Response to reviewers

We thank the editor and both reviewers for their evaluation of our manuscript. Below we respond point-by-point in blue to the original reviews reproduced verbatim in black.

Review of Nolan et al. (2026): Modulation of glacier surge cycles on decadal to centennial timescales by intrinsic thermal-structure variability

Yoram Terleth

This paper presents a series of model experiments with a thermo-mechanically coupled ice flow model. The work investigates the response of the thermal structure of small polythermal glaciers to strong increases in ice velocity akin to glacier surges. The paper evaluates the impact of and recovery from individual surge events as well as the broader impact of cyclical ice velocities and (very) long term thermodynamic feedbacks. I think this is a great paper that describes a very interesting, and somewhat niche, aspect of glacier surging. The writing is clear and concise throughout, and the figures are well made. I only have minor comments that I have listed below with reference to the line numbers in the pre-print.

Thanks, Yoram! We appreciate your careful reading and accurate reflection of the intention and outcome of our work.

1 Minor comments

• Fig. 1 and ll. 178-193: I understand the logic of using surface forcings available for Kaskawulsh glacier, and I appreciate that there is a mention that Kaskawulsh is representative of the wider region. I wonder if the authors expect the specific conditions on RGI60-01.16732 to also be represented well by the data from Kaskawulsh? I am not super familiar with the particular climate but I imagine these small polythermal glaciers can sit in cirques with surface conditions that are different than the regional average. Since it seems that most spin up runs yield similar thermal structures this is probably not super important, but it might be nice to make a mention of it.

Indeed we would expect the smaller glaciers to have a different energy balance (and therefore mass balance) than the large Kaskawulsh Glacier, with shading likely playing a bigger role in ablation and avalanching likely playing a bigger role in accumulation, for example. However, we have well calibrated mass-balance models for very few glaciers in the region and Kaskawulsh happens to be one of them (Robinson et al., 2025a, 2025b). Although the Kaskawulsh Glacier is larger than our study glacier, its multi-decadal geodetic mass balance is indistinguishable from the regional average, so we have some confidence in using it to produce a plausible, rather than site-specific, mass-balance forcing for the synthetic glacier that is the focus of our study. In the originally envisioned work, we planned to use 9 different glacier geometries in the modelling exercise (see Figure 3.7 in Nolan, 2023) and attempted a Bayesian inversion for mass-balance model parameters. In this exercise, the mass-balance–elevation profiles of all the decidedly continental glaciers were similar (including the glacier that inspired the current study), whereas they were different for the two glaciers (Fisher and Tweedsmuir) that were closer to the coast. This observation adds to our confidence that using an adjusted model originally calibrated for the Kaskawulsh Glacier is reasonable. Our expectation is that the mass-balance–elevation profile derived for the Kaskawulsh is a defensible starting point for generating a mass-balance forcing for the small synthetic glacier. As the reviewer notes, the resulting glacier thermal structure is not very sensitive to the details of the imposed mass balance.

Following this text:

"The geodetic balance of the Kaskawulsh Glacier has been negative and closely follows the regional average over several decades (Berthier et al., 2010), suggesting that this glacier is representative of the larger region (including our area of interest ∼25 km away)."

we will add:

"Although small and large glaciers are likely to have different energy balances, preliminary work suggested the mass-balance–elevation profiles of selected small and large glaciers in the area are similar, provided the glaciers were on the continental side of the range (Nolan, 2023)."

• l.259: I might have missed this, but with the zero sliding for cold beds condition, does the entirety of the bed briefly become temperate during simulated surges? Or does the toe of the glacier just not undergo basal sliding at all?

With the exception of the sliding being suppressed under steep slopes at the headwall, sliding is free to occur anywhere else the bed is or becomes temperate. To clarify, we will change the following text:

"Sliding can only occur where the ice–bed interface is at the pressure melting point (Eq. 20). We further restrict sliding to zb ≤2400ma.s.l. to inhibit excessive drawdown in the steepest highest region of the accumulation zone."

to

"Sliding can only occur where the ice–bed interface is at the pressure melting point as determined by the thermo-mechanically coupled model (Eq. 20). During the surge-like perturbations, the bed thermal state is free to evolve, with basal friction reduced everywhere the bed is temperate. We restrict sliding to zb ≤2400ma.s.l. to inhibit excessive drawdown in the steepest highest region of the accumulation zone."

