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). Based on your comments and suggestions, we have made a thorough revision to the first submission. We hope that the revised manuscript can meet the standard of publication for *The Cryosphere*.

Below, we provide a summary of major revisions, followed by a detailed, point-by-point response to each reviewer's comment. Reviewer comments are shown in blue, and our responses follow in black.

(1) Enhancing uncertainty analysis

We provided a detailed explanation of the uncertainties associated with the datasets and derived results, including glacier velocity derived from ITS_LIVE, glacier surface elevation, lake level, terminus position changes, and glacier/lake area changes. All related results, figures, and discussions have been updated accordingly.

(2) Reorganizing the manuscript

We reorganized and refined the structure and language of Results and Discussion sections to improve clarity and coherence in response to the Reviewers' comments. Discussion Sections 5.2 from the previous manuscript have been moved to Section 4.5 in the revised manuscript, while the previous Section 4.5 has been removed. Additionally, we have added a dedicated discussion on surge mechanism and glacier surge responses to climate change and other influencing factors. The Conclusion has been fully rewritten based on the updated results and discussions.

(3) Expanding and validating results

We incorporated new ICESat-2 results for hotspot regions and extended the elevation change series of Regions B and C to 2024. GF-7 DEM data were re-produced, re-registered, and reprocessed to ensure robustness.

(4) Supplementing relevant references

We added more than 40 relevant references, including both recent studies and seminal works, to strengthen the introduction, methodology, and discussion in accordance with the Reviewers' suggestions. We also thoroughly reviewed and corrected any inaccuracies in the cited data and literature throughout the manuscript.

Again, we greatly appreciate your efforts in reviewing our manuscript and your thoughtful suggestions, which have been instrumental in enhancing our study.

Reviewer comments 1:

Overall comments

This paper effectively utilizes a wide variety of remote sensing datasets and methods to enhance our understanding of the dynamics and surge history of the NKG basin in the Eastern Karakoram. It effectively demonstrates a novel application of Jason-3 altimetry data to measure elevation changes of glaciers and glacial lakes, as well as a new approach for automatically mapping glacier extents and terminus positions using Sentinel-1 SLC images. These methods have potential for widespread application within High Mountain Asia and other glaciated regions, increasing our ability to efficiently monitor glacier change over time.

The authors provide a detailed description of surges in NKG I and NKG V, employing sound methods that are generally well described. However, I believe the uncertainty analysis requires more detail, particularly regarding the uncertainties in the ITS_LIVE glacier velocities used in this study. I recommend that the authors describe these uncertainties more transparently, offering specific uncertainty ranges for each dataset, where possible. Additionally, some sections of the text and certain figures could benefit from modifications to enhance clarity and interpretability. Suggestions for these improvements are provided in the specific comments below.

Once these minor revisions are addressed, I believe this paper will make a valuable contribution to our understanding of glacier surging in the Karakoram and offer important insights into the mechanisms driving glacier dynamic changes in this region.

Response: We sincerely thank the reviewer for the positive and thoughtful comments on our manuscript. We have improved clarity and consistency in the Methods and Results sections as suggested by the Reviewer. The uncertainty estimation has been enhanced to include quantified uncertainties for all datasets and derived results. Inappropriate wording has been corrected. Additional details have been supplemented to the Results and Discussion sections. We believe these revisions will substantially improve the rigor and clarity of our work and sincerely thank you for your constructive feedback and support for the manuscript.

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

Specific comments

Abstract:

1. L10: "Increased occurrence" is a bit ambiguous. Please specify what the increase is relative to, such as whether surging occurrence is higher in this region compared to other glacier regions or whether the occurrence of surging has recently increased.

Response: "Increased occurrence" means that the occurrence of surging in the Karakoram has increased recently. We have changed "increased occurrence" to "occurrence increasing recently".

2. L11–12: I would not say that observations are particularly limited in this region, as several studies have reported on glacier surging in the Karakoram and HMA. I therefore suggest to change this sentence to "However, more observations are needed to further our understanding of surging dynamics and their underlying mechanisms".

Response: We have changed this sentence as suggested.

3. L21: Change "and raising its surface elevation by ~ 180 m" to "and raising the glacier surface elevation by ~ 180 m".

Response: Done.

Introduction:

4. L29–30: Consider revising to: "Frequent glacier surges and slight mass gains over recent decades are defining characteristics of Karakoram glaciers (Farinotti et al., 2020; Bazai et al., 2021), collectively known as the Karakoram Anomaly (Hewitt, 2005; Berthier and Brun, 30 2019; Bolch et al., 2012)."

Response: We have revised the text as suggested to clarify that frequent glacier surges and slight mass gains are defining characteristics of Karakoram glaciers in recent decades.

5. L33: Change "up to 10 to 100 times of the normal (Guo et al., 2022)" to "increase to 10 to 100 times the normal rate (Guo et al., 2022)". Also consider revising the 10 to 100 times figure to 10–1000 times, a more widely accepted range, as some glaciers have been seen to accelerate well over 100 times above background levels, such as Variegated Glacier in its 1982–83 surge (Kamb et al., 1985).

References

Kamb, B., Raymond, C. F., Harrison, W. D., Engelhardt, H., Echelmeyer, K. A., Humphrey, N., ... & Pfeffer, T. (1985). Glacier surge mechanism: 1982-1983 surge of Variegated Glacier, Alaska. Science, 227(4686), 469–479. https://doi.org/10.1126/science.227.4686.469

Response: The glacier flow velocity during surge/active phase generally experiences a ten- to hundred-fold acceleration. Given that some glaciers have been seen to accelerate over 100 times above background levels, we have changed "up to 10 to 100 times of the normal" to "increasing by 1–2 orders of magnitude".

6. L46–48: Since you mention "mass-energy balance", which is based on the idea of the enthalpy balance theory of surging, you should cite this paper by Benn et al. (2019): https://doi.org/10.1017/jog.2019.62.

References

Benn, D. I., Fowler, A. C., Hewitt, I., & Sevestre, H. (2019). A general theory of glacier surges. Journal

of Glaciology, 65(253), 701–716. https://doi.org/10.1017/jog.2019.62

Response: We have cited this paper by Benn et al. (2019).

7. L69: Specify which "key variables" you are referring to here.

Response: Key variables here include glacier velocity, surface elevation, terminus position, and glacial lake level. We have revised the structure of the manuscript, and this issue no longer exists.

8. L85–86: The coordinates seem to be reversed; they should read "34.823°N, 77.863°E" instead.

Response: Done.

9. L91: It would be useful to also mention mean annual air temperatures in this sentence.

Response: We have added the mean annual air temperature for this area (−2.54 °C).

10. L110: Synthetic aperture radar should not be capitalised.

Response: Thank you for pointing this out. We have revised it.

Methods:

11. L142–143: How did you determine that longer intervals resulted in underestimated velocities rather than shorter intervals overestimating velocities if you did not have independent data to validate the ITS_LIVE velocities? Smaller time intervals in the ITS_LIVE image pair data, such as the interval that you used for this study, can be more noisy and have higher errors. Therefore, it would be important to properly quantify uncertainty estimates for the ITS_LIVE data that you used in this study (see other comment for L262–270 below).

