

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

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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 minimises 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/summer time threshold). I recommend the authors also take my specific comments listed below into account.

Specific comments

L4: The sentence structure seems odd. Remove *and*?

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

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

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?

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

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?

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

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?).

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.

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?

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)?

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)

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

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

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

L98 – *The flowlines*: Change to *The chosen flowlines*

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

L100: Where exactly in the Supplement?

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?

L104: By filtering out measurements with $dt > 45$ days, don't you introduce larger 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?

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

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

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

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.

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

L177-178: By selecting the stress coupling length that minimises 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.

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

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

L198: Why KUJ and not one of the other glaciers? Do you 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.

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.

L219 – *low residuals*: small residuals?

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

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

L229-231: Move to discussion.

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.

L236: Move to discussion.

L247 – *that plateau*: Remove?

L249 – *indicate*: indicates. Move to discussion?

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

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?

L261: Figure 9 referenced before Figure 8.

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 \pm 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.

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.

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)?

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.

I hope the authors find my comments helpful.

References

- 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*, Volume 4 - 2016, <https://doi.org/10.3389/feart.2016.00104>, 2016.
- Felikson, D., Bartholomaus, T. C., Catania, G. A., Korsgaard, N. J., Kjær, K. H., Morlighem, M., Noël, B., van den Broeke, M., Stearns, L. A., Shroyer, E. L., Sutherland, D. A., and Nash, J. D.: Inland thinning on the Greenland ice sheet controlled by outlet glacier geometry, *Nature Geoscience*, 10, 366–369, <https://doi.org/10.1038/ngeo2934>, 2017.