#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.

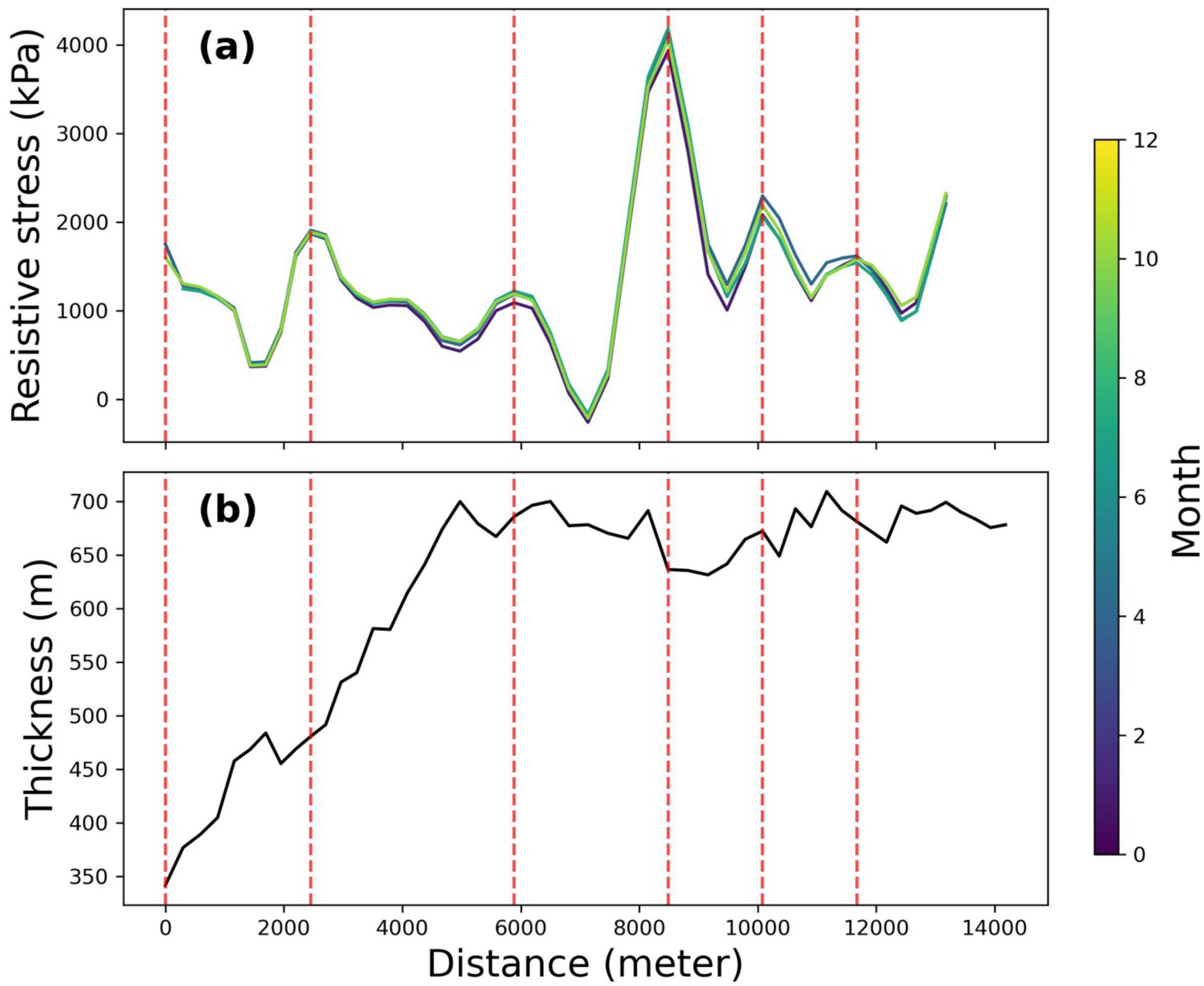

Figure R1. (a) Along-flow resistive stress profiles in 2019 and (b) thickness profile for EQP. The vertical dashed purple lines in (a, b) mark the manually identified local stress maxima following Enderlin et al. (2016).