Response: The flow velocity at a specific point on the glacier is calculated as its displacement divided by the time interval (Equation (1)). Due to seasonal or short-term variations in glacier velocity, longer time intervals between image pairs tend to underestimate velocities. In hydrologically controlled surges, which typically last only a few weeks or longer, using extended time intervals could markedly underestimate peak velocities (Fig. R1a).

$$\begin{cases} v_X = \dfrac{d_X}{dt} \\ v_Y = \dfrac{d_Y}{dt} \\ v = \sqrt{{v_X}^2 + {v_Y}^2} \end{cases} \tag{1}$$

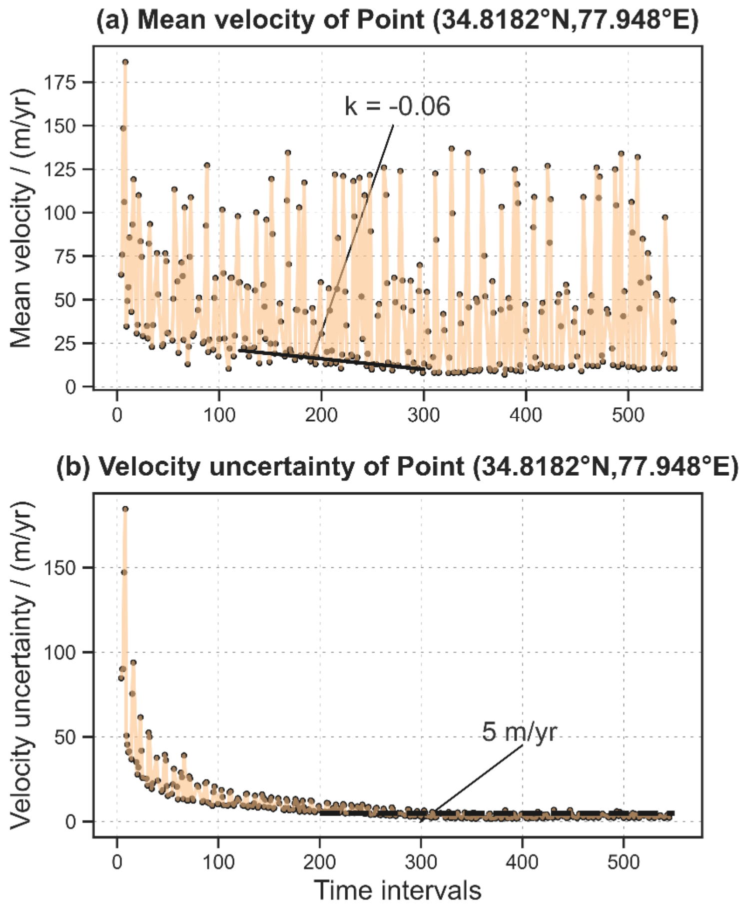

Figure R1: (a) Mean velocity and (b) Velocity uncertainty of Point (34.8182°N, 77.948°E)

Conversely, excessively short time intervals between image pairs may introduce greater noise (Fig. R1b). For example, at Point (34.8182°N, 77.948°E), we analyzed the relationship between image-pair time intervals and derived glacier velocities. We found that very short time intervals lead to substantial velocity uncertainties. However, image pairs with intervals shorter than 6 days account for only 3.5% of all cases, minimizing their overall impact.

To derive accurate peak velocities during surging and reduce uncertainty, we refine our selection of image-pair time intervals as 3–45 days and systematically quantify uncertainties in the ITS_LIVE dataset in subsequent revisions.

12. L145: Change "Give" to "Given".

Response: Done.

13. L153: Specify the resolution of the DEM.

Response: We have added the DEM resolution (1 m) in the text.

14. L156–157: Consider adding this DOI for the Theia data portal, where the Hugonnet et al. (2021) data can be downloaded: https://doi.org/10.6096/13.

Response: We have added the citation for the ASTER DEMs as suggested, and other relevant sections have also been updated accordingly.

15. L189: Small typo: Change “Jaosn-3’s” to “Jason-3’s”

Response: Done.

16. L262–270: It is good to know that the velocities derived from these different satellites tend to agree well with each other. However, you did not give any estimated uncertainty value (i.e., in m yr^{-1}). The ITS_LIVE dataset provides GeoTIFF files of estimated velocity errors. It would be good to mention typical error values over the glacier from the velocity maps that you downloaded over the specific time periods that you analysed. At the very least, you could quantify velocities over unglaciated terrain to give an estimate of uncertainties.

Response: Thanks for pointing this out. We have refined the uncertainty analysis by assessing the velocity uncertainties for different regions and time periods based on the estimated velocity errors provided by the ITS_LIVE dataset, and have updated the corresponding results and figures accordingly.

Generally, the uncertainty in the monthly mean velocity is greater than that of the annual mean velocity. Moreover, as the number of images available for deriving glacier velocity increases, the uncertainty in the glacier flow velocity decreases significantly. Regarding the entire NKG I, before 2014, the mean uncertainty of the monthly mean velocity was 23 m/yr. Between 2014 and 2018, the uncertainty of the monthly mean velocity dropped to 10 m/yr, and after 2019, it further decreased to 3 m/yr.

Results:

17. L299–300: “A notable increase was observed in June 2015”: please quantify this velocity increase.

Response: The velocity increased more rapidly between June 2015 (185 m/yr) and June 2017 (479 m/yr) than it did between June 2010 (92 m/yr) and June 2015. We have quantified the velocity increase in the revised manuscript.

18. L301–302 and L326–328: Once again, it would be helpful to provide specific values for these velocity peaks to clarify their magnitude and improve the interpretability of the text

Response: We have supplemented the velocities for the two peaks in November 2017 (567 ± 43 m/yr) and July 2018 (623 ± 15 m/yr) in the long-term time series and the two intra-annual peaks in May and June (90–95 m/yr) and September–October (~50 m/yr).

19. L352–354: While the elevation change rates are shown in Figure 6d, I suggest also mentioning them in the text for increased clarity.

Response: The elevation change rates over different periods in Region C have been specified in the text.

20. L376: Change "taking" to "taken".

Response: Done.

21. L376–377: Please specify uncertainties for the terminus position changes (i.e., 43 ± ?? m and 180.3 ± ?? m).

Response: We estimated the uncertainty in glacier terminus positions (σ_t) derived from Sentinel-1 SLC images by considering both the image geolocation uncertainty (σ_{co}) and the algorithm uncertainty (σ_{al}) (Guan, 2024). The geolocation uncertainty was taken from Senitnel-1's annual performance report, while the processing uncertainty was assumed to be half a pixel:

$$\sigma_t = \sqrt{{\sigma_{co}}^2 + {\sigma_{al}}^2} \quad (2)$$

The uncertainty in terminus position change (σ_{tc}) was then calculated by combining the positional uncertainties from both time periods (σ_{t1} and σ_{t2}):

$$\sigma_{tc} = \sqrt{{\sigma_{t1}}^2 + {\sigma_{t2}}^2} \quad (3)$$

Uncertainties in the terminus position changes were estimated to be 43 ± 28 m and 180 ± 29 m.

22. L398–400: In terms of glacier extent changes, have you quantified glacier area changes (with associated uncertainties)? If so, please mention these estimates and associated uncertainties here.

Response: The uncertainty in glacier area change (σ_A) was estimated by considering both the resolution of the data source and the clarity of glacier outlines (Minora et al., 2016; Guan, 2024).

$$\sigma_A = l \times \sqrt{{LRE_{yr}}^2 + {\sigma_{co}}^2} \quad (4)$$

Where l is the glacier boundary length (excluding ridgelines), LRE_{yr} denotes the resolution-related error (assumed as half a pixel), and σ_{co} indicates the geolocation accuracy of the imagery.

The uncertainty in glacier area change (σ_{Ac}) was then calculated by combining the positional uncertainties from both time periods (σ_{A1} and σ_{A2}):

$$\sigma_{Ac} = \sqrt{\sigma_{A1}{}^2 + \sigma_{A2}{}^2} \quad (5)$$

Between 2002 (RGI 6.0) and 2024, the glacierized area in the NKG basin decreased by 4.8 ± 11.2 km². Excluding the surge-affected expansion of NKG V and bare terrain in high-altitude areas, small glaciers and permanent snow cover within the basin experienced an pronounced reduction of 21.3 ± 10.9 km².

23. L427: Change "has commenced" to "had commenced".

Response: We have corrected the tense.

Discussion:

24. L513: Change "data-scare" to "data-scarce".

Response: Done.

25. L537–538: Either state the revised glacier volume changes here or direct the reader to Table 2 for details.

Response: We have directed the reader to Table 2 for details.

26. L564–565: Briefly explain how enhanced the steeper surface slope and erosion from the former proglacial lake would have contributed to NGK V surging first.

Response: A steeper surface slope generates greater gravitational driving stresses that overcome bed friction (Round et al., 2017; Dehecq et al., 2019), making it easier for NKG V to accelerate once internal or basal resistance is reduced. After controlling for confounding variables, NKG V's steeper profile necessitates less cumulative mass than NKG I to achieve comparable driving stresses, thereby making it more susceptible to surge. This also facilitates faster ice flow during surging.

