#Review 1

Review of “Compounding sub-seasonal variations in Greenland outlet glacier dynamics revealed by high-resolution observations”

February 10, 2026

General comments

Zhang et al. examine the controls on seasonal velocity change for four outlet glaciers in Central Western Greenland over 2015-2021 using both observations and simplified modelling. Specifically, they assess the role of a change in terminus position on seasonal velocities by calculating the residual between observed and modelled velocities. Their results indicate that seasonal velocity changes are dominated by terminus advance/retreat for two glaciers, whereas the other two glaciers show no response to a change in terminus position. Furthermore, they show that all glaciers experience multiple drivers of velocity change. Hence, the authors suggest that simple categorisations of glacier velocities based on just a single process are incorrect. I believe the science behind this study aligns with the focus of The Cryosphere (TC). However, for each glacier, the authors choose a stress coupling length that minimizes the residual between the observed and modelled velocities, effectively tuning their model to represent the observed velocities well. This is rather problematic, as they use the same residual to determine whether seasonal velocity changes are driven by terminus advance/retreat, especially given that their stress coupling lengths are much larger than, e.g., the independent estimates of Enderlin et al. (2016). As outlined further in my specific comments, Table S1 helps to alleviate some of these concerns, but I strongly suggest the authors repeat their analysis with the Enderlin et al. (2016) (and perhaps the Felikson et al., 2017) stress coupling lengths and discuss the differences. Furthermore, the authors claim that the four glaciers represent the majority of other well-grounded outlet glaciers in Greenland experiencing seasonal change, but given the similarities across all glaciers (same regional climate forcing, same calving style, ...), I am unsure how defensible this is. Finally, critical information is often missing (e.g., Table 1 and Figure 9), and the authors provide no justification/reasoning for some of their design choices (e.g., 2 m winter/summertime threshold). I recommend the authors also take my specific comments listed below into account.

We thank the reviewer for the thorough and constructive comments. We have carefully addressed all the major concerns and specific comments, including the stress coupling length, the missing information in the manuscript, and the justification for the winter/summertime threshold. Detailed responses to each comment are provided below.

Regarding the stress coupling length

We thank the reviewer for prompting us to re-examine our definition. Upon doing so, we realized that our original use of the term "stress coupling length" was inaccurate. In the revised manuscript, we have corrected this: λ is now defined strictly as the upper limit of stress coupling following Joughin et al. (2012), and we no longer equate it with the stress coupling length of Enderlin et al. (2016). The two concepts are physically related but not identical. Accordingly, we have removed the reference to Enderlin et al. (2016) in the revised manuscript to avoid confusion.

Below we briefly clarify the differences between the two concepts, and then, solely for the reviewer's reference, we quantify the stress coupling length of Enderlin et al. (2016) for our glaciers.

Differences between the two concepts

1. **Different mechanisms of upstream influence**

   The upper limit in Joughin et al. describes how far upstream seasonal terminus changes can affect ice flow. In contrast, the stress coupling length (SCL) in Enderlin et al. represents the intrinsic physical scale of stress transmission. The impact of terminus changes can propagate upstream via direct stress coupling (over a few kilometers), and then further upstream through indirect, slower feedbacks—such as changes in surface slope and effective pressure (diffusive processes). Joughin et al. explicitly distinguish these two processes: *"thinning-induced change in basal effective pressure is the dominant process influencing near-terminus behavior, while diffusive processes drive the upstream response."* Thus, the "upper limit of stress coupling" likely reflects a combination of direct stress coupling and subsequent geometric/diffusive adjustments, rather than the pure stress coupling length.

2. **Different methods**

   Joughin et al. use a simplified approach mainly to explain seasonal velocity variations. Enderlin et al. use a more sophisticated method aimed at precisely extracting the spatial scale that governs stress balance.

Quantify stress coupling length based on Enderlin et al. (2016)

We have quantified the stress coupling length using the method from Enderlin et al. (2016), only for the reviewer's reference. Following Enderlin et al. (2016), we computed the stress coupling length for EQP and KUJ based on oscillations in resistive stress. Figures R1 and R2 show the resistive stress profiles for these two glaciers in January, April, July, and October 2019. The mean stress coupling length across the profile is 2.2 km for EQP and 2.4 km for KUJ. The stress coupling length is 3.9 times the ice thickness for EQP and 2.8 times the thickness for KUJ, consistent with the findings of Enderlin et al. (2016). Because these values correspond to a different physical concept, we do not use them directly in our model. We have therefore kept this analysis outside the main manuscript and present it here only in response to the reviewer's comment.