• ll.359-364: I think the result that surging behavior results in smaller glaciers could be hedged a bit more: surging results in smaller glaciers in the model, but it seems like this might only be the case because surges happen on a timer in the model, regardless of mass increase in the reservoir zone.

This conceit of the model does probably contribute to the effect of more vigorous surges leading to smaller glaciers. The field-based observation that surge-type glaciers are smaller than their non-surge-type counterparts in Iceland (Adalgeirsdóttir et al., 2005) leads us to expect that surge vigour also has an effect on glacier size. In light of the reviewer's comment we will add:

"The magnitude of this effect may be exaggerated in the model given that we prescribe surges at regular intervals in time rather than on the basis of surge recovery."

after

"Surging results in smaller (Figure 5a) and colder (Figure 5b and c) glaciers relative to the initial condition. Surging has previously been shown 360 to alter glacier mass balance and therefore volume (Adalgeirsdóttir et al., 2005), but here we further demonstrate that glaciers become smaller with increasing surge magnitude (decreasing β2 in Figure 5a)."

• l.526: In the Yukon (?)
We are not certain what this query refers to around line 526. We refer to "Yukon/Alaska" on line 525 in the submitted manuscript:

The environmental conditions needed to achieve the thermal structure used in this study are not unique to Yukon/Alaska but also occur in Svalbard (e.g. Sevestre et al., 2015; Lovell et al., 2026), another location with a high concentration of polythermal and surge-type glaciers (e.g. Jiskoot et al., 2000). While the thermal regimes of large surge-type glaciers in Yukon are largely unmeasured, polythermal conditions have been observed based on borehole measurements at Steele Glacier (Jarvis and Clarke, 1975), where vertically extensive cold ice in the upper reaches of the glacier was detected in multiple boreholes. Future work

• ll.478-490: It could be worth discussing whether assigning surge as a thickness threshold rather than a set time would still yield an overall smaller and colder glacier as well.
I am also curious what motivated the choice to set surges at specific times rather than at a threshold in mass, it seems like the second approach would also be worth pursuing.

We expect that this alternative approach to triggering surges in the model would lead to qualitatively similar (but perhaps quantitatively different) results based on the observations of Adalgeirsdóttir et al. (2005) mentioned above. Indeed this alternative was, initially, the preferred approach, and one we intended to compare to the approach described in the manuscript. As mentioned, it proved computationally infeasible within the project timeline, so remains regrettably unexplored. We will revise that last part of this section to read

"While some surge-type glaciers adjust their recurrence intervals to mass inputs (e.g. Eisen et al., 2001; Striberger et al., 2011), others continue to surge at a nearly constant recurrence interval (e.g. Abe and Furuya, 2015; Bevington and Copland, 2014; Nolan et al., 2021), irrespective of changes in mass balance. The observation of (nearly) constant recurrence intervals, especially under negative mass-balance conditions (e.g. Zemp et al., 2019; Young et al., 2021), lends some support to our approach. The alternative approach, in which surges are triggered when a mass or thickness threshold is reached, is well worth exploring in future work. Given the observations Adalgeirsdóttir et al. (2005), we hypothesize that this experiment would yield qualitatively similar results with more vigorous surges producing smaller colder glaciers."

• ll.524: What is meant exactly by ”fundamentally unchanged”? Are there maximum/ minimum lengths of the temperate bed outside of which the oscillation would not (re) emerge?

What we meant there by “fundamentally unchanged” is that the thermal structure is cold-layer polythermal (as opposed to, for example, warm-core polythermal or polythermal with a cold accumulation area and warm ablation area), rather than a threshold of min/max temperate bed length. The latter is something we didn’t explore; since the temperate bed length is not prescribed but is rather an outcome of the model, it would have to be manipulated indirectly in order to run an experiment. I suspect as one approaches a fully temperate and fully cold bed, the oscillations could be suppressed. To clarify our meaning, the revised text would read:

“Provided that the thermal structure-–where warm ice from the accumulation area is overlain by a cold shell in the ablation area—remains fundamentally unchanged, we would still expect the secondary oscillations to emerge.”