The former proglacial lake calves the terminal and destabilizes the glacier front, reducing back pressure and allowing the glacier to advance more easily. Additionally, the lake water erodes NKG V, may form drainage channels (Gao et al., 2024), and modify subglacial water pressure systems. Lake water intrusion through crevasses or subglacial drainage further increases basal sliding.

Together, these factors lower the resistance to ice flow, enhance driving stresses, and create instability thresholds. Thus, the steeper surface slope and erosion from the former proglacial lake would have contributed to the earlier surge of NGK V.

27. L565–566: "Historical climate records indicate that from 1977 to 1980, temperatures

in the NKG region were higher than average": 1980 appears to have been a relatively warm year, but 1979 was one of the coldest years according to Figure 13a. Therefore, from looking at the graph qualitatively, I would not say that 1977–1980 temperatures were higher than average, unless you can quantitatively prove that.

Response: Although 1979 was one of the coldest years, the mean temperature in the NKG region during 1977–1980 (−2.35 °C) was higher than the 1970–1980 average (−2.60 °C). Additionally, the annual mean temperature in 1980 was 2.11 °C higher than that in 1979, creating warmer conditions for the accumulation of basal water pressures—a key driver of glacier surges (Murray et al., 2003). We have added such quantitative description to support it.

28. L581–582: "This transition could lead to reduced surge magnitude, shorter return periods, and greater instability in glacier structure": While a transition to surging controlled by hydrological processes would not in itself cause reduced surge magnitude (hydrologically-regulated surges are often more intense than thermally-regulated ones, for instance), I believe you are referring instead to more negative mass balance conditions and a reduced ability of the reservoir zone to build up mass between more frequent surge events causing this decrease in surge magnitude. Please ensure to clarify this in your text.

Response: Climatic warming and precipitation phase changes amplify warm-season meltwater and rainfall inputs, elevating basal water pressures and inducing hydrological complexity (Harrison and Post, 2003). This shifts surge regimes toward seasonally hydraulic-controlled behavior. At the same time, enhanced ablation under higher temperatures reduces cumulative ice mass, potentially suppressing surge magnitudes due to diminished reservoir volumes. We have revised the text accordingly.

29. L606: Replace "remaining" with "maintaining".

Response: We have re-written the conclusion, and this issue no longer exists.

Figures and tables

30. Figure 4: Mention how the extent of the black rectangle was determined. For example, "The two black rectangles highlight the extents of the identified glacier surges, showing where velocities increased by an order of magnitude over quiescent rates".

Response: We have revised it to "showing where velocities increased by 20-fold (2004) and 5-fold (2017) of magnitude over quiescent rates (~ 50 m/yr in 2023)".

31. Figure 5: For easier comparisons between the four graphs, I would suggest using the same y axis value range (i.e., 0–1400 m yr^{-1}), or to at least inform the reader that the y-axis scales differ between the graphs.

Response: If we used the same y axis value range, panels (a)–(d) would appear excessively sparse (Fig. R2). Therefore, we have explicitly noted in the figure caption that panels (a)–(d) employ different ranges for y axis.

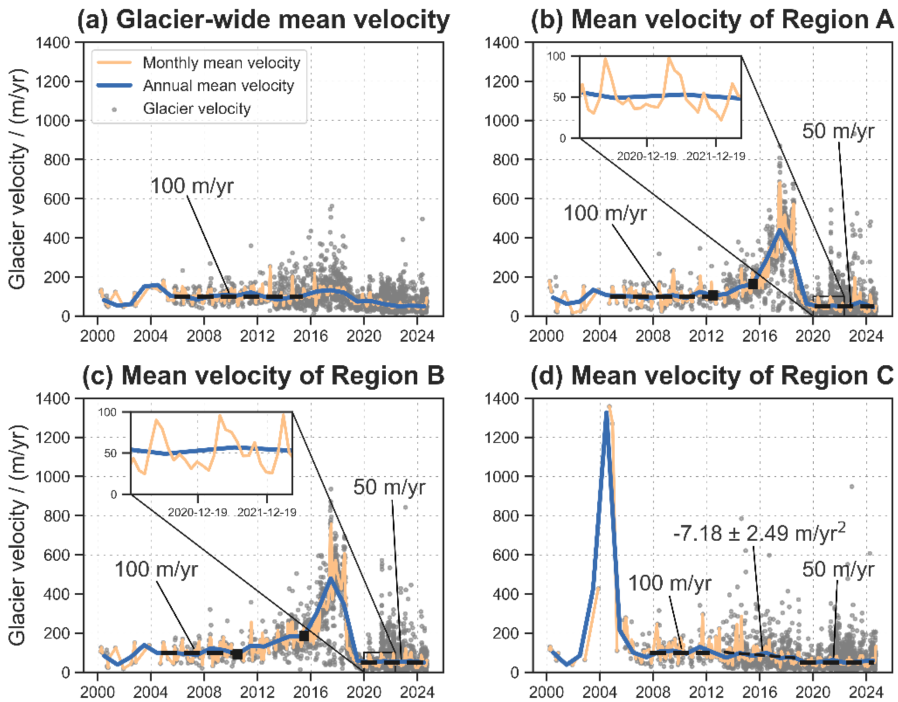

Figure R2: Fig. 5 (a)–(d) using the same y axis value range

32. Figure 7: Elevation change data in Figure 7e look rather noisy, especially in higher elevation areas. I recommend mentioning uncertainty estimates for these results in the text.

Response: Due to GF-7's 26° off-nadir angle (Zhu et al., 2021) and image oversaturation in high-altitude areas, the elevation uncertainty in these regions is relatively large, manifested as increased noise in the elevation differences. In further processing, we applied a slope threshold of 15° to exclude high-altitude areas with large uncertainties.

33. Figure 7: The colour bar, running from -25 m to 50 m of surface elevation change, is asymmetric. It would be better if it were around 0 (e.g., -50, -25, 0, 25, 50). This adjustment would make it easier to directly compare the intensity of elevation gains and losses through time, and would ultimately improve clarity and visual interpretation of the figure.

Response: Due to the glacier surge, the thickening of NKG I is significantly greater than the thinning. If a symmetric range ([−50, 50]) is used, the glacier thinning across most areas would appear insignificant (Fig. R3). Therefore, we retain the previous display range.

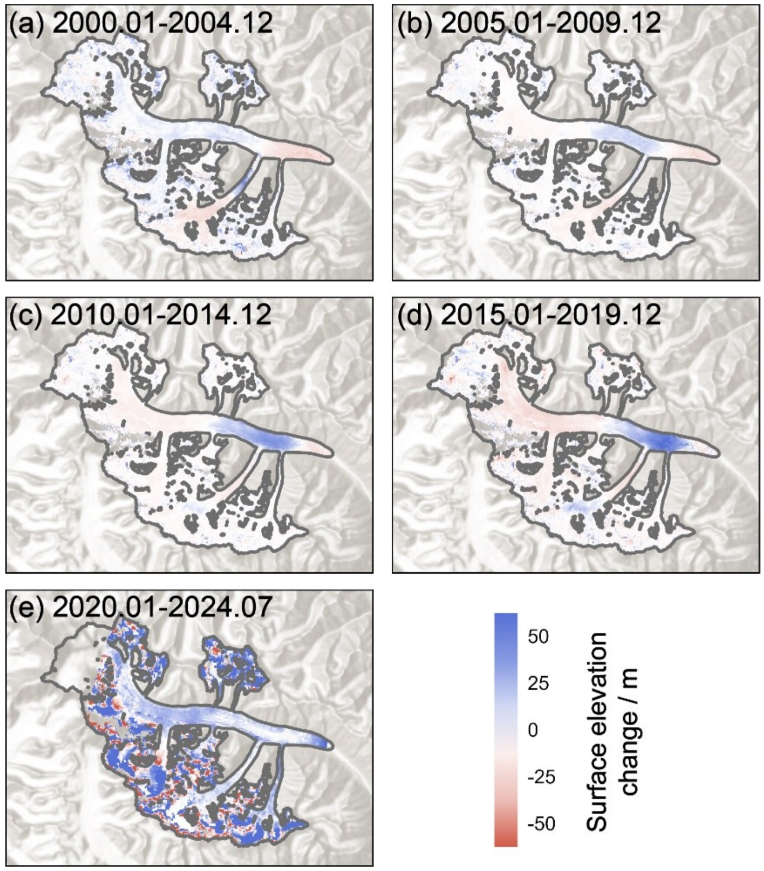

Figure R3: Modified Fig. 7

34. Figure 8: Why did you choose to display changes in the longitude of the terminus position on the y-axis? Displaying changes in terminus position in meters or kilometers would be much easier to interpret.