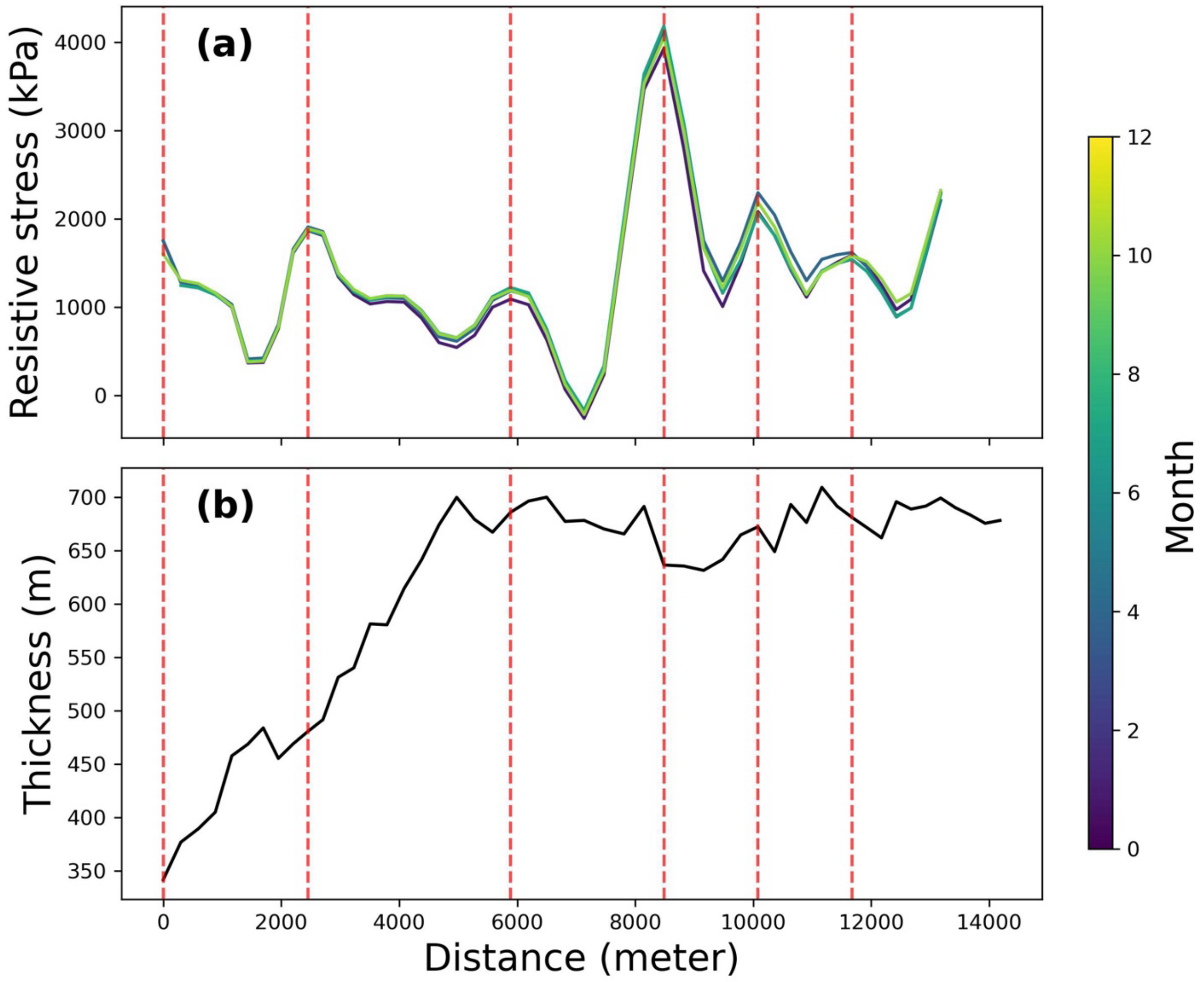

Figure R1. (a) Along-flow resistive stress profiles in 2019 and (b) thickness profile for EQP. The vertical dashed purple lines in (a, b) mark the manually identified local stress maxima following Enderlin et al. (2016).