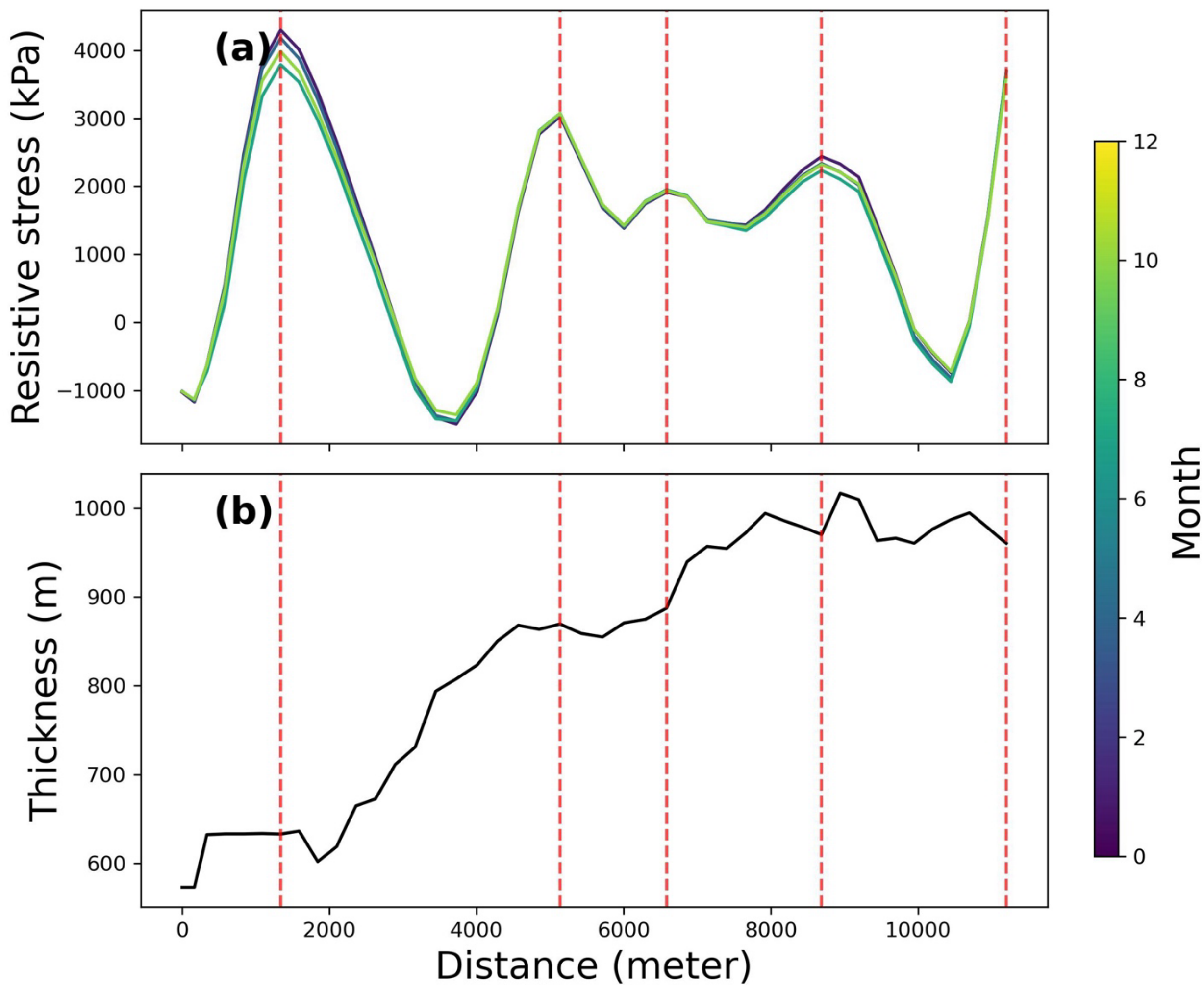

Figure R2. (a) Along-flow resistive stress profiles and (b) thickness profile for KUJ. The vertical dashed purple lines in (a, b) mark the manually identified local stress maxima.

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.