Response: Since the main trunk of NKG I follows a west-east orientation, longitude effectively captures the variations in terminus position. Therefore, in our previous figures, we displayed changes in the longitude of the terminus position on the y-axis. In the revised manuscript, we have updated the y-axis to represent the distance from the 1984 terminus position (Fig. R4).

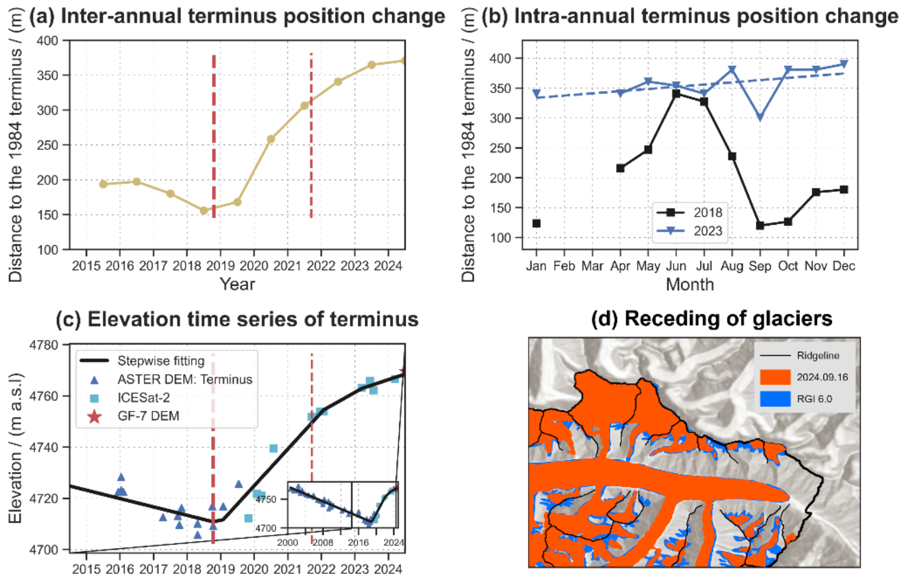

Figure R4: Modified Fig. 8. (a) Inter-annual and (b) Intra-annual terminus position variations derived from Sentinel-1 imagery; (c) Surface elevation time series at the glacier terminus, with the lower right subfigure displaying elevation changes from 2000 to 2024; (d) Glacier recession of NKG I, where blue represents the glacier extent derived from the RGI 6.0 inventory (August 2, 2002) and red depicts its configuration as of September 16, 2024.

35. Figure 10: In line 434 of the figure caption, change "insert map" to "inset map".

Response: Done.

36. Figure 12: This is a nice figure, but as with Figure 7 (see previous comment), it would be better to center the surface elevation change scale on 0 with the following labels: -100, -50, 0, 50, 100.

Response: During glacier surges, the thickening in the receiving area significantly exceeds the thinning in the reservoir area. If we used symmetrical axis ranges ([−100, 100]), the thinning in the reservoir area would appear insignificant (Fig. R5). Therefore, we retain the previous display range.

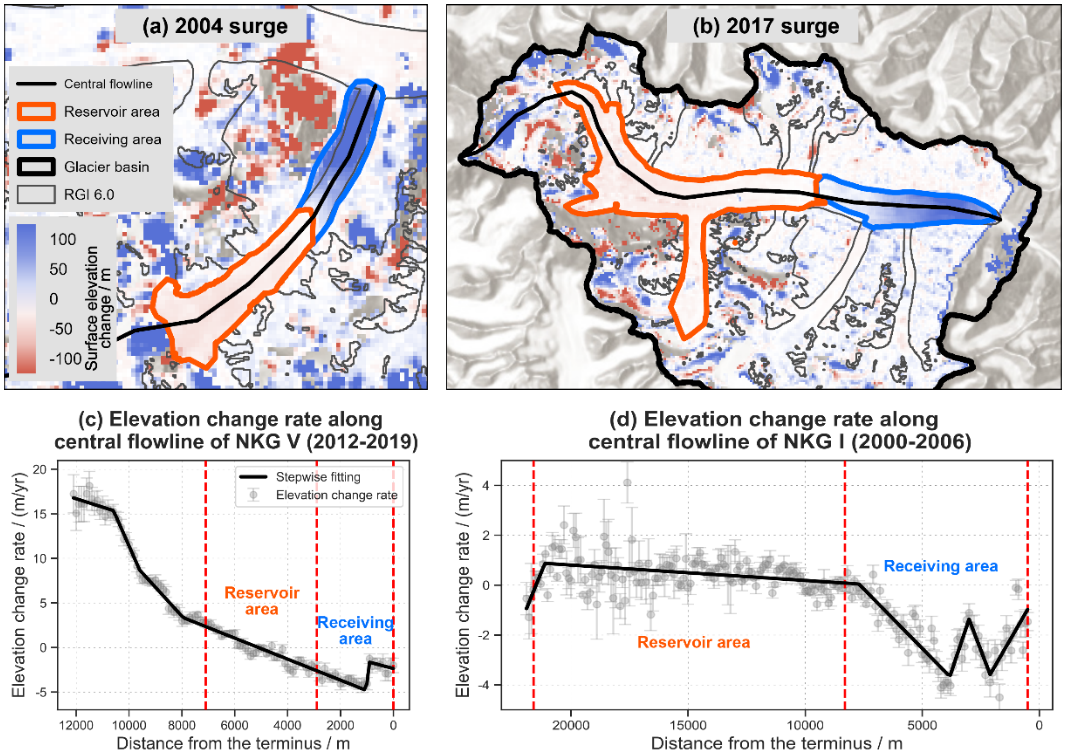

Figure R5: Modified Fig. 12

References

Dehecq, A., Gourmelen, N., Gardner, A. S., Brun, F., Goldberg, D., Nienow, P. W., Berthier, E., Vincent, C., Wagnon, P., and Trouvé, E.: Twenty-first century glacier slowdown driven by mass loss in High Mountain Asia, Nat Geosci, 12, 22-27, https://doi.org/10.1038/s41561-018-0271-9, 2019.

Gao, Y. P., Wang, J. L., Liu, S. Y., Yao, X. J., Qi, M. M., Liang, P. B., Xie, F. M., Mu, J. X., and Ma, X. G.: Monitoring dynamics of Kyagar Glacier surge and repeated draining of Ice-dammed lake using multi-source remote sensing, Sci Total Environ, 928, 172467, https://doi.org/10.1016/j.scitotenv.2024.172467, 2024.

Guan, W. J.: Distribution and characteristics of surge-type glaciers in High Mountain Asia, Lanzhou University, https://doi.org/10.27204/d.cnki.glzhu.2024.000034, 2024.

Harrison, W. D. and Post, A. S.: How much do we really know about glacier surging?, Ann Glaciol, 36, 1-6, https://doi.org/10.3189/172756403781816185, 2003.

Murray, T., Strozzi, T., Luckman, A., Jiskoot, H., and Christakos, P.: Is there a single surge mechanism? Contrasts in dynamics between glacier surges in Svalbard and other regions, J Geophys Res-Sol Ea, 108, 2237, https://doi.org/10.1029/2002jb001906, 2003.

Minora, U., Bocchiola, D., D'Agata, C., Maragno, D., Mayer, C., Lambrecht, A., Vuillermoz, E., Senese, A., Compostella, C., Smiraglia, C., and Diolaiuti, G. A.: Glacier area stability in the Central Karakoram National Park (Pakistan) in 2001-2010: The "Karakoram Anomaly" in the spotlight, Prog Phys Geog, 40, 629-660, https://doi.org/10.1177/0309133316643926, 2016.