• ll.533-551: I think that the comparison with the ice stream activation stagnation cycle is very interesting but I was a little confused by the first part of this paragraph (roughly until l.540). The text mentions the absence of a link between Heinrich events and climatic/oceanic cycles, but doesn’t really link that to the mechanism modeled here. I think this paragraph could be shortened and focused a bit more on the comparison with ice-stream cycles. Are there any other examples where secondary oscillations emerge as a consequence of primary oscillations?

Upon re-reading we suspect the confusion arises because we neglect to mention the mechanism of flow reduction (conductive cooling through thin ice resulting in a bed transition from warm to cold), which is the same as in our model. To clarify the mechanism and hopefully alleviate the confusion, we will shorten and revise the first paragraph to read:

“Ice stream activation–stagnation cycles, initially formulated by MacAyeal (1993) as an internal mechanism for Heinrich Events (Heinrich, 1988), are an example of thermodynamically controlled oscillations. ~~While representations of these cycles have varied from zero-dimensional lumped-parameter models (e.g., Robel et al., 2013) to complex ice-dynamic models (e.g., Marshall and Clarke, 1997; Feldmann and Levermann, 2017), the~~ The underlying mechanism is similar to what we model for surging glaciers: the ice–bed interface oscillates between cold and temperate conditions, where ice streaming (i.e., sliding) is enabled during temperate conditions, while conductive cooling of the thinning ice mass leads to bed refreezing and therefore flow stagnation. ~~The internal nature of the thermal oscillations and independence from atmospheric and oceanographic drivers (cf. Bassis et al., 2017; Mann et al., 2021) has hindered their ability to explain the observed relationship between Heinrich events, Dansgaard–Oeschger cycles, and variability in Atlantic meridional overturning circulation (Marcott et al., 2011). Nevertheless, the mechanism initially proposed by MacAyeal (1993) does serve~~ This mechanism has also been invoked to explain the past and present stagnation of ice streams along the Siple Coast of

Antarctica (Robel et al., 2013) which appear to occur on shorter timescales than Heinrich Events (e.g. Catania et al., 2012)."

We will leave the second paragraph as is, because it focuses on the ice-stream comparison and explicitly compares and contrasts the surging glacier case with the ice-stream case.

Tidal forcing/response could probably be used as an example of primary oscillations triggering secondary ones (e.g. King et al., 2010), in some cases at much lower frequencies as in our study. We are reluctant to introduce examples where the variables and mechanisms differ significantly from those at hand and would prefer to retain only the discussion of ice-stream activation/stagnation.

• ll.577-580: If I understand correctly,this section carefully hints at the link between internal thermomechanical feedbacks and the examples of polythermal glaciers seemingly "shrinking" and "cooling" outside of the surge envelope (which before was ascribed to changing climate). I understand that this might be too speculative, but do the authors think that the "secondary" oscillation could in turn affect individual "primary" surge Cycles?

That would be one logical extrapolation of our discussion (glad it came across as careful as we intended). Another would be that the secondary oscillations could also make surges more and less vigorous than they otherwise would be, even in a stationary climate. Our preference is not to extrapolate explicitly in the text beyond what is already there. The far-from-stationary climate makes us cautious about suggesting that the secondary oscillations would emerge in any detectable way in reality.

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

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July 27, 2026

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, lowfrequency 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.

Thanks for the careful reading and accurate summary of our work!

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.

Thanks for the suggestion. We hesitate to attribute glacier-specific observations to the mechanism described in this paper, given the likely dominance of climate-driven changes, but instead will add to the discussion to emphasize what the implications would be if the secondary oscillation were real and observable. We propose to lead Discussion section 5.3 (Likelihood of observing the secondary periodicity) with the following text:

“Were the secondary oscillations uncovered in this study at work in the real world, they could contribute to long-term glacier mass loss through climate-independent processes. Such findings are important when engaged in attribution studies and climate inference (e.g. Roe and O’Neal, 2009; Roe 2011). Indeed, previous work has highlighted the confounding effects of glacier surging and tidewater glacier dynamics that make it challenging to relate regional climate change to glacier variation, especially on short time scales (e.g. Arendt et al., 2008). The oscillation described in this study which acts over multiple surge cycles would further complicate the relationship between climate and glacier change.”

