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.

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.
- 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.
- 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.

- 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.

Specific and technical comments (by line number)

4. remove “and”

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).

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.

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.

101. change “was” to “were” for “data…”

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.

194. remove “in order”

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.

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.

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

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

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

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

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

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.

Supplement

23. I don't understand what "…interpolation into the <u>corresponding model component</u>" means.

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.

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

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

75. Edit to simply "DEM slope"

83. remove extra "the"

92. recommend removing "occasionally"

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

98. remove ","

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 couple lengths, you could consider moving more of this to 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.

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