Round, V., Leinss, S., Huss, M., Haemmig, C., and Hajnsek, I.: Surge dynamics and lake outbursts of Kyagar Glacier, Karakoram, Cryosphere, 11, 723-739, https://doi.org/10.5194/tc-11-723-2017, 2017

Zhu, X. Y., Tang, X. M., Zhang, G., Liu, B., and Hu, W. M.: Accuracy Comparison and Assessment of DSM Derived from GFDM Satellite and GF-7 Satellite Imagery, Remote Sens-Basel, 13, 4791, https://doi.org/10.3390/rs13234791, 2021

Reviewer comments 2:

Comments on Egusphere-2025-652

The manuscript proposed by Zhao et al. presents a very detail record of surging dynamics for North Kunchang glaciers in the Karakoram.

The authors rely on a combination of different remotely-sensed datasets to produce a wide array of

While the work and the methods shows promise, the paper appears to be in an early stage of development and requires from significant revisions before it is considered for publication.

As it stands, I think sub-section 3.6, and sections 4 and 5 require the most attention. I find the overall opacity over the proposed uncertainties concerning. Some sections clearly gloss over problems to quickly and some statements suffer from insufficient justification - see my specific comments.

I recognize the amount of effort that went into this work. This is why I want to restate my strong support for the manuscript as I think it brings an important contribution to the field.

I strongly encourage the authors to address the points I have highlighted, as doing so will enhance the clarity, rigor, and overall impact of the work.

Greg Guillet, University of Oslo

Response: We sincerely thank the reviewer for the constructive and encouraging comments. We truly appreciate your recognition of the significance of our work and your thoughtful suggestions for improvement.

We acknowledge that the previous version of the manuscript lacked sufficient clarity in several aspects, particularly regarding the estimation and presentation of uncertainties. In the revised manuscript, we have addressed these issues and supplemented more details. Specifically, we:

(1) Provide a more thorough explanation of the uncertainties associated with the datasets and derived results (i.e., glacier velocity derived from ITS_LIVE, glacier surface elevation, lake level, and terminus position changes);

(2) Reorganize and refine the structure and language of Results and Discussion sections to improve clarity and coherence;

(3) Ensure that all major claims are better justified, with clearer references to supporting evidence or figures;

We are committed to significantly improving the manuscript and believe that the revisions will enhance its rigor and overall impact. Thank you once again for your detailed feedback and continued support for this study.

General comments

1. The authors provide very detailed and quantitative descriptions of changes in glacier surface elevation changes. However, I am wondering to which extent this is noise-mining. While I agree on the larger elevation change trends, without a clear understanding and representation of the uncertainties in each dataset, I think the authors are over-interpreting systematic biases in the data as physical signal.

Response: In our previous manuscript, we did not adequately estimate the uncertainties of the datasets and the results, which may have led reviewers and readers to question the reliability of our findings. In the revised version, we have made every effort to quantify the uncertainties associated with the datasets used. We have also carefully reviewed the data processing and systematic bias correction to ensure the reliability of the results.

In fact, for the elevation time series of different subregions, after datum conversion and slope correction, the elevation time series from three different sources (ASTER DEM, ICESat-2, and GF-7 DEM) can be smoothly connected without additional systematic error correction. This demonstrates that systematic biases have not been misinterpreted as physical signals. Furthermore, in the revised manuscript, we have added the corresponding data uncertainties in both the results and the figures.

2. Throughout the manuscript, the authors provide quantities up to one decimal - I think this is way more "precision" that can actually be derived from the data used - I suggest rounding the results.

Response: Thank you for highlighting this important issue. In previous studies (Dehecq et al., 2019; Beaud et al., 2022; Guillet et al., 2022), glacier flow velocities were typically rounded to whole numbers when the values were large (e.g., 150 m/yr). However, for smaller velocities (e.g., <10 m/yr), rounding could introduce significant errors. As a result, results are often kept to one or even two decimal places in such cases. Similarly, values related to glacier mass balance or surface elevation change are typically presented with decimal precision (Brun et al., 2017). In this study, we followed the same approach and updated the relevant results in the manuscript accordingly. The glacier velocities presented in the text have mostly been rounded to whole numbers now.

3. The reference list is sparse and citations within the main body are not always relevant - see specific comments.

Response: Thank you for bringing this to our attention. We have carefully reviewed and supplemented the relevant references in accordance with your suggestions, and thoroughly verified all cited works to ensure accuracy.

Specific comments

Introduction

4. L29: "collectively known as the Karakoram Anomaly", I would remove mention to the Karakoram Anomaly. The relationship between surges and the Karakoram Anomaly is still too poorly constrained. This sentence gives a false sense of consensus.

Response: Thanks for pointing this out. We have revised the statement as suggested.

5. L30 and throughout the manuscript: "(Hewitt, 2005; Berthier and Brun, 2019; Bolch et al., 2012)": Please keep a consistent citing style with ascending order from oldest to newest research.

Response: We have updated the order of multiple citation throughout the manuscript.

6. L31-32: This sentences seriously lacks references and Guo et al. 2022 should not be the only work referenced when making such a statement about glacier surges. Here is a non-exhaustive list of relevant work:

Meier, M. F. and Post, A.: What Are Glacier Surges?, Can. J. Earth Sci., 6, 807–817, https://doi.org/10.1139/e69-081, 1969.

Raymond, C. F.: How Do Glaciers Surge? A Review, J. Geophys. Res.-Sol. Ea., 92, 9121–9134, https://doi.org/10.1029/JB092iB09p09121, 1987. a

Sharp, M.: Surging Glaciers: Behaviour and Mechanisms, Prog. Phys. Geogr.-Earth and Environment, 12, 349–370, https://doi.org/10.1177/030913338801200302, 1988. a

Truffer, M., Kääb, A., Harrison, W. D., Osipova, G. B., Nosenko, G. A., Espizua, L., Gilbert, A., Fischer, L., Huggel, C., Craw Burns, P. A., and Lai, A. W.: Chapter 13 - Glacier Surges, in: Snow and Ice-Related Hazards, Risks, and Disasters (Second Edition), edited by: Haeberli, W. and Whiteman, C., Elsevier, 417–466, https://doi.org/10.1016/B978-0-12-817129-5.00003-2, 2021. a, b

Jiskoot, H.: Glacier Surging, in: Encyclopedia of Snow, Ice and Glaciers, pp. 415–428, Springer Dordrecht, 2011.

Response: Thank you for providing these relevant references. We have cited them in the manuscript.

7. L31-32: I would avoid the use of "normal" and refer to something more neutral here like "quiescent behavior" or "quiescent flow".

Response: We have revised the sentence to "Glacier surges are periodic events marked by a rapid acceleration in flow velocity, generally increasing by 1–2 orders of magnitude compared to quiescent phases".

8. L34-37: There are way more up-to-date numbers for this. See for example :

Guillet, G., King, O., Lv, M., Ghuffar, S., Benn, D., Quincey, D., & Bolch, T. (2022). A regionally resolved inventory of High Mountain Asia surge-type glaciers, derived from a multi-factor remote sensing approach. The Cryosphere, 16(2), 603-623.

Guo, L., Li, J., Dehecq, A., Li, Z., Li, X., & Zhu, J. (2023). A new inventory of High Mountain Asia surging glaciers derived from multiple elevation datasets since the 1970s. Earth System Science Data, 15(7), 2841-2861.

Response: We have cited the suggested studies and newly published studies by Yao et al. (2023) and Ke et al. (2024).

9. L42: Remove "transforming glacier morphology"

Response: Done.

10. L45: There are more than simply those 2 references focusing on the hazards posed by glacier surges. See the following works as an example:

Leinss, S., Willimann, C., & Hajnsek, I. (2019, July). Glacier detachment hazard analysis in the West Kunlun Shan mountains. In IGARSS 2019-2019 IEEE International Geoscience and Remote Sensing Symposium (pp. 4565-4568). IEEE.

KOMATSU, T., & WATANABE, T. (2014). Glacier-Related Hazards and Their Assessment in the Tajik Pamir: A Short Review. Geographical Studies, 88(2), 117-131.

Muhammad, S., Li, J., Steiner, J. F., Shrestha, F., Shah, G. M., Berthier, E., ... & Tian, L. (2021). A holistic view of Shisper Glacier surge and outburst floods: from physical processes to downstream impacts. Geomatics, Natural Hazards and Risk, 12(1), 2755-2775.

Gao, Y., Liu, S., Qi, M., Xie, F., Wu, K., & Zhu, Y. (2021). Glacier-related hazards along the International Karakoram Highway: status and future perspectives. Frontiers in Earth Science, 9, 611501.

