: (a) Annual and (b) Monthly mean velocity of Region B considering different time intervals (i.e., 3-45 d, 45-90 d, and 3-90 d).5. L156-165: This section glosses over a few important details which I think should still be addressed, at least briefly: "High-quality DEMs from pre- and post-surge" - what defines high quality? and which DEMs are these ? Be more specific here. If these are ASTER DEMs, I would refrain from referring to them as "high-quality".

Response: Thanks for your comment. We avoided using “high quality” or “low quality” to describe the selected DEMs (Line 160). In the revised version, we specify that the DEMs used are ASTER DEMs, and the selection criteria are that the DEMs were acquired as close as possible to the start and end of the surge period and covered the surge extent (Lines 163-164). Section 4.5 has been revised accordingly (Line 495).

6. L221-232: This section still lacks a few references. There are works specifically on SAR data – typically thinking here of Guillet et al 2025, and Mannerfelt et al., 2025.

> Mannerfelt, E. S., Schellenberger, T., & Kääb, A. M. (2025). Tracking glacier surge evolution using interferometric SAR coherence—examples from Svalbard. Journal of Glaciology, 71, e43.

Response: Thank you for providing these relevant references. We have cited them in the manuscript (Line 225) and supplemented the reference regarding the foreshortening and layover of SAR images (Matsuo, 1993) (Line 228).

7. Equations 7 and 8: This is a bit of a headache - and I should have caught this problem earlier. I want to make it clear that this is a common mistake in the glaciological community so I didn't expect the authors to know about it.

The authors here correctly claim that equation 8 is derived from Jakob and Gourmelen, 2023. However, Jakob and Gourmelen in turn cite McNaab et al, 2019 as their main source - I could not find any trace of this equation in McNaab et al., 2019.

What has me worried is the formulation of the which, in previous work, I had tracked down to a wrongly-derived equation in Gardelle et al., 2013.

I recommend the authors use the formulation of the proposed by Hugonnet et al (2022) - in the supplementary information.

Given the spatio-temporal averaging used by the authors. I suspect that both results will be somewhat similar.

> Hugonnet, R., Brun, F., Berthier, E., Dehecq, A., Mannerfelt, E. S., Eckert, N., & Farinotti, D. (2022). Uncertainty analysis of digital elevation models by spatial inference from stable terrain. IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 15, 6456-6472.

Response: We thank the reviewer for raising this important issue. We had noticed the difference between the formulas used in Jakob and Gourmelen (2023) and McNabb et al. (2019), but we overlooked it because the formula in Jakob and Gourmelen (2023) has also been adopted elsewhere (e.g., Fan et al. (2025)).

In the revised version, we re-estimated the uncertainty of glacier surface velocity according to the formulation proposed by Hugonnet et al. (2022) in the supplementary information and the Python open-source library xDEM (https://xdem.readthedocs.io/) (Lines 272-277). We have updated the corresponding results (Section 4.1) and figures (Fig. 5). The revised uncertainties differ from the original values, mainly because the new estimates are more strongly tied to the magnitude of the glacier velocity (e.g., the relative uncertainty of the monthly mean velocity of NKG I is about 7%, whereas that of the annual mean velocity is only around 5%), while the previous uncertainty estimates were more influenced by the number of pixels within each region.

Section 4

8. The results described by the authors here are very similar with our own in Guillet et al. 2025. I think a comparison between both studies - even short and only in a few lines - would be valid here.

Response: Thanks for your suggestion. We compared our results with those of Guillet et al. (2025), emphasizing the consistency between the two studies regarding the relatively low peak velocity and more symmetrical velocity profile during the surge of the main trunk and NKG V (Lines 346-348).

9. L360: "This pattern aligns with the deceleration trends observed in other HMA glaciers (Dehecq et al., 2019)"

I would remove this statement. The deceleration mentioned in Dehecq et al 2019 is related to a reduction in driving stress driven by glacier mass loss.

The deceleration mentioned by the authors here, is related to the termination of a transient event and a return to the glacier's meta-stable condition.

The second part of this statement is also problematic, as the authors mention "pronounced deceleration phases before their respective surge", which is a statement contradicted by Figure 5. I suggest to remove it.

Response: We appreciate your insightful comment. As suggested, we have removed the corresponding statement and revised the text accordingly.