Later in this discussion section where the climate-driven change to glacier surge cycles is noted, we would add a citation to the Tunabreen study by Flink et al:

“Finally, given that climate-driven glacier mass deficits appear to undermine surge propensity and/or vigor (Dowdeswell et al., 1995; Frappé and Clarke, 2007; Copland605
et al., 2011; Bevington and Copland, 2014; Flink et al., 2015), it stands to reason that these deficits would also undermine the secondary oscillations, which depend on a threshold mass flux”.

Beyond this, we are reluctant to draw too close a connection between our model results and glacier observations, given the numerous limitations to our approach that are detailed in the discussion. We view this study as an exploratory one that will, ideally, motivate future work with more sophisticated models (e.g. 3D models with more physically-based surface energy/mass balance schemes) that can be applied to real glaciers well constrained by observations.

2. Secondly, it would be interesting to hypothesise possible mechanisms for the secondary, lowfrequency 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.

Section 4.2.1 in Results and Figure 6 were intended to convey the process-based mechanism by which the secondary oscillation occurs, by inspection of the spatial and temporal evolution of key variables. Upon rereading, we can see why the immediate presentation of detail may have obscured the bigger picture. In order to more clearly and concisely convey the physical mechanism at work, as requested by the reviewer, we will add a short preamble to the details of 4.2.1 to give a high-level overview of the details that follow:

“Figure 6 summarizes the physical mechanism responsible for the secondary oscillation through inspection of key variables in space and time. In essence, sufficiently vigorous surges overextend the temperate portion of the glacier bed through rapid advection of warm ice. Following such a surge, the sliver of temperate ice that remains near the bed in the receiving area cools, allowing a transition from temperate to cold conditions along a significant fraction of the glacier bed. Initiation of the subsequent surge is therefore restricted to a diminished fraction of the glacier bed. Over multiple surge cycles, the maximum extent of the temperate bed increases until the cycle repeats itself. Mechanistic details are described below. ”

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

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

Propagation is what we had in mind, but initiation and propagation also makes sense. We will amend to “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.

Fair point. The only propagation mechanisms included in the model are those that would arise from the Stokes equations and thermomechanical coupling, and certainly the study is restricted to land-based glaciers. Rather than making the abstract longer with new text, we will add text to the end of the Introduction so that it specifically states we focus on land-based glaciers and that nothing outside of ordinary model physics is included to represent surge propagation. This text would read:

“With polythermal glaciers in mind, we seek to determine whether glacier surging alters thermal structure on long (multisurge) timescales, and whether this could have a feedback on surge character and propensity. We focus on small ($\lesssim$ 15 km in length) land-terminating glaciers in this study, as the harbingers of change whose surge cycles and thermal structures are known to be evolving. … We use the thermomechanically coupled ice-flow model Elmer/Ice (Gagliardini et al., 2013) to produce a steady-state reference glacier, which we then subject to perturbations of basal friction to produce events of varying magnitude and recurrence interval that resemble surges. No explicit surge-propagation mechanisms are represented in the model outside of those that arise naturally from Stokes flow and thermo-mechanical coupling. By observing the glacier’s response to these perturbations and its evolution on various timescales, in the absence of climate variations, we are able to isolate the intrinsic variability that arises from coupled thermomechanical processes.”

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

We will amend to “oscillation in volume and enthalpy”. Because enthalpy encompasses temperature and the variable we use is “fraction temperate” rather than temperature, we prefer not to list temperature along with volume 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?

We will add a sentence to the abstract to attempt to succinctly convey the bigger picture interpretation of this oscillation, in the spirit of the above comment. In context, this will read:

"This oscillation reflects variations in the length of the temperate (thawed) portion of the glacier bed, induced by imbalances between horizontal advection of temperate ice and vertical conductive cooling. Its amplitude and period are a function of surge vigor, with periods ranging from ~ 150–450 years. This work suggests that surge magnitude may be affected by a glacier's surge history over multiple cycles purely through thermomechanical feedbacks. While the existence, let alone detectability, of such a secondary oscillation in a non-stationary climate may be questioned, this result motivates a more thorough attribution study of the evolution of surge character in polythermal glaciers."