In addition, change "catastrophic floods" into "glacier-lake outburst floods", and cite relevant work at each type of hazard.

Response: We have changed "catastrophic floods" into "glacier-lake outburst floods". Suggested works and other works on glacier-lake outburst floods (e.g., Lovell and Muhammad, 2024), ice avalanches (e.g., Wu et al., 2025), and glacier-induced debris flows (e.g., Evans et al., 2009) have been properly cited in the manuscript.

11. L46-52: There are somewhat true statements in this paragraph, but I still find it pretty problematic. First, I am unsure as to what the authors refer to when they mention the "mass-energy balance", I assume they refer to the "mass-enthalpy balance" theory developed in Benn et al. (2019), in which case, please keep the terminology consistent and cite the relevant references. Then, the authors rightfully mention that surges cannot be solely attributed to either hydrological or thermal control mechanisms. They however only mention surges in the Karakoram and create a comparison with surges in Alaska and Canada. This breaks with the current idea of unity of processes to trigger surges, where an imabalance in between the mass and the enthalpy lead to unstable behavior. I would rephrase this whole paragraph and, again, reference the relevant work where it needs to be.

Response: Thank you for highlighting this issue. We have thoroughly revised this paragraph and incorporated relevant work (e.g., Paul et al., 2022) to support the updated content.

12. L53-59: Again, the authors cite the same references, while there is plethora of studies that use very similar datasets and methods. Diversify the reference list.

Response: We have reviewed more studies (e.g., Guo et al., 2023) on glacier surges and diversified the reference with studies identifying surge-type glaciers through terminus position changes (e.g., Vale et al., 2021), flow velocity variations (e.g., Gao et al., 2021), and glacier surface elevation anomaly (e.g., Chen et al., 2022).

Section 2

Section 2.1

13. L76: You have not defined High Mountain Asia at this point yet.

Response: We appreciate your attention to clarity. The term "HMA" (High Mountain Asia) is defined in the first paragraph of the Introduction section (Line 34), but we added a brief reminder here for better readability.

14. L94: I do not get why data is being mentioned here, when you do so more extensively in the following section. Consider removing.

Response: This is to highlight the advantages of selecting the NKG I as study area. We have revised it accordingly.

Section 2.2

15. L109: This is a very wordy sentence for not much. Please rephrase

Response: We have revised this sentence as follows: This study integrates multisource remote sensing data, including altimetry, optical and synthetic aperture radar (SAR) imagery, DEMs, and velocity maps, to investigate the surging dynamics of NKG I.

16. References for ITS_LIVE are incorrect. Please see the following and address: https://itslive.jpl.nasa.gov/#how-to-cite. Similarly, references for ICESat2 are incorrect: https://nsidc.org/openaltimetry/cite.

References to autoRIFT are also incorrect : Gardner, A. S., G. Moholdt, T. Scambos, M. Fahnstock, S. Ligtenberg, M. van den Broeke, and J. Nilsson, 2018: Increased West Antarctic and unchanged East Antarctic ice discharge over the last 7 years, Cryosphere, 12(2): 521–547, https://doi:10.5194/tc-12-521-2018.

Response: Thank you for pointing out this issue. We have updated citations for ITS_LIVE and ICESat-2 ATL06. The autoRIFT algorithm was initially developed by Gardner et al. (2018) for processing Landsat images. Subsequent work by Lei et al. (2021) enhanced its capabilities through integration with Geogrid, enabling its application to any type of imagery. We have now supplemented the citation to Gardner et al.'s foundational work.

17. The authors should make clear whether or not they generated the velocity time series themselves, or if they used the publicly available ITS_LIVE product. I am unsure as to which is which right now.

Response: We have revised the statement and clarified that the velocity maps used in our study are the publicly available ITS_LIVE product.

Section 3

18. The introductory sentence to section 3 brings very few information, I suggest removing it.

Response: We appreciate this suggestion. The introductory sentence in Section 3 has been removed as proposed.

19. After a read of this section, there is a crucial need for more references throughout the section.

Response: We sincerely appreciate the reviewer's constructive suggestion. We have comprehensively strengthened both the methodology and uncertainty estimation section, with additional citations from key references.

Section 3.1

20. L143-145: ". To address this, only data with time intervals ranging from 2 to 45 days were retained for the final analysis, ensuring more reliable velocity estimates."

I see the author's point here, but in practice, velocity estimates from longer baselines will always display lesser variance compared to estimates form relatively short baselines. I would rephrase and remove reliable as it doesn't mean much in this context, and rather use "[...] to ensure correct representation of surge signals within the velocity estimates."

Response: Thanks for this insightful comment. Our selection of 2–45 day temporal intervals (3–45 in the revised manuscript) for velocity analysis was to capture both the rapid velocity variations and peak velocities during surge events. We have revised our statement as suggested.

21. L150-151: This sentence is also pretty wordy and doesn't really bring any new information, I suggest to remove it.

Response: We have removed it as suggested.

Sections 3.3 and 3.4

22. Having no knowledge on the processing of data from laser altimeters and SAR, I will refrain from forming an opinion on both sections and will leave this to the Editor's discretion.

Response: We appreciate the reviewer's transparency regarding their expertise boundaries. We have further checked the processing of data from laser altimeters and SAR.

Section 3.6

23. This whole section is very weak and qualitative.

What the authors describe is quite cryptic and lacks a more quantitative approach.

ITS_LIVE is known to be highly uncertain and especially over-confident in its estimates.

So far, this section glosses over things far to quickly.

The authors need to describe what is being done, and provide actual quantitative estimates of the uncertainties within each data set.

Response: We acknowledge that the previous manuscript did not adequately estimate the uncertainties associated with the datasets and results. In the revised version, we have quantified the uncertainties associated with the datasets used, as well as with glacier velocity, terminus position changes, glacier/lake area changes, and glacier surface elevation. Please refer to Section 3.6 for revisions.

We appreciate this valid concern regarding ITS_LIVE's uncertainty. However, ITS_LIVE product offers high temporal resolution estimates of glacier velocity, which are valuable for capturing both long-term and short-term changes in glacier velocity. In this study, we have reduced the uncertainty of ITS_LIVE product by applying temporal and spatial averaging.

Section 4

Section 4.1

24. L300 : "[...] onset of the surge" and "Surge phase", replace with "active phase".

Response: Done. And "surge phase" in other sections have also been updated.

25. L301 and throughout: Refrain from using decimals in your velocity estimates, this is precision well under the signal-to-noise ratio for ITS_LIVE estimates.

Response: Thank you for this comment. We have rounded the glacier velocity estimates presented in the manuscript.

26. L320: The uncertainty reported by ITS_LIVE for measurements within this date range is 50-150 m/yr.

I would thus refrain from commenting on such minor velocity changes and how they relate to regional trends like the one studied in Dehecq et al. (2019).

Response: Although the uncertainty of the ITS_LIVE product may be relatively large, our results here represent the mean velocity within a specific subregion over a period of four years or longer, allowing to capture velocity variation of NKG I during quiescent phase. Furthermore, Yang et al. (2021) also reported glacier velocities below 60 m/yr (0.17 m/d) in Region A and B of NKG I in 2020, based on offset tracking using the GAMMA software. Their findings are generally consistent with ours, indicating that the ITS_LIVE product can also reliably represent relatively slow glacier flow.

The reviewer's doubts may also be partly due to the way we presented the results. Although there appear to be many outliers in glacier velocity across different regions, these points are relatively few in number. We have incorporated velocity point density into the visualization to make the results appear more robust and convincing.

27. L325: As said previously, avoid using "normal". What is meant here is "during quiescence". This whole sentence is very confusing and I am not sure I understand what the authors are referring to. I am not sure ITS_LIVE allows for the identification of seasonal velocity changes for NKG, given how noisy the data is.

Response: We have revised the wording as suggested, and have made corresponding changes to other instances where "normal" was used.

We have revised it accordingly in the subsequent revision. The reviewer's main concern is that the ITS_LIVE product contains considerable noise, which may limit its ability to capture the seasonal variation in glacier velocity. However, as we mentioned in our previous response, although there are some outliers in each time period, the utilization of Sentinel-1/2 imagery ensures a large number of observations in each time frame. The proportion of outliers is actually quite small and has little impact on the regional average.