10. L516-519: I would refrain from doing a volume to mass conversion. The Huss, 2013 density conversion is highly debated and does not always hold, especially in cases where mass loss is highly negative. In addition, it's absolute value, and general standard deviation present high spatial correlation that need to be accounted for. Without further explanation of how spatial correlation in these fields have been accounted for, I do not think estimating the mass transfers brings any additional information and suggest simply reporting volume transfers.

Response: We appreciate the reviewer for pointing out this issue. Doing a volume to mass conversion indeed introduces uncertainty, even without considering factors such as moraines and meltwater. In the revised version, we no longer converted the transferred volume to mass for both surge events. Corresponding modifications have been made to the Abstract, Conclusions, and all other sections where transferred mass was mentioned.

Section 5

11. L533: There are other references than Nanni et al. 2023 which specifically focus on the velocity signal for surges. At least give some.

Response: We have supplemented the related work of Huang et al. (2023), Li et al. (2024), and Troilo et al. (2024) (Lines 531-532).

12. L544-554: I find these explanations very far-fetched and a bit over-interpretative.

First, hydraulic vs thermal control is not a consensual approach to the study of surges anymore. I would refrain from using this dichotomous view.

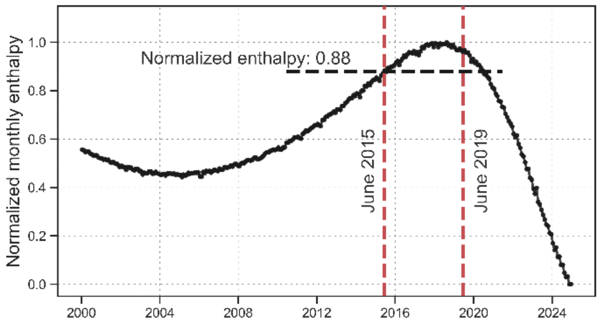
Then, the authors are here clearly over-interpreting results : what we can see is a surge with seasonal increases in velocity (peak 2 and 4 as defined by the authors) which likely stem from meltwater influx, further destabilising the glacier. What it shows is that the surge, and the stability of the glacier during this period, are heavily affected by meltwater influx, and that, the abrupt influx resulting in peak is likely what allows to drain, thus dissipating excess thermal energy stored and terminating the surge.

Finally, the authors only discuss their findings in regards to that of Jiang et al. (2021). I suggest having a look at - at least - Quincey et al (2015), which is cited by the authors, and Guillet et al,. 2025 in which this hydraulic vs thermal control of surges is widely discussed. This is a part of the paper where the authors need to situate their research within the broader literature and I see a significant problem in referencing the Benn et al., 2019 paper and still using the thermal vs hydraulic control of surges hypothesis.

Response: We acknowledge the limitations of attempting to explain the surge of NKG I through either hydraulic or thermal control mechanism.

During this revision, we utilized monthly mean temperature, monthly mean precipitation, initial thickness, thickness change in Region B derived from the DEM time series, and monthly mean velocity in Region B for NKG. By incorporating the parameters and corresponding formulations from the enthalpy balance model proposed by Benn et al. (2019), we calculated the monthly enthalpy change for NKG I.

(a) Monthly enthalpy



(b) Monthly sliding speed

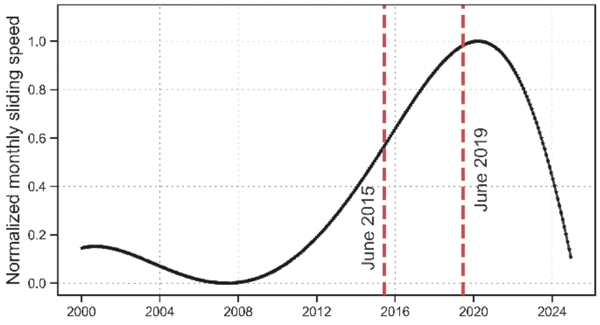


Figure R5: (a) Monthly enthalpy and (b) Monthly sliding speed of NKG I. Vertical red lines show the initiation and end of the surge derived from velocity time series. Horizontal line denotes the threshold of normalized monthly enthalpy.