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

We will expand this text to read:

"Glacier surges are quasi-periodic fast-flow events that have long fascinated glaciologists (e.g., Meier and Post, 1969, 1987; Clarke, 1987; Truffer et al., 2021) due to their departure from typical glacier behavior characterized by slow viscous flow, their complicated relationship with climate (e.g. Sevestre and Benn, 2015), the window they offer into basal processes (e.g. Beaud et al., 2022), and the pure spectacle they offer to the field observer. Surges are recognized as part of a spectrum of ice-flow instabilities that represent internal or unforced dynamics, rather than climate-driven variations (Clarke, 1987; Lovell et al., 2026)."

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.

We will expand this text to include some of the drivers of thermal evolution as follows:

"Projected changes in thermal structure under a warming climate include both warming and cooling of glaciers (e.g., Irvine-Fynn et al., 2011; Gilbert et al., 2012; Gusmeroli et al., 2012). These divergent pathways arise from the interplay between the effects of increasing air temperatures and declining glacier volume, including increased surface ablation and changes in latent heat production, strain heating, and the advection and diffusion of heat within a glacier (e.g. Wilson and Flowers, 2013). Together, past observations (e.g., Sevestre et al., 2015) and future projections (e.g., Wilson et al., 2013) motivate our interest in elucidating the mutual influence of glacier surging and thermal structure in the absence of climate change."

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

Yes we mean modern obs rather than paleo, so will change this to “modern observations”. “Recent” might not fit the citation from 2015 in some readers’ minds.

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

Good idea. We will add this as follows:

“In approaching this problem from a fundamental as opposed to glacier-specific perspective, we adopt a two-dimensional (flowband) idealized glacier geometry inspired by a real surge-type glacier in Yukon, Canada (Figure 1a & c), where surge return intervals are on the order of decades with some exceptions.”

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?

The purpose of this study was always intended to be generalizable and therefore somewhat stylized. Originally, we set out to do this work on numerous glaciers in the region with different characteristics (namely size and geographic location). The scale and pace of the project, however, required triage, and we ended up using only one synthetic glacier, chosen as representative of many in the area. There are two well studied glaciers of similar size in the region (Trapridge Glacier, South Glacier), but we don’t think the site-specific data for these glaciers would have improved the work at this early exploratory stage, because the data represent snapshots in time. Conceivably it would make more sense to do this work on a glacier that has an established observational record over multiple surge cycles; these exist for some glaciers in the region whose thermal structures are not known but are suspected to be temperate.

We will attempt a better justification of our approach by adding the following text to the end of the paragraph: “At this exploratory stage of investigation, our aim is to understand the generalized dynamics of small polythermal surge-type glaciers rather than seeking model fidelity to site-specific observations, hence our idealized approach.”

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

We will emphasize this in introducing the free-surface equation as:

“Glacier surface elevation and therefore volume are permitted to evolve in the time-dependent (i.e., prognostic) simulations according to the free surface equation…”

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?

The mass-balance model can be applied at the point-scale or distributed scale. In this study, the model tuned to the Kaskawulsh Glacier was used to obtain a mass-balance—elevation relationship that could be applied to an arbitrary glacier geometry rendered at any resolution. In the original application to the Kaskawulsh Glacier, the NARR 2m air temperatures (30 km posting) were statistically downscaled and bias corrected (as described by Young et al, 2021 and Robinson et al., 2025) to grids of 200m. Figures 1b and 1d show how mass balance and air temperatures are treated in the model. Because we fit functions to both curves to use in the model, the resolution of the original model has only indirect bearing on the derived forcing. We will insert a mention of the resolution as follows:

"The surface mass balance estimates used in this study, produced by Robinson et al. (2025), employ the mass-balance model of Young et al. (2021), which uses an enhanced temperature-index melt model (Hock, 1999) driven by downscaled and bias-corrected North American Regional Reanalysis (NARR) 2-m air-temperature and precipitation data on a 200m grid. Model parameters are tuned for the Kaskawulsh Glacier using the glacier-wide 2007–2018 geodetic balance, the average equilibrium line altitude (ELA) and in-situ mass-balance data. … We then use a second-order spline $S(z)$ (m a^{-1} ; black line in Figure 1b) to describe mass balance as a function of elevation for these zero net-balance conditions, and use this function to drive the simulations described below."

L243: Why was 3 ka chosen?

It was important to run the sims long enough that a thermodynamic steady state was achieved in all cases. This could have been shorter, but it was convenient to choose a round number that was the same for all simulations. We will amend the text to:

"Initialization simulations and sensitivity tests are run for an arbitrary time of 3 ka, long enough for all to achieve a steady state, …"

L256: 'active' rather than 'fast'?

