Response Letter

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 will carefully address these issues and supplement more details. Specifically, we will:

(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 will make every effort to quantify the uncertainties associated with the datasets used, as well as with glacier velocity, terminus position changes, glacier/lake area changes, and glacier surface elevation. We will also carefully review the data processing and systematic bias correction to ensure the reliability of the results.

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 will follow the same approach and update 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 will carefully review and supplement the relevant references in accordance with your suggestions, and thoroughly verify 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: We have removed it.

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 will comprehensively strengthen 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 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 will further check and improve 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 will quantify the uncertainties associated with the datasets used, as well as with glacier velocity, terminus position changes, glacier/lake area changes, and glacier surface elevation. We have completed initial revisions addressing this concern, please refer to our responses to Reviewer #1's comments 21 and 22 for details. Building on this, we will also provide a more comprehensive description of uncertainty estimation in the subsequent 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 will round 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 is the mean velocity 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. When we update Fig. 5, we will consider incorporating 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.

The corresponding result was primarily included to illustrate the intra-annual variation in glacier velocity. However, given that this section is not closely related to the rest of this part, we may consider removing it 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. In subsequent revisions, we will also consider filtering the ITS_LIVE observations before conducting our analysis to ensure the results are more robust and reliable.

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 will further clarify this aspect in the subsequent revision and include it in the figures.

(2) Although the elevation uncertainty for specific ASTER DEM pixel can be relatively high (~ 10 m), this uncertainty is reduced through regional averaging. On this basis, the temporal trend of regionally averaged ASTER DEM elevations is considered reliable. We will refine the elevation segmentation of the glacier surface with uncertainty in mind to obtain more robust results.

(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 will include the GF-7 DEM and ICESat-2 results for Region B 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 will clarify this in the text and reorganize the paragraph accordingly.

(6) The reported 0.9 m/yr lake level rise still needs further investigation. However, 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 have larger uncertainties. Please refer to our response to Comment 32 of Reviewer #1. 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. We will further examine GF-7 DEM to ensure the reliability of the results.

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

(9) The uncertainty shown in the figure represents the results of linear fitting. In the revised manuscript, we will consider the elevation uncertainties. However, we are confident that the elevation change rates and other results in each region are robust and that our conclusions remain unaffected.

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 will also add 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. R1). The wording in this paragraph may have led to misunderstanding, and we will revise it to improve clarity.

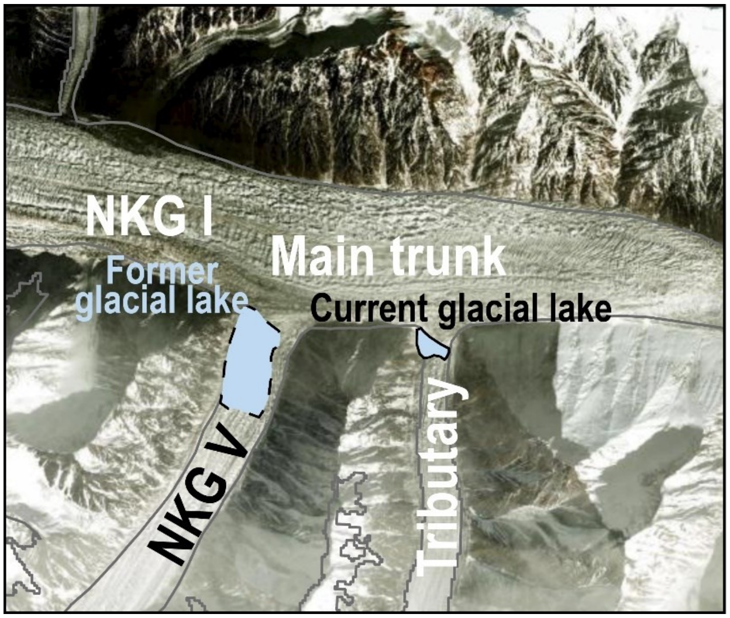

Figure R1: Locations of Main trunk, NKG V, tributary, and glacial lake. The basemap is derived from ESRI World Imagery (Credits: Esri, TomTom, Garmin, FAO, NOAA, USGS).

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, and we have replaced “interdependent” with “correlated”. We will further refine this section in subsequent revisions.

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

Response: Done.

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 and agree that the paragraph could be more concise. We aim to convey that due to the spatiotemporal correlations between different variables across different regions, it is possible to infer unknown or difficult-to-observe variables in one region based on known or more easily observable variables in another. We will revise this section for clarity.

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 plan to move Section 5.2 to the Results section. Regarding Section 5.3, we will revise it further and seek additional evidence to support our findings. We will also expand the discussion on the surge mechanisms of NKG I and include comparisons with existing studies.

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 will carefully rework this section 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. In our subsequent figures, we will take into account both the uncertainties of the raw data and the derived results. We will also improve the figure captions to help readers quickly understand our findings.

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. R2).