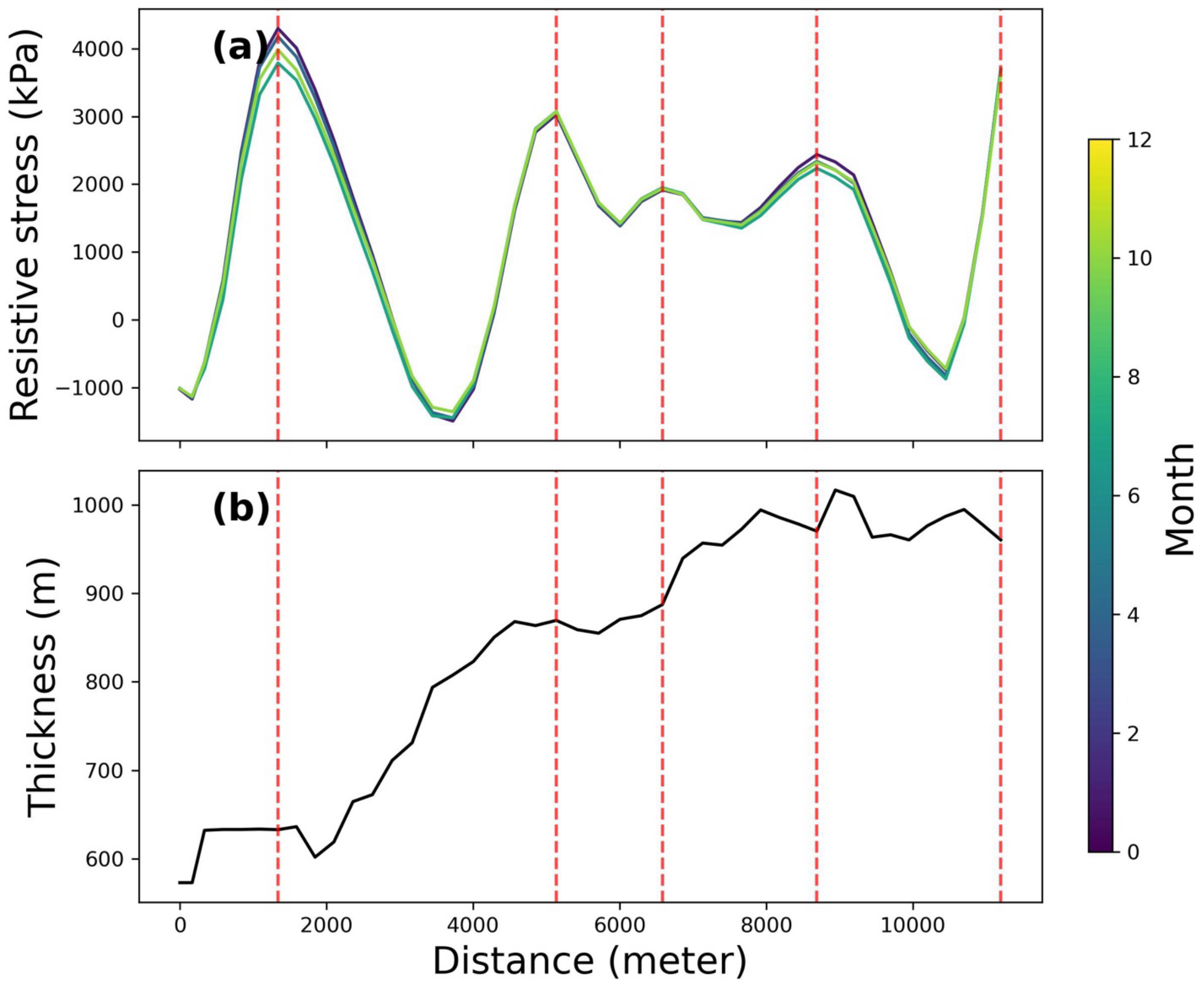

Figure R2. (a) Along-flow resistive stress profiles and (b) thickness profile for KUJ. The vertical dashed purple lines in (a, b) mark the manually identified local stress maxima.

Furthermore, the authors claim that the four glaciers represent the majority of other well-grounded outlet glaciers in Greenland experiencing seasonal change, but given the similarities across all glaciers (same regional climate forcing, same calving style, ...), I am unsure how defensible this is.

We have removed this statement.

The missing information in Table 1 and Figure 9

We have included the missing information in Table 1 and Figure 8 (previously Figure 9). Please see our detailed responses later in this response.

The authors provide no justification/reasoning for some of their design choices (e.g., 2 m winter/summertime threshold).

Thank you for the comment. We have now changed our approach: instead of our original threshold, we use the 1st percentile of each year's runoff as the threshold to define winter/summertime. We have modified the figures and related content as: "*Following Stevens et al. (2016), We define runoff values below the 1st percentile of each year as wintertime, and values above that threshold as summertime.*"

Specific Comments

L4: The sentence structure seems odd. Remove and?