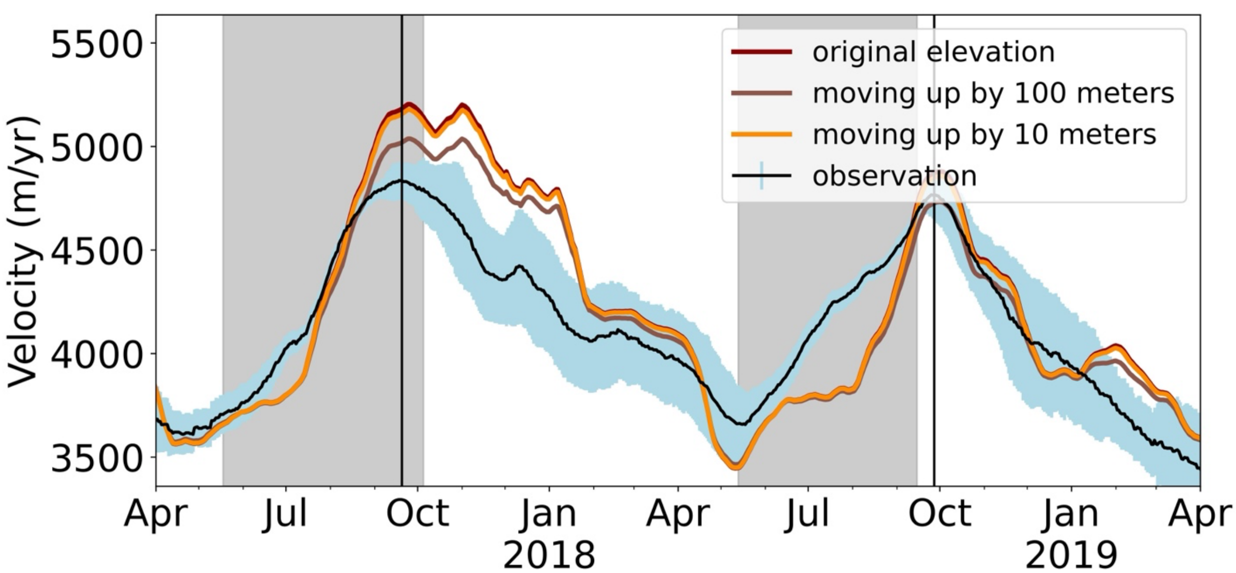

Figure R3. Experiment results using KUJ as an example using the artificially shifted elevation profiles. The vertical black lines mark the onset of annual glacier retreat. The gray shaded vertical bars mark the melt season.

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

Joughin, I., Smith, B. E., Howat, I. M., Floricioiu, D., Alley, R. B., Truffer, M., and Fahnestock, M.: Seasonal to decadal scale variations in the surface velocity of Jakobshavn Isbrae, Greenland: Observation and model-based analysis, Journal of Geophysical Research: Earth Surface, 117, F02 030, https://doi.org/10.1029/2011JF002110, 2012.

Enderlin, E. M., Hamilton, G. S., O’Neel, S., Bartholomaus, T. C., Morlighem, M., and Holt, J. W.: An empirical approach for estimating stress-coupling lengths for marine-terminating glaciers, Frontiers in Earth Science, 4, 104, https://doi.org/10.3389/feart.2016.00104, 2016.

Moon, T., Joughin, I., Smith, B., van den Broeke, M. R., van de Berg, W. J., Noël, B., and Usher, M.: Distinct patterns of seasonal Greenland glacier velocity, Geophysical Research Letters, 41, 7209–7216, https://doi.org/10.1002/2014GL061836, 2014.

Vijay, S., King, M. D., Howat, I. M., Solgaard, A. M., Khan, S. A., and Noël, B.: Greenland ice-sheet wide glacier classification based on two distinct seasonal ice velocity behaviors, Journal of Glaciology, 67, 1241–1248, https://doi.org/10.1017/jog.2021.89, 2021.

Zhang, E., Catania, G., and Trugman, D. T.: AutoTerm: an automated pipeline for glacier terminus extraction using machine learning and a "big data" repository of Greenland glacier termini, The Cryosphere, 17, 3485–3503, https://doi.org/10.5194/tc-17-3485-2023, 2023.

Gardner, A. S., Moholdt, G., Scambos, T., Fahnstock, M., Ligtenberg, S., van den Broeke, M., and Nilsson, J.: Increased West Antarctic and unchanged East Antarctic ice discharge over the last 7 years, The Cryosphere, 12, 521–547, https://doi.org/10.5194/tc-12-521-2018, 2018.

#Reviewer 2

Summary

The study examines the influence of seasonal terminus change on glacier velocity changes at four marine-terminating glaciers in Central West Greenland using a novel 1D model. The results show that the glacier velocities are driven by a multitude of drivers with some being dominated by terminus change, and others being dominated by wintertime acceleration. The terminus-driven model introduced in the study provides a new and relatively simple way to investigate the influence of terminus change on near-terminus glacier velocities, which can be applied to most marine-terminating glaciers in Greenland. I commence the authors on a well-conceived and executed study. The manuscript is well written and presented, so I have few minor comments which are outlined below.

We thank the reviewer for the thoughtful and encouraging summary of our work. We appreciate the positive assessment of our study's conception, execution, and presentation, as well as the constructive minor comments. We have carefully addressed each of the detailed comments as indicated above, and we believe the revised manuscript has been strengthened as a result.

Detailed comments:

Line 78: Reference is in all caps – needs reformatting

Revised as suggested. Thank you.

Figure 1 caption: You say the terminus positions are coloured by data, but they are coloured by day of year. Consider rephrasing

Revised as suggested.

Line 92 – 94: I was wondering why you chose to use ITS_LIVE velocities given that earlier you say you selected the specific study period as Sentinel-1 data became available with a higher resolution. Would it then not make more sense to use PROMICE v5 data which solely relies on Sentinel-1 data and has a consistent 11-day temporal resolution from 2016 to 2025?

Thank you for the comment. We selected ITS_LIVE because it integrates Sentinel-1, Sentinel-2, and Landsat-8, offering higher effective temporal resolution than PROMICE v5's 11-day product. Moreover, our terminus positions derive from AutoTerm, which also uses the same multi-sensor suite; thus ITS_LIVE ensures better consistency between the velocity and terminus datasets.

Line 111 – 113: There are some standard methods to quantify terminus change (e.g. curvilinear box method) and I was wondering why you chose to not use them. Consider adding a sentence or two on how your method differs from those.

Thank you for the suggestion. Our area-based metrics account for both terminus shape and fjord geometry, and unlike the curvilinear box method, they do not require a predefined box to represent the fjord geometry. We have added this information in the main text.

Line 117 – 118: While it is a nice addition to use the novel DG-IS2_DEM, I am wondering why you don't include any elevation change prior to 2018. I am aware that this would mean using ArcticDEM prior to 2018, but it seems odd to only have elevation data for half of the study period.

The DG-IS2_DEM product is based on ICESat-2 data, which became available only after the satellite's launch in 2018. Our primary objective in incorporating this dataset was to examine seasonal elevation changes, and although it does not span the entire study period, it covers two full seasonal cycles, providing meaningful insight into seasonal variability.

Line 189: Why do you not include uncertainties from elevation data? Consider adding a sentence here to explain why.

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.*"

Line 236: behaivor -> behaviour.

Revised as suggested.

Figure 2 caption: with light yellow shading shows uncertainty – change "shows" to "showing". Also, you state that the grey bars show when runoff is great 2m/yr – should runoff not be in cubic meters?

We have changed "shows" to "showing". The runoff represents per-unit-area values, so the unit is m/yr. We have added a brief clarification in the caption to avoid confusion.

Line 249: indicate -> indicates

Revised as suggested.

Figure 4 panel b: Total elevation change should be a black line to conform with previous figures.

Revised as suggested.

Line 274: increase -> increases

Revised as suggested.

Line 304 – 305: […] the ratio of velocity range between upstream and frontal region is 13% in summer […] - Insert “the” before “upstream”.

Revised as suggested.

#Reviewer 3

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

The study uses an existing 1D terminus force balance model to examine the seasonal influence of terminus advance/retreat on glacier velocity, combines this with observed velocities to determine the extent to which other factors (namely, runoff and meltwater) influence velocity, and tests how surface elevation and slope changes influence the results. The methods are applied at four neighboring marine-terminating, grounded western Greenland glaciers to examine 2015-2021 behavior. The results show differing influences of terminus and hydrologic changes at the four glaciers, including shifting influences over time. The results provide new details about the behavior of these four glaciers and demonstrate the usefulness of investigating glacier behavior using a simplified model and relatively high-resolution data in both space and time.

Dear Dr. Twila Moon, we sincerely thank you for your encouraging and constructive review. Your 2014 paper laid the foundation for this study, and we greatly appreciate your guidance in improving this manuscript, particularly regarding the accurate interpretation of the type 1–3 classifications. We have revised the text to avoid misrepresenting previous research and to better position our study as an extension to existing knowledge. All specific technical comments have been carefully addressed. We are truly grateful for your recognition of our study as “a compact and complete exploration” and “a useful addition” to the field. Your insights have made this a more accurate manuscript. Thank you once again for your time, expertise, and thoughtful mentorship.

General comments

- Overall, the paper provides a compact and complete exploration of the research undertaken. Figures and Supplementary Materials are appropriate, as are the methods. The study is well described and provides a useful addition to the research on Greenland Ice Sheet seasonal glacier behavior.

  We sincerely thank you for your encouraging and constructive assessment of our work. We are glad that the study is considered well described, with appropriate methods and materials, and that it contributes usefully to the understanding of Greenland Ice Sheet seasonal glacier behavior. We greatly appreciate the reviewer's time and encouragement.

- I have an important general comment re: tone and discussion of previous research. The authors suggest that one of the conclusions is that "simple categorization of glacier velocities as the result of a single process are not correct". I agree, but I also think the authors have misrepresented the previous research by saying that earlier research suggested that single processes are used to classify glacier velocity. This is not true in any of the previous research cited (Solgaard et al. 2022, for example, includes some nice discussion on this matter). Application of the type 1-3 categories focuses on velocity patterns, which have since the inception of the concept been known to connect to multiple processes. Velocity patterns are then combined with other data to examine dominate processes in just the same way as these authors examine dominant/non-dominant processes. So, their conclusions here align with previous research rather than suggesting a needed change. Attention to this matter is also important to ensure that the meanings of "type 1" and so on remains consistent across research papers. I'm concerned that the authors here begin to use these classifications to designate dominant processes rather than to describe velocity patterns (see lines 203-206). Be careful to avoid this and revise as needed.

Thank you for this thoughtful and constructive comment. We fully agree with your assessment. We realize that we misrepresented previous research by suggesting that earlier studies attributed glacier velocity patterns to single processes. In fact, as you note, the Type 1–3 classification has always been about velocity patterns rather than dominant processes, and previous work (e.g., Solgaard et al. 2022) already discussed multiple processes.

To address this, we have revised the manuscript throughout. Specifically, we now describe the Moon et al. (2014) classification as reflecting velocity patterns, not dominant processes. We have also corrected the wording in lines 203–206 and elsewhere to avoid implying that these types designate processes. Our findings align with and extend the existing understanding that glacier velocity patterns reflect compounding processes.

Thank you again for helping us improve the clarity and accuracy of our paper.

- It is unclear to me why the simulation for AVA is so bad. The authors say that there is no correlation between terminus and velocity (line 256, with no other explanation) and no seasonality to simulated velocities because of small terminus changes (line 252), though the magnitude of terminus change across the record (range of ~300 m total) is comparable to EQP. The AVA simulation is simply wildly off from even the mean velocity, which is not true for any of the other glaciers. What is going on here? It really looks like something is wrong with the AVA simulation, so an explanation is very needed.

  The main reason is that the surface elevation in the terminus variation region of AVA has a very low slope. As shown in Figure R3 (surface elevation in 2019, with terminus variation areas indicated by vertical blue dashed lines), the slope of AVA is much gentler than for the other three glaciers. Under such low topographic gradient, terminus changes produce only a small change in the frontal force (F). Additionally, the seasonal terminus variations of AVA are indeed small. For reference, the averaged seasonal range in terminus position is 144 meters for AVA, while EQP is 224 meters, KAN is 390 meters, and KUJ is 417 meters. Combining the small seasonal range with the flat frontal topography, there is no significant seasonality in the simulated velocity. We have included Figure R1 into the supplementary material.

We will revise the text to include these points more explicitly. A suggested revised sentence is: "*For AVA, the simulated velocity lacks significant seasonality because (1) the seasonal terminus variations are small (Figure 5), and (2) the frontal region has a very low surface slope (Figure S6). Together, these two reasons lead to small change in frontal force F (Eqn. 2), and thus little seasonality in simulated velocity.*"

To clarify, the terminus variation range shown in the Figure R1 (flowline-based change) and the values reported in Table 1 / the main text (based on sequential area changes between termini, normalized by glacier width) are not directly comparable.

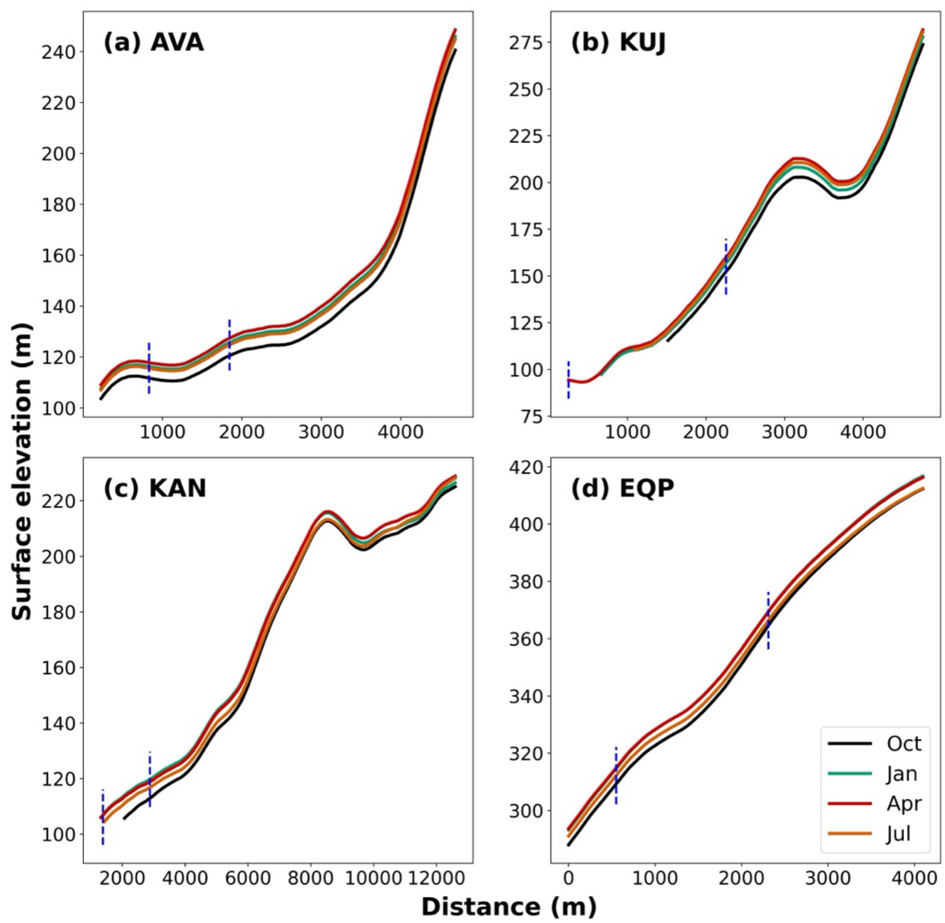

Figure R1. Surface elevation profiles in 2019 for AVA, KUJ, KAN, and EQP. The vertical blue line denotes the extent of terminus variations.

- In the final print layout, I hope the figures can be moved so that they are as close as possible to where they are first referenced. No problem to have several pages of figures in a row, rather than string them out. I suspect this is a layout matter for The Cryosphere team.

We appreciate the reviewer's suggestion. We will work with The Cryosphere team to position figures near their first citation when the manuscript reaches the final proof stage.

Specific and technical comments (by line number)

4. remove "and"

Revised as suggested.

14. Accelerating ice loss is a result of discharge and surface mass balance. Recommend "in part" instead of "largely" and consider including more recent citation(s).

Revised as suggested.

53. "these studies classified glacier behavior within a year as a single category" This is false. For example, Moon et al. (2014) included multiple "types" in some years and Poinar et al. (2023) discuss multiple patterns (from EOF/PC analysis) being present in an individual glacier. I appreciate that the authors need to clearly distinguish their work from previous efforts, but those previous efforts must be accurately summarized. Also, by engaging with the details of previous work on the four focus glaciers, the authors can achieve a richer discussion of their own results and focus on the most meaningful conclusions.

Thank you for your insightful comments. They have helped us significantly improve the accuracy and clarity of our manuscript. We apologize for the inaccurate statement in our original manuscript. We have now carefully revised the text to correctly represent the previous work. Specifically, we have revised the last two paragraphs in the Introduction. The main changes include:

(1) Removed the claim that previous studies "classified glacier behavior within a year as a single category."
(2) Explicitly acknowledged that Moon et al. (2014) reported multiple velocity types occurring within a single year for some glaciers, and that Poinar et al. (2023) quantified the coexistence of multiple seasonal patterns at individual glaciers.
(3) Reframed our scientific distinction not as a correction of a factual error, but as a different focus: previous studies primarily described dominant or statistically separable patterns, often limited by temporal resolution (e.g., monthly or bi-monthly data), whereas our study uses sub-weekly data and a physics-based model to examine the sequential occurrence and compounding effects of multiple processes within a single season. This more accurately positions our contribution without misrepresenting earlier work.

We have compared our classification with Vijay et al. (2021). Our classification is described at the beginning of the Results section: "*Following the velocity-pattern classification of Moon et al.*

(2014), we describe each glacier's seasonal behavior as a combination of patterns: EQP and KUJ exhibit patterns consistent with type-1 plus type-2, whereas KAN and AVA exhibit patterns consistent with type-2 plus type-3." The comparison with the Vijay et al. (2021) is described in the Discussion: "*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.*"

Section 2. I think it would be helpful to include in the study region description a note about all flowline samples being within the ablation zone, as a matter of commenting on expected spatial influence of meltwater. This is mentioned re: EQP at line 295, but I think it's worth also clarifying early for all glaciers.

We thank the reviewer for the suggestion. We have added a brief note in Section 2: "*All flowline samples for these glaciers are located within the ablation zone (Noël et al., 2019), implying that meltwater influence is expected across all sampled sites.*"

101. change "was" to "were" for "data..."

Revised as suggested.

188-189. Can you provide a rough guide re: anticipated influence if you were to also include elevation data uncertainties? Help the reader to understand if those are anticipated as small/large compared to presented uncertainties (from terminus data only), highly variable, etc.

The elevations uncertainties are not directly included as our model assumes invariant geometry. However, we analytically examine the impact of 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.*"

Our analysis shows that a 2% absolute change in slope leads to a 2% change in average velocity and a 6% change in peak velocity (Section 5.3). Using a horizontal distance of 1000 m, an elevation uncertainty of 4 m translates into a 0.4% slope change (4 m/1000 m = 0.4%). Following the same linear sensitivity, this would cause approximately 0.4% uncertainty in simulated velocity. For KUJ, 0.4% of the mean velocity (3699 m/yr) equals 15 m/yr, which is smaller than the uncertainty attributed to terminus data alone (62 m/yr).

194. remove "in order"

Revised as suggested.

200. Why +/–2% for surface slope testing? I note that Anonymous Referee #1 asks for more explicit mention of why different assumptions are made. While I don't agree with this referee on all fronts, I do agree that some changes in this regard are helpful.

Thank you for raising this. Regarding why we use ±2% for the slope testing: we initially found that including seasonally varying elevation had a negligible effect on the simulated velocity. Therefore, we designed two analytical experiments, one with spatially uniform along-flow changes and one with surface slope changes, to identify which type of elevation change actually influences the impact of terminus variations on glacier velocity.

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.*"

Figure 8. I suspect that the surface profile lengths shown for AVA, KUJ, and EQP are due to data coverage from the DG-IS2-DEM input. However, from Figure 1, I expected longer profiles for subsequent data. Consider adding DEM borders in Figure 1 or clarifying in Figure 8 caption.

Thank you for raising this. We have revise the figure caption as: "*Surface elevation difference profiles for AVA, KUJ, KAN, and EQP in 2019 relative to the October profile. Profile lengths are limited by the overlapping area of the DG-IS2-DEM.*"

303. change "is" to "are" and edit the rest of the sentence accordingly

Revised as suggested.

309. change "any" to "notable" (terminus influence is likely >0)

Revised as suggested.

313-315. Useful to note what is meant by "upstream" - info can be included in parentheses.

We have revised as: "*For KAN, the range in upstream (farther from the terminus*)….". We also revised a sentence in the caption of Figure 9 (previously Figure 10): "*The shaded areas indicate regions where we obtain velocity variations in the frontal (left) and upstream (right) sections*."

Figure 10. Option to shorten the caption by combining explanatory information for panels (a, c, e, g) and (g, d, f, h).

Figure 10 is now Figure 9. We have revised the caption as: "*Velocity profiles over time for EQP, KUJ, KAN, and AVA. The average velocity profile has been subtracted for a better display of changes over time. The original velocity profiles are shown in Figure S7. The shaded areas indicate regions where we obtain velocity variations in the frontal (left) and upstream (right) sections. Left column (a, c, e, g): Melt-season profiles for EQP (a), KUJ (c), KAN (e), and AVA (g). Right column (b, d, f, h): Post-melt-season profiles for EQP (b), KUJ (d), KAN (f), and AVA (h), when velocity is primarily influenced by terminus changes.*"

335-336. I found this sentence confused. Please revise for clarity.

We have revised the sentences as: "We find that the seasonal elevation changes for EQP, KAN, KUJ, and AVA show limited spatial variation along the flow (Figure 10). Quantitatively, for each profile, the standard deviation along the profile (i.e., spatial variation with distance) averages only 22% of the profile's mean value. This low ratio indicates that the profiles are relatively uniform along flow."

348. I think there's a strong case that terminus change and meltwater are the two big factors to consider (I'm co-author on a submitted paper suggesting mélange cannot buttress calving in Greenland, for example), so I think it's fine to mention sea ice/mélange and air temperature, but I wouldn't consider them particularly "important" (line 344) processes and I think you can reduce emphasis on potential impact from either if you like.

Thank you for this constructive comment. We agree that terminus changes and meltwater are the primary factors. Accordingly, we have removed ice mélange and de-emphasize air temperature as a secondary factor.

Supplement

23. I don't understand what "...interpolation into the corresponding model component" means.

We have revised the sentence to clarify that the coarse-resolution (400 m) elevation-change field is linearly interpolated onto the fine (50 m) DEM grid before being summed with the DEM to produce the model elevations. The revised sentence now reads: *Model elevations are calculated by linearly interpolating the elevation-change field onto the DEM grid and summing them.*

49-50. Moving the parentheses with the scaling parameter notations to right after "scaling parameters" will make it clearer what they are. Given the confusing use of "component" above, I was further confused to find these listed after "component" in this sentence. At this point, "component" is confusing to me.

Thank you for raising this. We have moved the parentheses with the scaling parameter notations to right after the mention of "scaling parameters". We have revised the "*the importance of each component*" as "*the importance of each component in model elevations (z_m)*"

63-65. This sentence is hard to follow. Edit for clarity.

We have revised the sentence as: "*For each tile, we construct two sets of equations: (i) those describing linear interpolation between the elevation grids and the data values (Equation 1), and (ii) the constraint equations (Equations 4–6). We then apply a QR-transform to the matrix of coefficients of this combined system and solve for the optimal grid-elevation and bias values*."

73. Edit to "mid-2019", "mid-2022", and remove "on"

Revised as suggested.

75. Edit to simply "DEM slope"

Revised as suggested.

83. remove extra "the"

Revised as suggested.

92. recommend removing "occasionally"

Revised as suggested.

94-95. Can remove both instances of "further"

Revised as suggested.

98. Remove ","

Revised as suggested.

101-102. Please list all corresponding stress coupling lengths for easier reference, then go on to specify and talk about EQP and KUJ. Also, "might be uncertain" is strange phrasing. Can you instead say roughly "We assume a constant stress couple length along the profile and use a simplified 1-D terminus-driven model, which may influence our estimations [in this way]." [Given other reviewer comments re: different stress coupling lengths, you could consider moving more of this to main text.]

KAN and AVA's seasonal velocities are not primarily driven by terminus variation, so we do not consider the estimation of λ (previously defined as stress coupling length) valid for these glaciers. We have included this reason here.

We have now defined λ as the upper limit of stress coupling following Joughin et al. (2012), on which our work is based, rather than as the "stress coupling length" following Enderlin et al. (2016). Please refer to our response to Reviewer #1 for further details.

We have revised the sentence as suggested. Thank you. We have moved this into the main text.

Figure S1. Please move to the end of Section 1 so that it's easy to read the description and see the figure at the same time.

Revised as suggested.

Figures S2-S5. Some additional info needed in captions re: grey shading, observation uncertainty and black vertical lines (only included in S3 and S5).

Thank you for raising this. We have included the missing information in the figure captions.

Reference

Moon, T., Joughin, I., Smith, B., van den Broeke, M. R., van de Berg, W. J., Noël, B., and Usher, M.: Distinct patterns of seasonal Greenland glacier velocity, Geophysical Research Letters, 41, 7209–7216, https://doi.org/10.1002/2014GL061836, 2014.

Solgaard, A. M., Rapp, D., Noël, B. P. Y., and Hvidberg, C. S.: Seasonal Patterns of Greenland Ice Velocity From Sentinel-1 SAR Data Linked to Runoff, Geophysical Research Letters, 49, e2022GL100 343, https://doi.org/https://doi.org/10.1029/2022GL100343, 2022.

Vijay, S., King, M. D., Howat, I. M., Solgaard, A. M., Khan, S. A., and Noël, B.: Greenland ice-sheet wide glacier classification based on two distinct seasonal ice velocity behaviors, Journal of Glaciology, 67, 1241–1248, https://doi.org/10.1017/jog.2021.89, 2021.

Poinar, K.: Seasonal Flow Types of Glaciers in Sermilik Fjord, Greenland, Over 2016–2021, Journal of Geophysical Research: Earth Surface, 128, e2022JF006 901, https://doi.org/https://doi.org/10.1029/2022JF006901, 2023.

Joughin, I., Smith, B. E., Howat, I. M., Floricioiu, D., Alley, R. B., Truffer, M., and Fahnestock, M.: Seasonal to decadal scale variations in the surface velocity of Jakobshavn Isbrae, Greenland: Observation and model-based analysis, Journal of Geophysical Research: Earth Surface, 117, F02 030, https://doi.org/10.1029/2011JF002110, 2012.

Enderlin, E. M., Hamilton, G. S., O'Neel, S., Bartholomaus, T. C., Morlighem, M., and Holt, J. W.: An empirical approach for estimating stress-coupling lengths for marine-terminating glaciers, Frontiers in Earth Science, 4, 104, https://doi.org/10.3389/feart.2016.00104, 2016.