Section 4.2

28. I think this section, as it stands, is quite problematic.

The authors put a lot of effort into quantitative descriptions and interpretations of supposed changes in regimes, without a clear and developed consideration of the uncertainties associated to each measurement.

The divisions of the presented time series into individual segments seem rather arbitrary given the general uncertainty in the data.

In particular, the "new phase of elevation increase" depicted to start in October 2023 needs to be further validated and commented on.

I do not see a physically valid reason as to why some segments exist (Figure 6 panels a,b and c) or why there should be different surface elevation change signals between regions A and B. Given the uncertainties from individual ASTER DEMs, I would consider panels a and b of figure 6 to show the same dynamics.

Also, Figure 1 shows ICESat2 footprints on region B, why are they not displayed on Figure 6 ? in other words, why does the time series for Region B stop in 2020 while there is still data? Does the GF-7 DEM not cover Region B?

The whole first paragraph of Section 4.2 is quite confusing. Please consider simplifying, and also, only mention regions that are shown, and show regions that are mentioned. I am unsure I understand which "upstream reservoir area" the authors are talking about on L340.

Paragraph starting L347: I doubt we can derive 0.9m/yr of increase in lake surface levels. The uncertainty reported by the authors here being approximately twice the magnitude of the reported increase, I would be more prudent.

Similar point to before, I do not believe the change in rate around 2012 to be physical, given the uncertainty of ASTER measurements.

For the final paragraph of this section, starting at L361: I think there is a problem with the GF-7 DEM, as it has shows a lot of aberrant values in higher altitudes (while supposedly being acquired in summer) and a general thickening of the main truck of a glacier post-surge.

The authors describe an advancing glacier front, which should be visible through dynamical thickening at the front of the glacier, but panel e) shows that the glacier front does not record changes in surface elevation - the authors even mention elevation gain at the terminus in the next section.

All of this makes me think that there must be problems in the authors' pipeline, either when the DEM is generated or co-registered.

As a more general point, all uncertainties reported in the plots, are - I assume - the uncertainties of the fit, assuming perfect data and not conditioned by data uncertainty.

Response: Thank you for your thoughtful and detailed comments.

(1) Regarding the uncertainty in glacier surface elevation, we have estimated the uncertainties in surface elevation in the revised manuscript and include it in the figures.

(2) Although the elevation uncertainty of individual DEM is relatively large (~ 10 m), ASTER DEMs share the same data source, resulting in small systematic biases in the elevation time series. This allows for reliable detection of continuous elevation changes in

a specific region, and the uncertainty decreases when a large number of DEMs are used (Ibarra et al., 2024). Additionally, regional averaging further reduces anomalous fluctuations in surface elevation. On this basis, the temporal trend of regionally averaged ASTER DEM elevations is considered reliable. We have also refined the segmentation of the elevation time series.

(3) The differences in surface elevation change signals between Region A and B are expected. Region B is located upstream of Region A and is at a higher elevation. During the quiescent phase, Region B experiences lower temperatures due to its higher elevation, resulting in smaller surface elevation changes and slower surface lowering rates. During the active phase, although both Regions A and B are part of the receiving area, Region A has a gentler slope, allowing more glacier mass to accumulate there, which leads to faster thickening in Region A.

(4) Initially, we used ICESat-2 data mainly to extend the Jason-3 time series. Since there are no Jason-3 footprint points in Region B, we did not show the ICESat-2 results for that region. We have included GF-7 DEM and ICESat-2 results for Region B and former glacial lake in the revised manuscript.

(5) The term “upstream reservoir area” in Line 340 was not clearly defined earlier. It refers to the area located approximately 12.5 km from the terminus. We have removed this sentence now.

(6) Since there are only 8 elevation observations for the former glacial lake area prior to 2004, the fitted rise rate in lake level may be somewhat uncertain. However, the overall trend of rising lake levels is expected. Moreover, the accelerated surface lowering observed around 2012 is considered reliable and coincides with the period when the glacier velocity in Region C decreased. As less glacier mass is transported from higher elevations and glacier melt has not slowed down. Actually, glacier melting in the former glacial lake area may have intensified due to rising temperatures. These factors likely contributed to the accelerated surface lowering in the former glacial lake area.

(7) Due to the viewing angle of GF-7 and image oversaturation, elevations in high-altitude regions from GF-7 suffers larger uncertainties. Please refer to our response to Comment 32 of Reviewer #1. We have reprocessed the GF-7 DEM to ensure the reliability of the results. Post-surge, the surface slope of the main trunk decreased, allowing more snowfall to accumulate in the higher-elevation areas, making thickening there reasonable. The thickening near the terminus is primarily due to glacier mass previously accumulated near Region A being transported downstream.

(8) The observed thickening near the terminus in Fig. 8 is valid. The missing values near the terminus in panel Fig. 7(e) resulted from our initial mishandling of DEM across UTM zones during processing. We have corrected this in the revised manuscript.

(9) The uncertainty shown in the figure represents the results of linear fitting. We have added uncertainties in elevation, velocity, and terminus position to corresponding figures in the revised manuscript.

29. L346: "destroyed" replace with drained.

Response: Done.

30. L355: This is just the elevation change rates computed by Hugonnet et al. (2021) correct? if so, cite the work there, if not, please explain how they differ.

Response: The results during 2000–2020 are elevation change rates computed by Hugonnet et al. (2021). We have cited the dataset now. For the 2020–2024 period, we conducted further analysis using our GF-7 DEM in combination with the Hugonnet et al.'s ASTER DEM.

31. L358: Regions A and B show exactly the same behavior with very minor elevation change, which is what is also shown in Figure 6

Response: Although Regions A and B may appear similar in Fig. 7, Region A is located downstream of Region B and shows more negative surface elevation changes (i.e., redder areas). This is consistent with the results shown in Fig. 6. To aid the reviewer's understanding, we have also added the boundaries of the regions A, B, and C in Fig. 7.

Section 4.3

32. L374: Not typically, Guillet et al. (2022) discussed that in HMA, around 1/3 surges result in terminus advance. I would replace with "Glacier surges can result in significant terminus advance".

Response: Thanks for this suggestion. We have replaced "typically" with "can".

33. L376-377: Same comment on the decimals, just write "around 40m" and mention over what period.

Response: We have revised them as suggested.

Section 4.4

34. L417: Point B referenced before the figure they appear on - it took me a bit of time to figure things out so please fix this.

Response: Thanks for this insightful comment. We have specified the locations of Point A and Point B in the preceding text.

35. Paragraph starting at L446: I am unsure as to what the authors refer to here.

They sequentially refer to main trunk and tributary, in a paragraph focusing on NKG V, the tributary. Please rephrase, as it is very confusing as it stands.

Response: We apologize for the confusion caused. In this study, the term "main trunk" refers to the tongue of NKG I, while the "tributary glacier" refers to the downstream tributary of NKG I (Fig. R6). We have revised it to improve clarity.

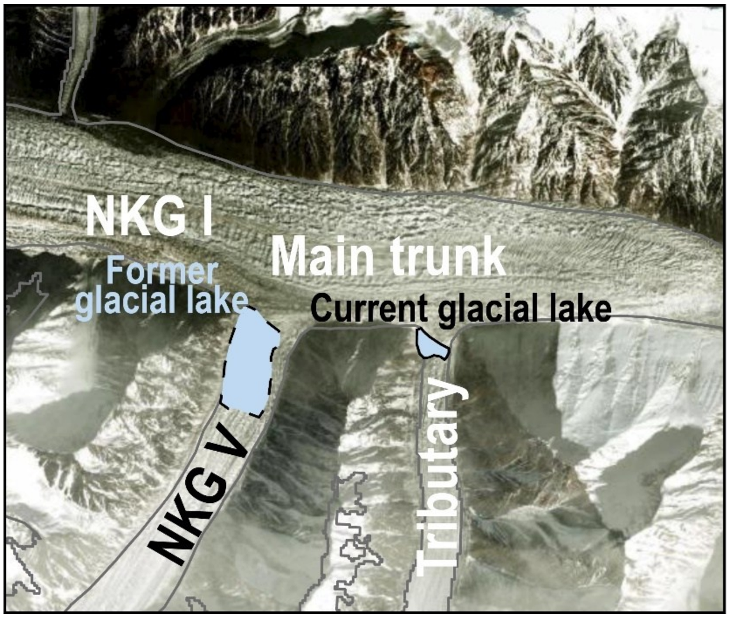

Figure R6: Locations of Main trunk, NKG V, tributary, and glacial lake. The basemap is derived from ESRI World Imagery (Credits: Esri, TomTom, Garmin, FAO, NOAA, USGS).

