Review of egusphere-2026-913: Modulation of glacier surge cycles on decadal to centennial timescales by intrinsic thermal-structure variability

William David Harcourt, University of Aberdeen

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Summary

The study of Nolan et al. (2026) utilises an idealised modelling set-up, based upon existing data and knowledge of a polythermal surge-type glacier in Alaska-Yukon, to study the effects of glacier surges on thermal regime. The key finding is that as glaciers surge, the changing ice thickness distribution alters the glacier thermal regime, leading to smaller and colder glaciers. This is an interesting finding, which although known before, has not been studied in detail. In particular, the authors find a secondary, low-frequency oscillation related to enthalpy and ice thickness that leads to variations in velocity during a surge and hence affects the surge magnitude. The authors clearly outline the limitations of their modelling approach and thus caution over-interpretation of this secondary oscillation, which I agree with, but points to a potential process by which thermal regime may change post-surge. Overall, I think the paper is well-written and detailed, providing a useful study to understand long-term impacts of surges on glacier thermodynamics.

General Comments

The technical details of the modelling appear to be detailed and well-written. The more substantive points I want to raise are the following:

1. The discussion could provide greater depth of glaciological interpretation. The results section is dense and it would be useful to extract the key findings and provide some further context in the discussion. For example, the long-term impact of surge magnitude is a key finding and points to a potential mass loss mechanism that has not been widely observed. In Svalbard, Tunabreen is one place where up to 5 surges over have been observed since the late 1800s and each surge has seen a reduced maximum terminus position. This provides a useful location to compare direct observations with the modelling results in this study.

2. Secondly, it would be interesting to hypothesise possible mechanisms for the secondary, low-frequency oscillation in a concise and clear way. After a surge passes some basal friction threshold, would we see the next set of surges appear progressively 'smaller' and then as enthalpy reorganises itself (how could it do this?), the surge magnitude then increases? I think a lot of this in the text already, I just felt reading through that a clear process-based explanation was lacking.

In essence, both points above relate to clarity over the processes underpinning the key results - some adapted text in the discussion will solve this. My other comments are detailed below in the technical comments.

Technical Corrections (References to line (L) numbers in preprint)

L2: Do you mean 'initiation'? It is probably fine to say 'initiation and propagation'.

L8: This thermal regime structure would impyl that your results pertain to down-glacier propagating surges (as opposed to upward propagation surges that have been observed at tidewater glaciers?). I think this should be stated here and also referenced in your discussion because up-glacier propagatig surges may not have the same characteristics.

L11: The oscillation is for volume, temperature, and and enthalpy.

L12-16: These final sentences would benefits from a succinct summary of the what this oscillation mechanism may represent e.g. could it that the high magnitude surges can force 'mini surges' of isolated spatial regions?

L18: Can you say why they have 'fascinated' glaciologists for so long? Something saying they deviate from traditional glacier physics could be mentioned...

L26-35: This section could be expanded a bit to more explicitly address the drivers of thermal regime in the future e.g. impacts of warming on meltwater reaching the bed, ice thickness changes due to negative SMB, dynamic thinning at tidewater glaciers and subsueqent advection near glacier margins, enhanced crevassing leading to increased local air turbulence and melt etc.

L34: Unclear what is meant by 'past observations' - do you mean recent measurements (i.e. in the last 20 years) or palaeo-evidence?

L36: As you've mentioned Alasja-Yukon above, it may be useful to state the general length of surge cycles in this region.

L49: I am wondering why you decided to amalgamate observations from different glaciers instead of focusing on a glacier with more detailed observations - or possibly such a 'well monitored' glacier simply does not exist?

L141-149: For clarity, it might be worth stating in this section that the z_s (and therefore volume) vary as the simulations progress - this is particularly important as volume / thickness has a significant control on thermal regime.

L185-188: Is this a gridded product? If so, what is the resolution of the product and did you average the 2 m air temperature data over the study glacier?

L243: Why was 3 ka chosen?

L256: 'active' rather than 'fast'?

L258: Are these basal slip values determined from observations or other modelling studies?

L259-260: If sliding is restricted to lower elevations, how does the velocity of the upper elevation regions increase during a surge?

L260: Is the 4 ka quiscent phase time period representative of the Alaska-Yukon region?

L340: Should there be numbers here?

L347: How do crevasses shorted the thermodynamic response time? Meltwater input to the bed?

L401: This is the first time where DBL is introduced - I think it should be defined explicitly to help with clarity.

L495: As per my earlier comment, it's important to highlight here that your approach specifically relates to downward propagating surges.

L526: Could also cite our new paper focused on Svalbard: Harcourt et al. (2026)

L600: This final sentence could be explored a bit further in the text, at least through a discussion. In Svalbard, recent studies (e.g. Nanni et al., 2025) have highlighted the increasing role of surface melt on surging processes, particularly where meltwater can be trapped at the bed and enhance sliding in the accumulation/reservoir zone. It would be interesting to contrast this with your results highlighting a difference mechanism i.e. thermal regime shifts due to surge characteristics. I believe the two are independent processes acting together at the same time, particularly where surge cycles are short, such as the case of Yukon (possibly less so in Svalbard with quiescent periods of $\sim$100 years).

Figure 1: The x-axis for panels b and d could be misinterpretd as being shared - could you plot the density x-ticks belowthe graph?

Figure 5: I find it odd that the smallest number is at the top of the colour scale (I understand it is because low basal friction = higher surge propensity) - it may make sense to invert the colourscale?