Yes, "active" is better, thanks, and will be adopted.

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

The values of the slip coefficient are empirically determined to achieve flow speeds that represent an acceleration compared to quiescence that is compared to that generally observed. Because we are working within the constraints of a numerical model, the lowest friction coefficients and highest flow speeds are determined by numerical considerations, namely model stability and convergence. We will modify the text to read:

“Surge vigor is varied by using nine logarithmically spaced values of the basal slip coefficient (β^2) ranging from 10^{-3} to 10^{-4} m a^{-1} MPa^{-1}. These values are empirically selected to produce a wide range of sliding speeds subject to the constraints of model convergence and stability.”

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

It doesn’t, but the zone above the threshold elevation represents only a small fraction of the glacier bed (see Figure 1c) that corresponds to the glacier headwall. To clarify this point we will add (also in response to R1):

“We disallow sliding, however, over a small fraction (< 10%) of the glacier bed where z_b > 2400 ma.s.l. (see Figure 1c) in order to inhibit excessive drawdown in the steepest highest region of the accumulation zone.”

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

No. This choice is a numerical one in the original set of experiments where we are investigating the recovery from a single surge. In the next section we use surge-return intervals that are inspired by the field area. We will rephrase the first text as:

“Following the surge, sliding is set to zero during a very long 4 ka quiescent period intended to allow a full post-surge recovery in all experiments.”

In the following section will will revise text to read:

“In the second suite of surging simulations we force 2 a surges with recurrence intervals T_s of 15, 30 and 60 a, typical of the study area, irrespective of whether the glacier geometry has recovered between surges.”

L340: Should there be numbers here?

Yes! Thanks for catching this. It should read:

“Following the surges, all simulations irrespective of surge vigor (i.e., set by the value of β^2) converge to the same configuration, which is slightly ($\sim$ 0.9 %) smaller in volume (V') than the reference glacier (Figure 4a). The recovered values of fraction temperate (FT) and mean enthalpy (H) are 2.8 and 0.7 % larger, respectively (Figure 4b,c).”

The revised manuscript will be amended to reflect these changes.

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

Yes, we will add text to read:

“The recovery timescales shown in Figure 4 should be considered an upper bound, as we do not include crevassing in our model which should shorten the thermodynamic response time by introducing meltwater, and therefore a possible source of latent heat, at depth (Gilbert et al., 2020).”

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

Text will be changed to:

“The dynamic balance line (DBL) separates the upstream reservoir zone from the downstream receiving zone and marks the line of zero ice-thickness change associated with a surge. Its position along the flowline relative to the ELA is illustrative of how this oscillation occurs.”

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

To acknowledge the model limitations on surge propagation, we will extend this sentence to read:

“Observations also show that surges initiate over a limited area of the reservoir zone and can propagate both up and down glacier (e.g. Altena et al., 2019), whereas our model does not include propagation physics outside of what arises naturally in thermomechanically coupled Stokes flow.”

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

Will do! This paper will be cited alongside the other citations to Svalbard thermal structure.

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 ~100 years).

Thanks for the Nanni et al. citation. We looked for a logical place to include this additional role for meltwater and decided on Section 5.1.1 where model limitations related to surface meltwater are discussed. We plan to revise the relevant text to read:

“Additional mass-balance violations occur as a consequence of allowing meltwater that exceeds the prescribed capacity of the firn column to escape. While this water is lost in the model, it may in reality contribute to feedbacks involved in surge initiation and propagation (e.g., Nanni et al., 2025) as well as more dramatic forms of flow instability such as glacier collapse (e.g., Gilbert et al., 2018). Future work would benefit from implementation of a surface process model that includes meltwater percolation in firn (e.g., Gilbert et al., 2012; Gilbert et al., 2020) and attention to the effects of meltwater elsewhere in the glacier, including in crevasses and at the glacier bed (e.g., Nanni et al., 2025).