Section 4.5

36. Why analyse spatio-temporal correlations between different quantities in different regions? i.e. Flow velocity in B and elevation changes in A. The authors mention themselves that these quantities are "interdependent" - I would personally use the term "correlated". This sounds very arbitrary and needs to be further explained.

Response: We analyzed different variables across different regions primarily to investigate their temporal and spatial correlations. If the variables were from the same region, they would largely exhibit synchronous changes in time (e.g., Fig. 8 a and c). The term "correlated" is more appropriate.

We have removed this section in the revised manuscript.

37. L460: Remove the first line as it does not bring any new information.

Response: We have removed this section in the revised manuscript.

38. Paragraph starting L483: I am not sure I understand the point of this whole paragraph as the author are describing trivial glacier dynamics. I think I am missing something here. Please condense and rephrase.

Response: We appreciate the reviewer's feedback. Considering that this section provides limited information, we have removed it.

Section 5

Sections 5.2 and 5.3

39. I am not sure as to why this sub-section is in the "Discussion" section. The authors are still clearly presenting results, without discussing them much.

Move both into the results and actually discuss the results.

I am very doubtful of the interpretations proposed in sub-section 5.3 as, without proper modeling glacier behavior before and after the surges this whole sections seems very speculative.

As mentioned previously, there are very view references used to back up/discuss the authors' claims and re-frame them in a broader context.

The paragraph starting at L564 is the most problematic and speculative to me. These claims have to either be tested, demonstrated, and discussed, or removed altogether.

Response: We agree with the reviewer's comment that Sections 5.2 and 5.3 are still presenting results. We have moved Section 5.2 to the Results Section 4.5. Additionally, we supplemented the discussion on the surge mechanisms of NKG I and include comparisons with existing studies. Regarding Section 5.3, we conducted further discussion based on our results.

Conclusions

40. Given my previous comments, this section needs to be re-written in light of new results and discussions by the authors.

Response: As suggested, we have carefully reworked the conclusion to reflect the updated results and insights from the discussion.

Figures

All figures

41. None of the plots have error bars, either in the raw data or for proposed fits.

Captions need to be more descriptive to make the manuscript more appealing to the reader. What am I seeing? and what should I take home?

Response: Thanks for your valuable comment. We have updated the results in the figures and supplemented the uncertainties associated with the measurements. The figure captions have also been revised accordingly.

Figure 4

42. If velocities reached attain 1500 m/yr, why stop the colorbar at 500m/yr?

Response: Although the peak velocity of NKG V reached 1700 m/yr in 2004, most areas of NKG I exhibited velocities of only 100–200 m/yr in other years. Additionally, around 2017, the main trunk also showed velocities exceeding 500 m/yr. Therefore, we set the colorbar display range to 0–500 m/yr. If the range were set to 0–1000 m/yr or even higher, the velocity variations in most years would appear less distinct (Fig. R7).

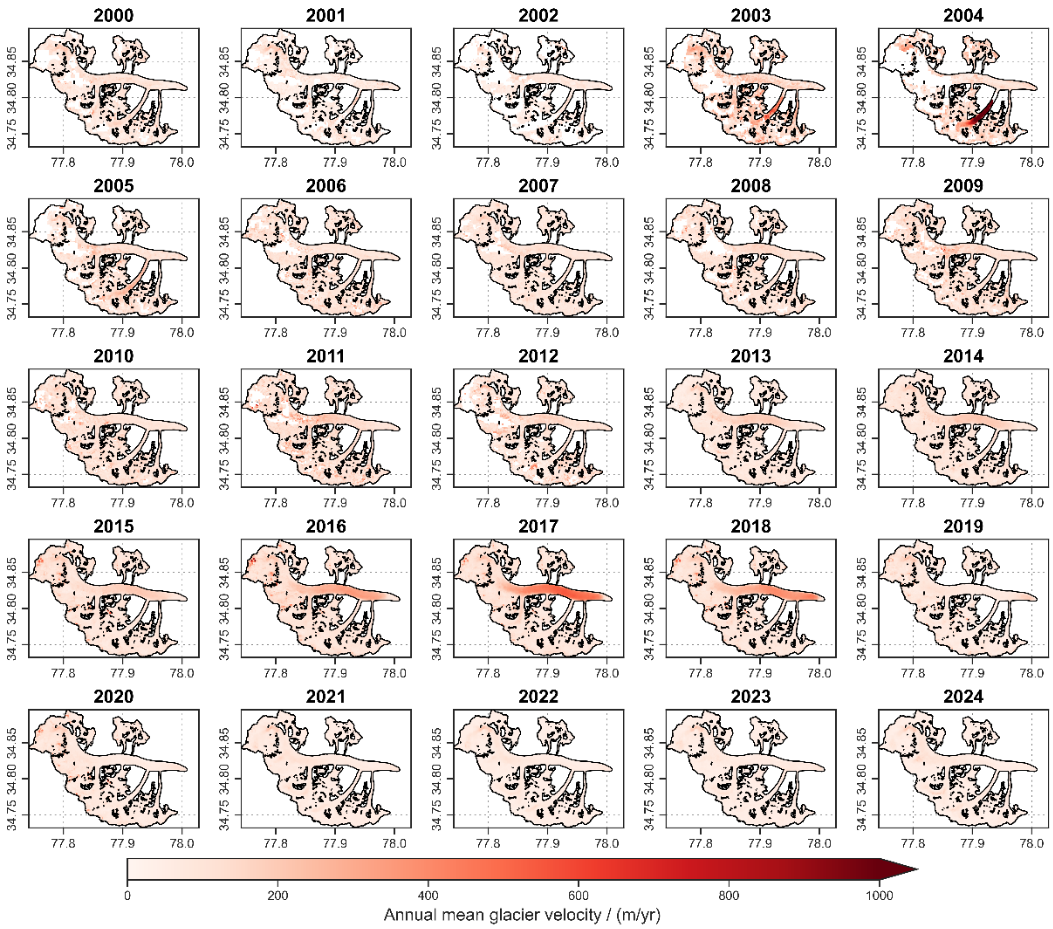

Figure R7: Modified Fig. 4. The colorbar range has been set to 0–1000 m/yr.

Figure 6

43. There are a few problems with this figure, which are directly related with my comments on section 4.2. In panel c), given the uncertainty associated with ASTER elevation measurements, I doubt that it is possible to define the 2012 transition point.

Same comment goes all panels within the figure.

Response: Thanks for your feedback. In Figure 6, we have added the uncertainties associated with ASTER elevation measurements, and re-segmented the elevation time series. The results indicate that the elevation change rate in the formal glacial lake indeed shifted around 2012. Please refer to Fig. 6 in the revised manuscript.

Figure 7

44. I think there is a problem with panel e). I cannot say if it is a problem with co-registration of the DEMs or what, but I am highly skeptical that the main trunk of the glacier has been overall gaining meters of mass following its surge. I strongly suggest the authors look further into their processing to understand what is happening here.

Response: Thanks for this insightful comment. We have further examined the production, co-registration, and subsequent processing of the GF-7 DEM. We excluded values with high uncertainties in high-altitude regions.

Between 2020 and 2024, the surface elevation did not increase uniformly along the entire main trunk. Instead, surface elevations in Region A, Region B, and adjacent areas have been decreasing, while elevations in the high-altitude areas and near the terminus have been rising. These observations are consistent with the results shown in the time series in Fig. 6 and Fig. 8.

Figure 8

45. Same comments as Figure 6 and Section 4.2.

Response: We have reviewed the segmentation of time series, presented the uncertainties of measurements, and changed the y-axis in panels (a) and (b) to distance for the terminus position changes.

Figure 10

46. The circle at profile C-C' is never explained in the caption even though it is in the main body. This ties into my general comment that captions need to be more informative.

Response: We have explained the location and purpose of the profile C–C' in both the main text and the figure caption. Fig. 10 has also been updated.

Reference

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