If we use a normalized enthalpy value of 0.88 to define the initiation and termination of the surge, the active phase for NKG I is identified as June 2019 to April 2020 (Fig. R5(a)). This is close to the duration derived directly from the velocity time series. Thus, it can be considered that the enthalpy balance model by Benn et al. (2019) is capable of adequately characterizing the surge process of NKG I. Furthermore, we attempted to input accumulation rate, annual mean temperature, thickness, degree-day factor, and slope into the enthalpy balance model and successfully reproduced the periodic surging. These demonstrate that the enthalpy balance theory is better equipped to describe the NKG surge than the hydraulic or thermal control theories.

When calculating monthly enthalpy, due to the lack of meltwater and subglacial hydrological data, we set the average depth of water stored at the bed to a fixed value, and simplifications were also made regarding the effective pressure and the proportion of surface meltwater reaching the bed in the model. While mass balance was calculated through thickness changes, accumulation rates, and ice flux. These simplifications introduced some deviation in the modeled sliding velocity (Fig. R5(b)), preventing it from accurately reflecting the sliding during the surge. This may also be the reason for the discrepancy between the surge duration identified by enthalpy and that identified by velocity.

Due to the oversimplification of processes like drainage, the results may contain uncertainties. Consequently, we chose not to include the above results in the main text but instead revised the statements within the paper (Lines 531-567).13. L564: "modifying the internal drainage system" replace with "modifying the glacier's enthalpy budget"

Response: Done.

14. L567-570: I see the point made by the authors here and I think there is value in this comparison. I wonder about the reliability of the Millan et al (2022) estimates, and would like to know about their respective error bars over NKG I and V.

I would further argue that actual estimates of the driving stress are needed to make such a point.

Response: Due to the scarcity of in-situ ice thickness data in High Mountain Asia, current estimates of glacier thickness in the region vary widely across different studies and are subject to high uncertainty. In this study, we adopted data from Millan et al. (2022), where the mean thickness for NKG I is 176.2 ± 156.2 m, and for NKG V is 113.5 ± 45.9 m. We also calculated the ice thickness for both glaciers using data from Farinotti et al. (2019); the mean thickness for NKG I is 135.2 m, and for NKG V is 98.4 m.

Assuming the driving stress equals the basal shear stress, the basal shear stress (τ) can be estimated by the ice thickness (H) and bed slope (θ):

$$\tau = \rho g H \sin(\theta), \tag{1}$$

Using Millan et al.’ thickness, the basal shear stress of NKG I (61.0 kPa) is 72% of that of NKG V (84.4 kPa). If using Farinotti et al.’ thickness, the basal shear stress of NKG I is 64% of that of NKG V. Therefore, the driving stress of NKG V is significantly greater than that of NKG I. We have supplemented the specific values and updated the statement in the text (Lines 580-582).

15. L571-575: This is a very interesting point - also touched upon in Guillet et al., 2025 for marineterminating glaciers.

Response: This inference is primarily based on our previous work in the southeastern Tibetan Plateau (Zhao et al., 2022). Over the past two decades, lake-terminating glaciers have exhibited an ~ 45% faster mass loss compared to land-terminating glaciers, suggesting that proglacial lakes exert severe erosion on the glacier termini. Furthermore, Zhang et al. (2023) also reported that mass loss of lake-terminating glacier in the Greater Himalayas was underestimated by 6.5 ± 2.1% due to glacial lake expansion. Therefore, we propose that proglacial lake erosion is one of the reasons why NKG V surged earlier than NKG I.

16. L583: "After the 1980 surge, the terminus continued to advance for nearly ten years before stabilization, and it then remained stable for almost two decades" - if the terminus is still advancing, then the glacier is most likely still surging.

You mention this further on Line 587 as "post-surge terminus advance" - I do not think such a thing exists and would argue that while a surge-type glacier is advancing, it should be considered as actively surging.

Response: Glacier surge could cause terminus advance, but terminus advance is not a necessary condition for a glacier surge (Truffer et al., 2021). The termination of a glacier surge should be marked by the glacier returning from a high-flow state to a slow-flow state. However, even in the slow-flow state, the glacier terminus may still advance if the glacier mass balance is positive.

Regarding the 1980 surge, due to the lack of velocity observations, we cannot accurately determine its duration. Therefore, our primary intention is to convey that the glacier terminus continued to advance for a relatively long period after the surge occurred. For the 2017 surge, we want to emphasize the terminus advance itself, rather than the post-surge advance. Furthermore, by comparing the differing durations of terminus advance for the two surges, we aim to illustrate the difference in their magnitudes. We have updated our statement accordingly (Lines 593 and 597).

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