Comments on egusphere-2025-652

I want to commend the authors for their diligent and thoughtful incorporation of the points raised in my previous review. The revisions demonstrate a clear commitment to improving the clarity, rigor, and overall quality of the manuscript.
That said, while the progress is evident and highly appreciated, I believe the manuscript would benefit from a bit more refinement in certain areas to fully realize its potential. I am confident that with these final adjustments, the work will achieve the impact and precision it deserves.

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General comments

- Several integers in the paper are formatted as X,XXX (L119 and 136, at least). I think the Copernicus format requires integers as XXXX : https://publications.copernicus.org/for_authors/manuscript_preparation.html#math
- I still think that a lot of the points made by the authors in section 5 are problematic and should be reworked or at least reformulated.
- Throughout my comments, I have made numerous reference to my own paper Guillet et al., (2025). I am aware that it sounds like plugging my own work in here, however, I think both studies address a similar problem, with similar methods and I do think there is great value in discussing your results with regards to my earlier findings. I leave this choice to the discretion of the authors.

Specific comments

Section 3

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

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

L156-165: This section glosses over a few important details which I think should still be addressed, at least briefly: "High-quality DEMs from pre- and post-surge" - what defines high quality ? and which DEMs are these ? Be more specific here. If these are ASTER DEMs, I would refrain from referring to them as "high-quality".

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

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

Equations 7 and 8 : This is a bit of a headache - and I should have caught this problem earlier. I want to make it clear that this is a common mistake in the glaciological community so I didn't expect the authors to know about it.
The authors here correctly claim that equation 8 is derived from Jakob and Gourmelen, 2023. However, Jakob and Gourmelen in turn cite McNaab et al, 2019 as their main source - I could not find any trace of this equation in McNaab et al., 2019.
What has me worried is the formulation of the n_{eff} which, in previous work, I had tracked down to a wrongly-derived equation in Gardelle et al., 2013.
I recommend the authors use the formulation of the n_{eff} proposed by Hugonnet et al (2022) - in the supplementary information.
Given the spatio-temporal averaging used by the authors. I suspect that both results will be somewhat similar.

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

Section 4

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

L360: "This pattern aligns with the deceleration trends observed in other HMA glaciers (Dehecq et al., 2019)"
I would remove this statement. The deceleration mentioned in Dehecq et al 2019 is related to a reduction in driving stress driven by glacier mass loss.
The deceleration mentioned by the authors here, is related to the termination of a transient event and a return to the glacier's meta-stable condition.
The second part of this statement is also problematic, as the authors mention "pronounced deceleration phases before their respective surge", which is a statement contradicted by Figure 5. I suggest to remove it.

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

Section 5

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

L544-554: I find these explanations very far-fetched and a bit over-interpretative.
First, hydraulic vs thermal control is not a consensual approach to the study of surges anymore. I would refrain from using this dichotomous view.
Then, the authors are here clearly over-interpreting results : what we can see is a surge with seasonal increases in velocity (peak 2 and 4 as defined by the authors) which likely stem from meltwater influx, further destabilising the glacier. What it shows is that the surge, and the stability of the glacier during this period, are heavily affected by meltwater influx, and that, the abrupt influx resulting in peak is likely what allows to drain, thus dissipating excess thermal energy stored and terminating the surge.
Finally, the authors only discuss their findings in regards to that of Jiang et al. (2021).
I suggest having a look at - at least - Quincey et al (2015), which is cited by the authors, and Guillet et al,. 2025 in which this hydraulic vs thermal control of surges is widely discussed. This is a part of the paper where the authors need to situate their research within the broader literature and I see a significant problem in referencing the Benn et al., 2019 paper and still using the thermal vs hydraulic control of surges hypothesis.

L564: "modifying the internal drainage system" replace with "modifying the glacier's enthalpy budget"

L567-57o: I see the point made by the authors here and I think there is value in this comparison. I wonder about the reliability of the Millan et al (2022) estimates, and would like to know about their respective error bars over NKG I and V.
I would further argue that actual estimates of the driving stress are needed to make such a point.

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

L583: "After the 1980 surge, the terminus continued to advance for nearly ten years before stabilization, and it then remained stable for almost two decades" - if the terminus is still advancing, then the glacier is most likely still surging.
You mention this further on Line 587 as "post-surge terminus advance" - I do not think such a thing exists and would argue that while a surge-type glacier is advancing, it should be considered as actively surging.