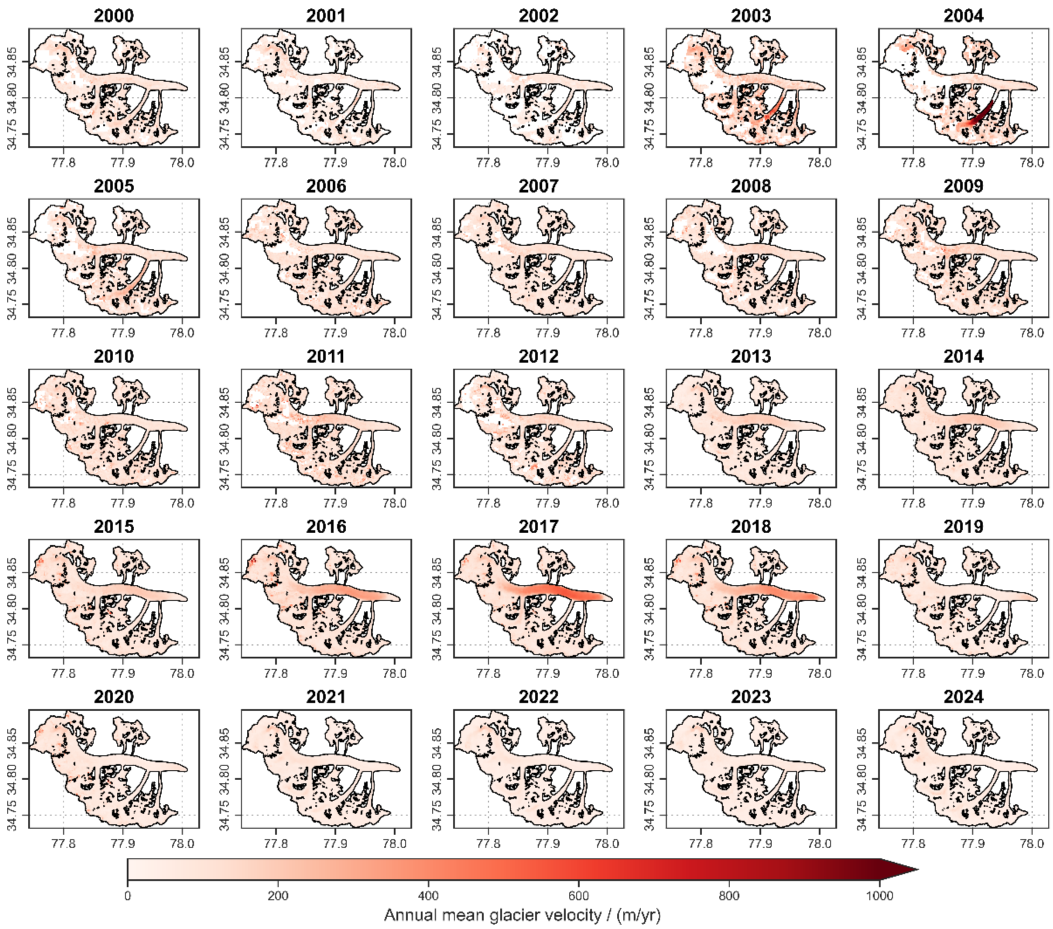

Figure R2: Modified Fig. 4. The colorbar range has been set to 0–1000 m/yr.

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. We will examine the segmentation and fitting of the elevation time series and display the corresponding uncertainties in the figures.

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 will further examine the production, co-registration, and subsequent processing of the GF-7 DEM, and update Fig. 7 based on the new results to ensure their reliability. However, due to image distortion and oversaturation in high-altitude areas, the GF-7 DEM may still exhibit relatively high uncertainty in high-altitude regions. We will further explore appropriate methods to handle data from these areas.

Figure 8

45. Same comments as Figure 6 and Section 4.2.

Response: In the revised manuscript, we will review the segmentation of time series, present the uncertainties of both the raw data and the linear fitting, and change 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. We will further update Fig. 10 to enhance its readability.

Reference

Beaud, F., Aati, S., Delaney, I., Adhikari, S., and Avouac, J. P.: Surge dynamics of Shisper Glacier revealed by time-series correlation of optical satellite images and their utility to substantiate a generalized sliding law, Cryosphere, 16, 3123-3148, https://doi.org/10.5194/tc-16-3123-2022, 2022.

Brun, F., Berthier, E., Wagnon, P., Kaab, A., and Treichler, D.: A spatially resolved estimate of High Mountain Asia glacier mass balances from 2000 to 2016, Nat Geosci, 10, 668-673, https://doi.org/10.1038/Ngeo2999, 2017.

Chen, W. F., Yao, T. D., Zhang, G. Q., Li, S. H., and Zheng, G. X.: Accelerated glacier mass loss in the largest river and lake source regions of the Tibetan Plateau and its links with local water balance over 1976-2017, J Glaciol, 67, 577-591, https://doi.org/10.1017/jog.2021.9, 2021.

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.

Evans, S. G., Tutubalina, O. V., Drobyshev, V. N., Chernomorets, S. S., McDougall, S., Petrakov, D. A., and Hungr, O.: Catastrophic detachment and high-velocity long-runout flow of Kolka Glacier, Caucasus Mountains, Russia in 2002, Geomorphology, 105, 314-321, https://doi.org/10.1016/j.geomorph.2008.10.008, 2009.

Gao, Y. P., Liu, S. Y., Qi, M. M., Yao, X. J., Zhu, Y., Xie, F. M., Wu, K. P., and Saifullah, M.: The Evolution of the Glacier Surges in the Tuanjie Peak, the Qilian Mountains, Remote Sens-Basel, 14, 852, https://doi.org/10.3390/rs14040852, 2022.

Gardner, A. S., Moholdt, G., Scambos, T., Fahnstock, M., Ligtenberg, S., van den Broeke, M., and Nilsson, J.: Increased West Antarctic and unchanged East Antarctic ice discharge over the last 7 years, Cryosphere, 12, 521-547, https://doi.org/10.5194/tc-12-521-2018, 2018.

Guillet, G., King, O., Lv, M. Y., Ghuffar, S., Benn, D., Quincey, D., and Bolch, T.: A regionally resolved inventory of High Mountain Asia surge-type glaciers, derived from a multi-factor remote sensing approach, Cryosphere, 16, 603-623, https://doi.org/10.5194/tc-16-603-2022, 2022.

Guo, L., Li, J., Dehecq, A., Li, Z. W., Li, X., and Zhu, J. J.: A new inventory of High Mountain Asia surging glaciers derived from multiple elevation datasets since the 1970s, Earth Syst Sci Data, 15, 2841-2861, https://doi.org/10.5194/essd-15-2841-2023, 2023.

Hugonnet, R., McNabb, R., Berthier, E., Menounos, B., Nuth, C., Girod, L., Farinotti, D., Huss, M., Dussaillant, I., and Brun, F. K., A.: Accelerated global glacier mass loss in the early twenty-first century - Dataset, Theia [dataset], https://doi.org/10.6096/13, 2021.

Jiskoot, H.: Glacier Surging, in: Encyclopedia of Snow, Ice and Glaciers, edited by: Singh, V. P., Singh, P., and Haritashya, U. K., Springer Netherlands, Dordrecht, 415-428, https://doi.org/10.1007/978-90-481-2642-2_559, 2011.

Ke, L. H., Wang, R. Z., Zhang, J. S., and Ding, X.: Remote-sensing characterization of surging glaciers in High Mountain Asia in the past two decades, Front Earth Sc-Switz, 12, 1499882, https://doi.org/10.3389/feart.2024.1499882, 2024.

Lei, Y., Gardner, A., and Agram, P.: Autonomous Repeat Image Feature Tracking (autoRIFT) and Its Application for Tracking Ice Displacement, Remote Sens-Basel, 13, 749, https://doi.org/10.3390/rs13040749, 2021.

Lovell, H. and Muhammad, S.: Multiple phases of ice-dammed lake formation and drainage associated with a surge of Shisper Glacier, western Karakoram, J Glaciol, 71, e10, https://doi.org/10.1017/jog.2024.80, 2024.

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.

Paul, F., Piermattei, L., Treichler, D., Gilbert, L., Girod, L., Kääb, A., Libert, L., Nagler, T., Strozzi, T., and Wuite, J.: Three different glacier surges at a spot: what satellites observe and what not, Cryosphere, 16, 2505-2526, https://doi.org/10.5194/tc-16-2505-2022, 2022.

Raymond, C. F.: How do glaciers surge? A review, Journal of Geophysical Research: Solid Earth, 92, 9121-9134, https://doi.org/10.1029/JB092iB09p09121, 1987.

Sharp, M.: Surging Glaciers - Behavior and Mechanisms, Progress in Physical Geography, 12, 349-370, https://doi.org/10.1177/030913338801200302, 1988.

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.

Vale, A. B., Arnold, N. S., Rees, W. G., and Lea, J. M.: Remote Detection of Surge-Related Glacier Terminus Change across High Mountain Asia, Remote Sens-Basel, 13, 1309, https://doi.org/10.3390/rs13071309, 2021.

Wu, J. H., Li, J., Guo, L., Li, L., Li, Z. Q., Zhong, W. J., Feng, J. J., and Kong, L. S.: Remote sensing monitoring and surge mechanisms analysis of the Amney Machen mountain Glaciers, Chinese J Geophys-Ch, 68, 1695-1710, https://doi.org/10.6038/cjg2024S0211, 2025.

Yang, J. R., Jiang, Z. L., Liu, S. Y., Wang, X., Zhang, Y., Zhang, Z., and F., W. J.: Characteristics of recent surging of Kunchhang Glacier, East Karakoram, Journal of Glaciology and Geocryology, 43, 1732-1745, https://doi.org/10.7522/j.issn.1000-0240.2021.0063, 2021.

Yao, X. J., Zhou, S. G., Sun, M. P., Duan, H. Y., and Zhang, Y.: Surging Glaciers in High Mountain Asia between 1986 and 2021, Remote Sens-Basel, 15, 4595, https://doi.org/10.3390/rs15184595, 2023.