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

We experimented with the density x-ticks below subpanel b, but unfortunately it resulted in a lot of dead vertical space between subpanel a and subpanel c. Instead we have opted to increase the vertical spacing between rows and insert a horizontal line demarcating the two rows of panels. Between the horizontal line demarcating the rows and the colorcoding of the axis label to match the density data, we hope this minimizes the chance of misinterpretation while keeping as tight a figure layout as possible. Below is the proposed revised figure:

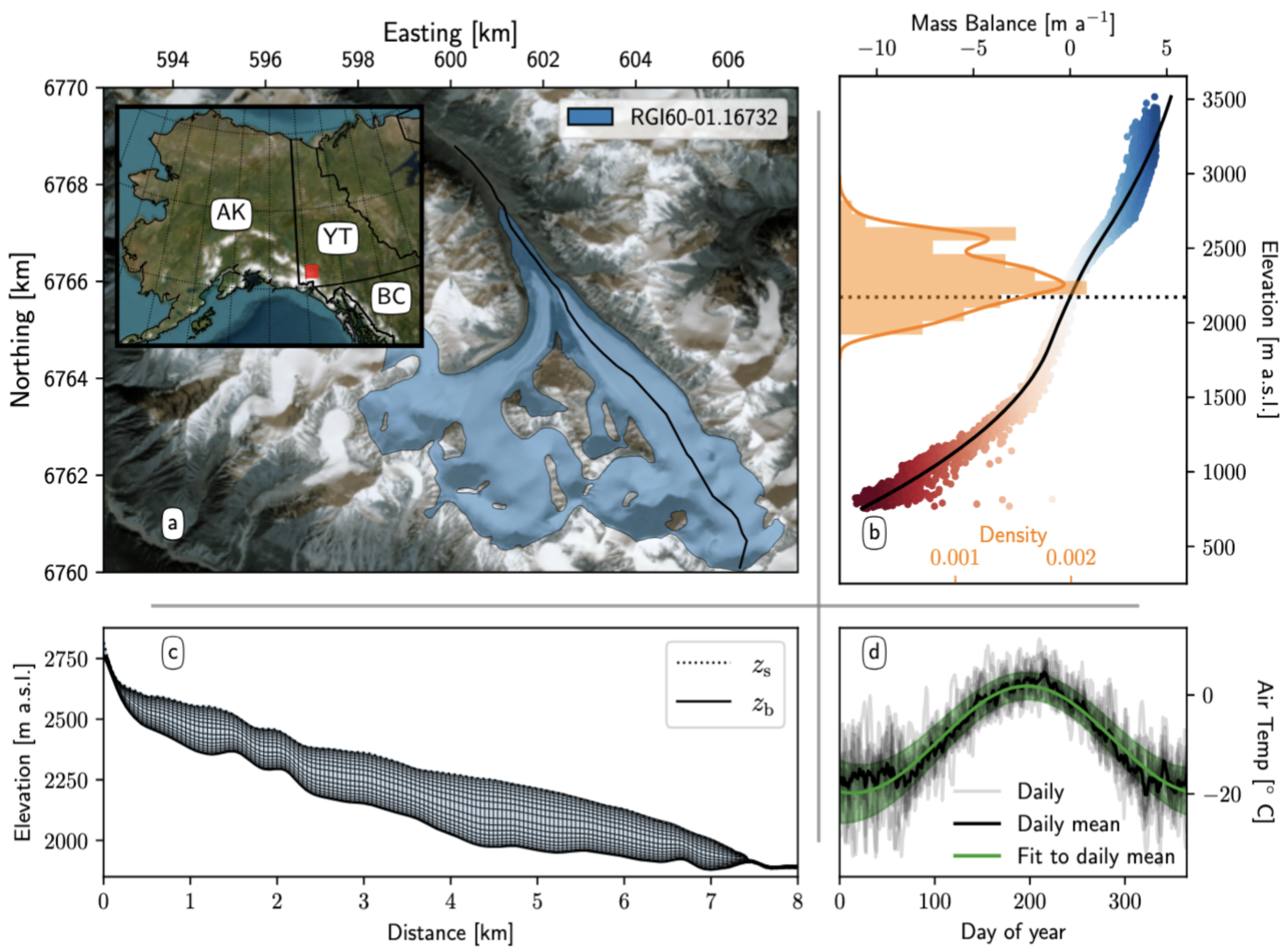
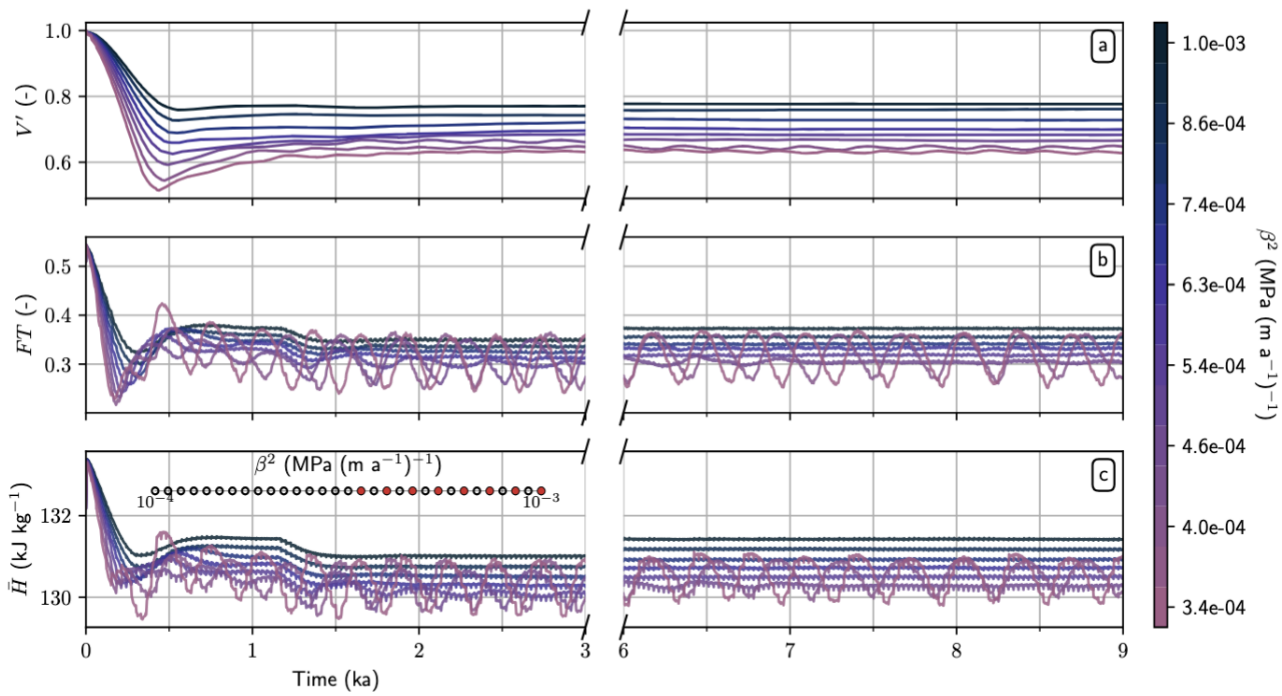


