#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.

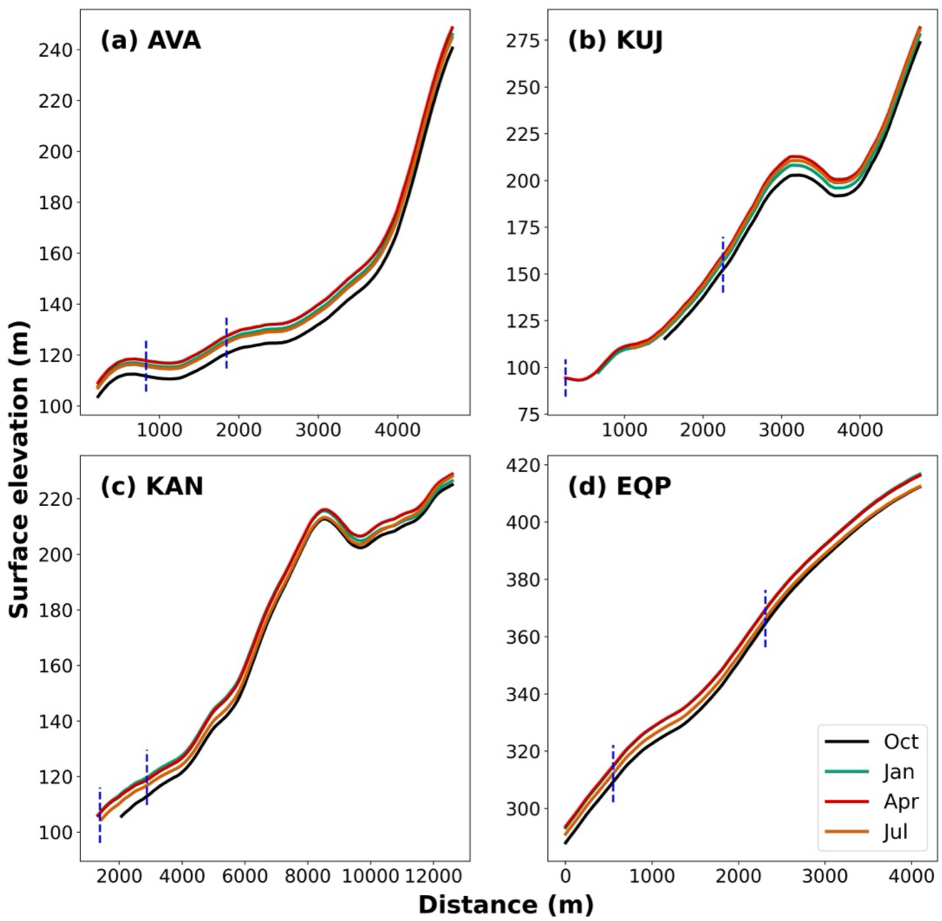

Figure R1. Surface elevation profiles in 2019 for AVA, KUJ, KAN, and EQP. The vertical blue line denotes the extent of terminus variations.

- 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.

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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.