Revised as suggested. Thank you.

L6 – including summertime pulses: What are the other signals?

There two signals in the velocity residuals: (1) summertime pulses and (2) wintertime speed up. We have revised the sentence as: *"Residuals between modeled and observed velocities identify two distinct signals: summertime pulses coincident with peak runoff and a wintertime speedup extending several kilometers inland from the terminus."*

L7 – test the sensitivity of our results: I generally recommend being as precise as possible. Sensitivity to residuals between modelled and observed velocities?

The reviewer is correct that the previous phrasing was ambiguous. We have revised the sentence as: *"We evaluate the sensitivity of terminus-driven velocity to elevation change by incorporating observed seasonally varying surface topography and applying controlled modifications to the profile, specifically uniform vertical shifts and variations in surface slope."*

L13 – GrIS is currently the largest land ice contributor: How much do the four glaciers you are focusing on contribute to this, and was this part of the reason you selected them?

We thank the reviewer for the question. The sentence "GrIS is currently the largest land ice contributor to sea-level rise" was intended only to provide global context for why studying Greenland glaciers in general is important. As originally stated in the "Study region" section, we selected these four glaciers because they: (1) are well-grounded, (2) exhibit regular seasonal changes in terminus position and velocity with minimal long-term trends over our study period, and (3) display a range of sub-seasonal terminus and velocity behavior.

L23 – what exact process: It could be more than one process.

Thank you. We have revised the sentence as: "*but it is not clear what exact processes are involved.*"

L43-44: I am quite surprised that extending the dataset by 221 glaciers resulted in only two distinct types when the previous 45 glaciers were classified into three categories. If anything, I would have expected that the number of distinct types would increase. Were some of the glaciers misclassified in the 2019 study, i.e. were there never three distinct types to begin with?

Thank you for your observation. The difference in the number of glacier types is due to different classification criteria: Vijay et al. (2021) focused only on two glacier types related to drainage networks and excluded the type related to terminus variations, whereas Moon et al. (2014) and Vijay et al., (2019) considered all three types. Thus, the reduction from three to two types therefore reflects a difference in scope, not a misclassification in the earlier study. To avoid any misunderstanding, we have revised the sentence as follows: "*Vijay et al. (2021) extended their previous analysis to encompass 221 glaciers across Greenland; focusing only on type-2 and type-3 behaviors.*"

L61 – high temporal resolution: What exactly do you mean by that? Hourly? Daily? Weekly?

We have revised the sentence as: "*Using high resolution (subweekly) data in this study, we can more accurately decompose these patterns, providing a sub-seasonal characterization of velocity that may be more appropriate for glacier velocity classification.*" The detailed information of temporal resolution is provided in the Table 1.

L70 – well-grounded: I'm not quite sure what exactly well-grounded glaciers are (i.e. what's the threshold between grounded and well-grounded?).

Thank you for your comment. There is no specific threshold between 'grounded' and 'well-grounded'—the latter is used here for emphasis, meaning clearly and unambiguously grounded. To prevent any misunderstanding, we have removed 'well-' and now refer to these glaciers simply as 'grounded.'"

L71 – minimal long-term variations: As part of your motivation, you mention that understanding seasonal drivers may help to understand longer-term changes (L32-36). I am not sure you can address this if you specifically selected a study period with minimal long-term variations.

The mention of long-term changes in lines 32–36 was meant only as general motivation for studying seasonal drivers, not as a claim that our analysis addresses long-term variations. Since investigating long-term changes lies outside the scope of this study, we deliberately selected a period with minimal long-term variations to allow us to focus clearly on seasonal changes.

L72-73: Still unsure what well-grounded glaciers are. Are all of your glaciers marine-terminating? Then what about the seasonal drivers of land-terminating glaciers? How does this affect your results?

All glaciers in this study are marine-terminating. "Grounded" means the glacier front is not floating. Seasonal drivers of land-terminating glaciers are outside the scope of this study.

L74 – all glaciers advance in winter and retreat in summer: How many GrIS glaciers show a different pattern (ie to what percentage of GrIS glaciers does this study apply)?

“All glaciers” here refers to the four glacier we investigated in this study. We have revised the relevant sentence to avoid misleading.

L 76-77 – located close to one another, suggesting that they likely experience the same regional climate forcing: While this helps in isolating the role of different processes for these four glaciers, I think your previous statement these four glaciers likely represent the majority of other well-grounded outlet glaciers in Greenland experiencing seasonal change is then too strong. E.g., are you saying that the distinct local features, such as geometry, will always play the same role in dynamic behaviour, no matter the regional climate forcing? (think non-linear systems)

We agree with the reviewer and have removed the sentence: “*As a result, these four glaciers likely represent the majority of other well-grounded outlet glaciers in Greenland experiencing seasonal change.*”

L84: Having the same primary calving style again limits the applicability of your results to glaciers experiencing a different form of calving.

We used calving style primarily as evidence that these glaciers are grounded. Since floating termini do not transmit the same stress changes to upstream velocity, our focus on grounded glaciers is deliberate.

L88-89: All four glaciers having the same driver of terminus change further limits the applicability of your results to other glaciers.

We chose these glaciers specifically because both their calving style and runoff-driven retreat confirm they are grounded, which is the central condition for our stress analysis. We’ve now acknowledged this scope more clearly in the text.

Fig. 1 and L95-96: How did you select the red and blue points? E.g., why not further down/upstream?

The red and blue points were selected at representative locations along the glacier centerline. The blue points were chosen to test the model sensitivity distance from the terminus. Our current data are sufficient to demonstrate that our results are not an artifact of selecting specific velocity extraction locations, as the model successfully reproduces terminus-driven component of glacier velocity from downstream to upstream.

Table 1: This table could use a lot more details. E.g. is the average velocity the velocity averaged over a season, your time period, the more extensive satellite era? What is the variance or standard deviation? Glacier width, frontal depth, and distance to sampling location at what time? You would assume that a seasonal terminus change of 400 m would at least affect the distance to the sampling location. Is the glacier retreat rate averaged over your whole period or a season? As you are examining multiple years, is the seasonal velocity change the maximum change between seasons across all years or the average? What exactly are the velocity and terminus uncertainty? Why does the terminus uncertainty have units of m/yr? Be consistent with your units (meters vs m). How is the average misfit calculated?

Thank you for your detailed review. We have carefully considered each point and would like to clarify the following:

- Average velocity: This is the mean velocity over the entire study period. We have clarified this in the table.
- Velocity variance: Included in the table.
- Glacier width: Negligible change within terminus variation area; fixed value used. We have clarified in the table caption
- Frontal depth: Averaged over the terminus variation area.
- Distance to sampling location: Averaged among all the terminus positions, with variation now added.
- Glacier retreat rate: Calculated over the study period
- Seasonal change: averaged values across all years. We have clarified in the table caption.
- Uncertainty: Velocity uncertainties are detailed in Gardner et al. (2018); and terminus position uncertainties in Zhang et al. (2023). As these methods are already established, we do not repeat them here.
- The uncertainty of terminus should be meters instead of m/yr. Thanks for pointing that out.
- Units: Standardized to m
- The average misfit is calculated over the study period. We have clarified in the table caption.

L98 – The flowlines: Change to The chosen flowlines

Revised as suggested.

L98 – converge: It doesn’t look like they are actually converging. Maybe concentrate?

Revised as suggested.

L100: Where exactly in the Supplement?

It was described in the section of the Supplement named “Model sensitivity along the flowline”. We have revised the sentence as: “*Blue points in Figure 1 are used to test the model sensitivity to distance from the terminus as described in the Supplementary Information (Section 2)*”.

L101: I assume this is a trailing rolling average then? How do you handle the start of the time series (<40 days)? Why 40 days instead of, e.g., a month?

Yes, it is trailing rolling average. For the first 40 days, the average is still computed using whatever data are available. Since the rolling average is used only for visualization, this minor edge effect is acceptable. We chose 40 days to balance visual smoothness and preservation of seasonal signals.

L104: By filtering out measurements with dt>45 days, don’t you introduce larger data gaps?

Thank you for the question. To clarify, one glacier velocity measurement requires two satellite observations at different times, and dt is the time difference between them. A large dt can reduce data quality, so we apply the dt > 45 day filter. This filter does not introduce large data gaps.

L111: Does, and if yes, how does the higher sampling frequency of your velocity data compared to your terminus position data affect your results?

Yes, the ITS_LIVE velocity data does have a slightly higher sampling frequency than the terminus position data. The temporal resolution of the two datasets is not very different, and both have sufficient resolution to resolve seasonal variations. So, we consider the difference unlikely to impact our conclusions.

L119 – Supplementary Information: You could reference the exact section in the supplement (Sect. 1).

Revised as suggested.

L127 –GSFC-FDMv1.2.1: Would be good to add a few more details on what kind of model this is.

We included more details about GSFC- FDMv1.2.1: “*We use the runoff component in GSFC-FDMv1.2.1, which provide the evolution of surface mass balance and its individual components from January 1, 1980 to June 30, 2022 at 5-day temporal resolution and 12.5 km spatial resolution on North Polar Stereographic Projection.*”

L128: So your results will depend quite heavily on the accuracy of this model.

We agree that the accuracy of the runoff model is important for our investigation. We use runoff time series for two roles:

- To define the timing of the melt season. This helps visually illustrate, in Figures 2–5, how multiple processes influence glacier velocity simultaneously or sequentially within a season, leading to compounding dynamic behaviors.
- To quantify the correlation between runoff and the velocity residual, which supports our finding that runoff can couple with terminus changes to influence glacier speed.

L130: Why the cut-off at 2 m/yr instead of, e.g., 1 or 3 m/yr? Whenever you choose a threshold, you should provide at least some justification/reasoning. Otherwise, it seems somewhat arbitrary.

We have now changed our approach: instead of our original threshold, we use the 1st percentile of each year's runoff as the threshold to define winter/summertime. We have modified the figures and related content as: "*Following Stevens et al. (2016), We define runoff values below the 1st percentile of each year as wintertime, and values above that threshold as summertime.*"

L154: Is a linear decrease upstream of the terminus realistic?

We adopted the same linear parameterization from Joughin et al. (2012), who proposed this as a reasonable ad hoc assumption based on their observation that the seasonal velocity component is maximum near the terminus and nearly negligible upstream. We acknowledge that this is a simplification, but it follows an established approach in the literature.

L177-178: By selecting the stress coupling length that minimizes the difference in velocity, you are effectively tuning your model to match the observations, but the differences might be due to a process not included in your simplified model. I think this is particularly problematic because you are also using the velocity model-data misfit to determine whether seasonal velocity variations are influenced primarily by terminus change. Seeing the relatively small mean relative differences in Table S1 helps to alleviate some of these concerns, but given all the similarities between your four glaciers, I think using the same length scale for all glaciers or an independent method to estimate them (as e.g., Enderlin et al., 2016) would be more defensible.

Thank you for your comment. We have taken two steps to address this, which we outline below.

1. Clarification of what λ represents

We realized that our original use of the term "stress coupling length" was inaccurate. In the revised manuscript, λ is now defined strictly as the upper limit of stress coupling following Joughin et al. (2012), not as the stress coupling length of Enderlin et al. (2016). The two concepts are related but not identical (see our detailed response to your earlier comment).

2. Why tuning λ does not lead to circularity in our seasonal analysis

Adjusting λ only changes the amplitude of the simulated terminus-driven seasonal velocity, not its phase or temporal pattern. The phase and temporal pattern of the model output match the observed velocities well (Figures 2 and 3), and this match does not depend on the specific value of λ. Therefore, even though we tuned λ to minimize the amplitude misfit, our conclusion that the terminus-driven model captures the observations is not based on circular reasoning. We have clarified this in the revised manuscript.

L185: Yes, but only after you already tuned your model.

Yes, the velocity residual is defined only after tuning the model.

L189: Why did you decide not to include the uncertainties from the elevation data?

The elevations uncertainties are not directly included as our model assumes invariant geometry. However, we analytically examine the impact of seasonal variations in surface elevation on velocity simulations, offering a complementary sensitivity test for elevation-related effects. We have revised the related sentences as: "*The elevation uncertainties are not directly included, as our model assumes invariant geometry (following Joughin et al. (2012)). Although the terminus-driven model assumes invariant geometry, we analytically examine the impact of seasonal variations in surface elevation on velocity simulations, offering a complementary sensitivity test for elevation-related effects.*"

L198: Why KUJ and not one of the other glaciers? Do you expect the results to be similar for the other three glaciers?

The coverage of elevations depends on the coverage of its source observations. We chose the April 2019 DEM for KUJ because its elevation coverage best aligned with the most advanced terminus position for that glacier. For the other three glaciers, the October 2019 DEM provided the best coverage. As we demonstrate in the manuscript, seasonal elevation changes have only a limited impact on the velocity changes caused by terminus variations. Therefore, we expect the results to be similar for the other three glaciers.

L206: Can you provide more details on why the classification differs? I think this part of the paragraph belongs in the discussion.

Thank you for the comment. The difference is caused by our use of high temporal resolution observations combined with a simplified physical model, which allows more precise discrimination of mixed (type-1+type-2, type-2+type-3) seasonal behaviors than the approach in Vijay et al. (2021). We have now moved that sentence to the Discussion section and expanded it slightly to clarify the comparison. The revised text is as follows: "*Our combined pattern* *descriptions offer a different perspective from Vijay et al. (2021), which classified EQP and KAN as type-3, identified AVA as type-2 in 2018 and type-3 in 2017 and 2019, and did not classify KUJ. This difference reflects our use of high-temporal-resolution observations together with a simplified physical model, which allows us to identify the presence of multiple coexisting patterns and link them to interacting processes. We emphasize that these different classifications are not contradictory; rather, they arise from different methodological focuses (pattern-based annual classification vs. process-informed sub-seasonal analysis) and together enrich our understanding of glacier seasonal dynamics.*"

L214: They are well described by the terminus-driven model after you have selected the stress coupling length that minimises the misfit. This is circular reasoning.

Adjusting λ only changes the amplitude of the simulated terminus-driven seasonal velocity, not its phase or temporal pattern. The phase and temporal pattern of the model output match the observed velocities well (Figures 2 and 3), and this match does not depend on the specific value of λ. Therefore, even though we tuned λ to minimize the amplitude misfit, our conclusion that the terminus-driven model captures the observations is not based on circular reasoning. We have clarified this in the revised manuscript.

L219 – low residuals: small residuals?

Revised as suggested.

L226 – -0.41 : Fig. 6 shows -0.36 for AVA.

Thanks for pointing that out. We have revised the number.

L229 – 0.35 : Fig. 6 has 0.356, so this should be 0.36.

Thanks for pointing that out. We have revised the number.

L229-231: Move to discussion.

Revised as suggested.

L234: Again, the additional sub-seasonal velocity changes might be more significant than suggested by your results because of the stress coupling length tuning. Your results are basically a best case scenario for terminus-driven changes.

We acknowledge that λ tuning could affect the magnitude of simulated velocity. However, the velocity residual correlates strongly with runoff, and notably, the temporal phase of seasonal variations is independent of λ. Therefore, our conclusion that the seasonal velocity variability at EQP and KUJ is primarily influenced by terminus changes, but also affected by runoff, remains robust.

L236: Move to discussion.

Revised as suggested.

L247 – that plateau: Remove?

We want to express that after the winter acceleration, the velocity remains elevated (plateaus) until just before the following melt season, then early melt season accelerations occur, with the annual maximum velocity reached in the middle of the melt season. So ‘plateau’ should refer to velocity, not acceleration. We have revised the sentence as: “*For KAN and AVA, we observe accelerations during winter (during terminus advance); velocities then plateau before the onset of the melt season in the following year, and early melt season accelerations with the annual maximum velocity reached in the middle of the melt season (Figure 4 and 5).*”

L249 – indicate: indicates. Move to discussion?

Revised as suggested.

L253 – averaged seasonal range: How does this differ from seasonal terminus change in Table 1?

Thank you for pointing that out. The values in Table 1 are correct, and we have updated the numbers here. We appreciate your careful reading.

L259-260: Do you think the insensitivity to spatially uniform along-flow changes in surface elevation could be due to the limitations of your model rather than an actual physical insensitivity?

Thank you for this important point. While model limitations could play a role, our sensitivity tests show that altering surface slope (which affects driving stress) produces a clear velocity response, whereas uniform vertical shifts do not. This suggests the insensitivity is primarily physical under the model's assumptions. That said, we acknowledge that our model is simplified (1-D, assumes linear decay of frontal stress, neglects lateral stresses and basal sliding feedback). A more sophisticated model (e.g., higher-order ice dynamics) might reveal some sensitivity that our analytical approach cannot resolve. We have therefore added a discussion in the Discussion: “*While our experiments indicate physical insensitivity to uniform vertical elevation changes under the assumptions of this model, we cannot rule out that other model physics (e.g., including basal sliding or full stress coupling) might produce different results. Future work with higher-order models could test the robustness of this finding.*”

L261: Figure 9 referenced before Figure 8.

We have reordered the figures so that they now appear in the same sequence as in the text: Figure 9 → Figure 8; Figure 10→ Figure 9; Figure 8→Figure 10.

Fig. 9: All figures should be as self-explanatory as possible. What are the black lines and blue shaded areas in panels a and b? Probably the observations +/- uncertainty, but it is not clear. Do all four lines in panel a overlap? How would a larger change of eg 100 m affect the results in panel c (obviously unrealistic over the course of a few years)? By how much did you steepen/flatten the surface in panel d? You should also mention in the caption which glaciers this is for.

We have included the missing information in the figure caption. Yes, all four lines in panel (a) overlap. The figure caption now reads:

"Figure 8. Experiment results for KUJ using artificially modified surface elevations. Panels (a) and (c) correspond to each other, as do (b) and (d). The blue shaded area represents observation uncertainties. Vertical black lines indicate the onset of the annual glacier retreat, and gray shaded vertical bars denote the melt season. In panel (d), the surface slope is altered by ±2% within the 2 km frontal region."

Figure R3 shows the results of shifting elevation profile by 100 meters and 10 meters. As 100 m change is considered unrealistic. we will not include this scenario in the main text but only provided here for reviewer's reference.

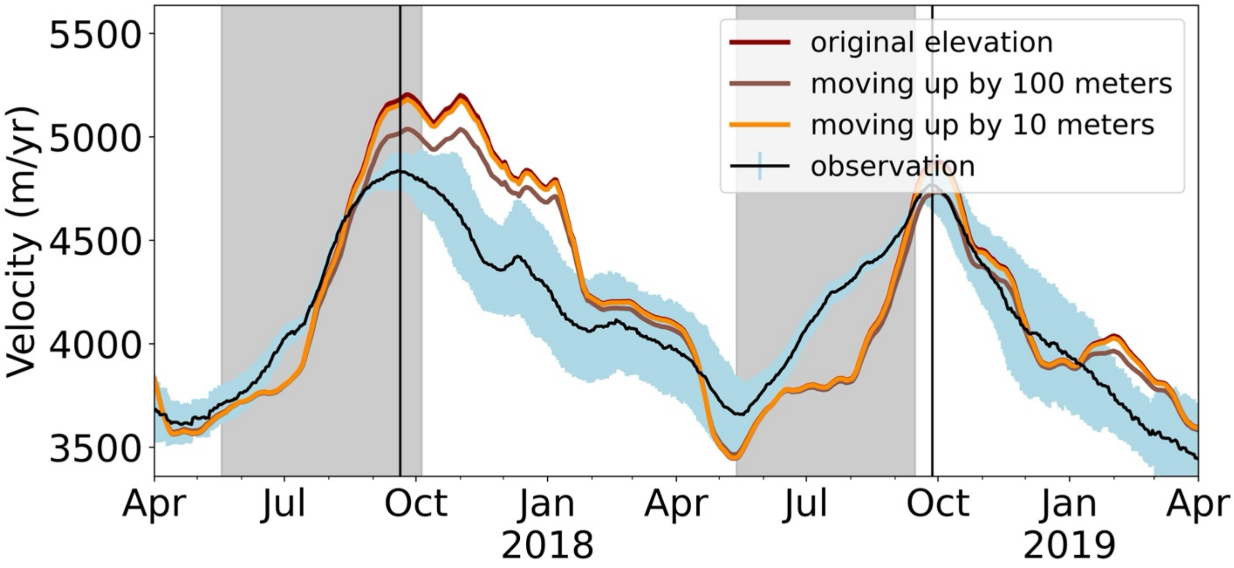

Figure R3. Experiment results using KUJ as an example using the artificially shifted elevation profiles. The vertical black lines mark the onset of annual glacier retreat. The gray shaded vertical bars mark the melt season.

Fig. 10: Remove up from bottom left panel. You should also briefly mention how you retrieved these data. There is no Fig. S8 in the supplement.

Thanks for pointing that out. We have removed “up” from the bottom left panel. The data retrieval is described in the Data section (the end of the first paragraph). We have corrected supplementary figure reference from Figure S8 to Figure S7 in the caption.

L326-329: Does your chosen stress coupling length depend on the position you extract the velocity data to calculate the misfit (e.g., further upstream)? How do your results change if you use the lengths of Enderlin et al. (2016)?

Yes, λ depends on the velocity extraction position. For the comparison with Enderlin et al. (2016), please see our previous response. We have removed that reference from the manuscript to avoid confusion.

L343: It would be really interesting to know how this simplified model compares to numerical models. This is obviously outside the scope of this study, but I’m wondering if others have done a comparison with a similar model that could be mentioned here.

We agree that such a comparison would be insightful but is beyond the scope of this study. Following the reviewer's suggestion, we have added a brief mention of this in the Limitations and Outlook section as a direction for future work.

Reference

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