Figure 5: I find it odd that the smallest number is at the top of the colour scale (I understand it is because low basal friction = higher surge propensity) - it may make sense to invert the colourscale?

Thank you for bringing this to our attention. We have inverted the color scale for both Figure 4 and 5 so that the largest numbers are at the top and smallest at the bottom. In addition to being more intuitive this ordering also more closely line ordering within the plots making them easier to interpret. Thank you for the suggestion. Below is the proposed revised Figure 5 (see response to Editor for revised Figure 4):



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Editor initial decision

Jan De Rydt

March 13, 2026

Minor comments:
Line 87 and in a few other places: reference to Table X should presumably be Table 1?

Thank you for bringing this to our attention. We have replaced this placeholder reference to Table 3 on Line 87 and within the caption of Figure 2. We have also replaced the placeholder numerical values on 340 with the appropriate quantities.

Line 306: do you mean \Delta T = +0.5C instead of -0.5C?

Yes. Thank you for bringing that mistake to our attention. It has been fixed in the manuscript.

Line 313: do you mean to refer to Figure S2c instead of Figure 3c?

Yes. Thank you for bringing that mistake to our attention. It has been fixed in the manuscript.

Figure 4: the pink lines are hidden beneath the grid lines and therefore not very visible. Perhaps they could be widened or plotted on top?

Agreed. Thank you for the suggestion. We now have the colored lines plotted over the gridlines, hopefully making them more visible. We have also extended the initial x-axis slightly backwards in time to help make the initial vertical line more visible. Here is the proposed revised Figure 4: