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.

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General comments

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

Specific comments

Introduction

- 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.
- 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.
- 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.
- L31-32: I would avoid the use of "normal" and refer to something more neutral here like "quiescent behavior" or "quiescent flow".
- 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.
- L42 : Remove "transforming glacier morphology"
- 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.

- 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.
- 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.

Section 2

Section 2.1

- L76: You have not defined High Mountain Asia at this point yet.
- L94: I do not get why data is being mentioned here, when you do so more extensively in the following section. Consider removing.

Section 2.2

- L109: This is a very wordy sentence for not much. Please rephrase
- References for ITS_LIVE are incorrect. Please see the following and address : https://its-live.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.
- 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.

Section 3

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

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

Section 3.1

- 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."

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

Sections 3.3 and 3.4

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.

Section 3.6

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.

Section 4

Section 4.1

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

Section 4.2

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.

- L346: "destroyed" replace with drained.
- 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.
- L358 : Regions A and B show exactly the same behavior with very minor elevation change, which is what is also shown in Figure 6

Section 4.3

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

Section 4.4

- 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.
- 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.

Section 4.5

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.

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

Section 5

Sections 5.2 and 5.3

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.

Conclusions

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

Figures

All figures

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 ?

Figure 4

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

Figure 6

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.

Figure 7

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.

Figure 8

Same comments as Figure 6 and Section 4.2.

Figure 10

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.