Response to Lizz Ultee

Carnahan, Catania Bartholomaus present a stress-balance analysis of three neighboring outlet glaciers in Greenland: Ingia Isbrae, Umiamako Isbrae, and Rink Isbrae. Despite their proximity, the three outlets showed a variety of retreat histories in recent decades, making them a useful site for comparative analysis. The study is interesting and relevant, certainly worth publishing in The Cryosphere. In some places, I suggest revising to make the text more precise. The discussion should also be strengthened, especially with regard to the Greenland-wide implications of the work. Finally, I suggest the authors take another editing pass through the text to simplify the language.

Thank you for taking the time to review our paper, understanding the arguments made, and making comments that will be helpful for contextualizing our results. This review largely focused on a few of the more sweeping claims we made in the discussion and some unclear statements throughout. We have cleaned up the language in the text and have spent time qualifying and specifying the two broad claims made in the discussion. We now feel that the discussion is supported by our results. This review helped us to present the potential broader implications of our results in a rigorous way. Thank you.

Specific comments

L58: would it be accurate to say simply "no secular trend emerges from seasonal fluctuations"?

Yes, thank you.

L62-65: Please revise here to describe why the force balance method is suitable for your study. Stating which studies have used it before does not provide your scientific motivation for using it here.

We have now added the motivation for this method, i.e., this method provides snapshots in time of the glacier stress state and is therefore useful to examine how the stress state varies in time during changes in glacier terminus position.

L84: I would not cite the Minchew et al. 2019 comment as evidence of "uncertainty" in the basal sliding relation. Minchew et al. 2019 do not advocate for or provide evidence supporting any sliding relation different from the one tested by Stearns and van der Veen

2018. If the authors wish to highlight the longstanding debate about an appropriate form of the sliding relation, I suggest older (classic) references: Kamb 1970; Budd et al 1979; Weertman 1957, 1964, 1972; Lliboutry 1968, 1975, 1979; Nye 1969, 1970, etc. No need to cite them all, of course, but to me the longstanding and ongoing debate is more compelling than the "present uncertainty".

We appreciate your suggestions and have removed the Minchew et al., 2019 reference and added references to Kamb, 1970; Nye, 1970; and Lliboutry, 1979.

L129-130: Please describe how you calculated the stress coupling length, for readers who are not familiar with it.

We estimate the stress coupling length (SCL) following Enderlin et al, 2016 as $SCL = 4H$, where H is the average ice thickness in the region of interest. We will add the calculation to the manuscript.

L137: "Average absolute changes in inferred basal drag..." - What does this average mean? Is it the average of per-point change from one time step to the next, at each point along the flowline? Or is it averaged in some other way?

Yes, it is the average of per-point change from one time step to the next. We will change these lines in light of the new calculations of uncertainty and will make the description of the average more specific.

L142-143: "...implies that in the absence of terminus retreat glacier dynamics are largely invariant" can you be more specific? Invariant in what way? Which observable variables would you expect not to change, and over which time scales?

We have made the statement more specific to our observations. "...implies that in the absence of retreat secular glacier dynamics are largely invariant for these three glaciers during our study period." This was clearly too broad as changes in subglacial hydrology, surges, etc. would all be examples of changing glacier dynamics that occur without retreat. Thanks for pointing this out.

L154-155: "The climate system can also force retreat through processes at the ice-ocean boundary (Motyka et al., 2011)." Is the backstress example (3) from Nick et al. 2009 in the preceding sentence not an example of forcing at the ice-ocean boundary? Please clarify wording in these sentences.

Although you are correct, we feel there is a subtle distinction here that we did not previously make clear. In Nick et al., 2009 they trigger retreat by changing backstress, that stress is triggered at the ice-ocean boundary, as you point out, but retreat is subsequent and caused by the stress change, as opposed to increased frontal ablation that causes retreat then stress changes and acceleration. We acknowledge that these processes are clearly coupled, we try and show that in our paper, and our distinction is somewhat pedantic. To tie them closer we now state, "3) decreases in terminus backstress forced by ice-ocean interactions that causes acceleration and subsequent retreat" and in the below line say "Similar to 3), the climate system can also force retreat through increased frontal ablation and successive dynamic changes (Motyka et al., 2011)." The reason for being nit picky here is that we observe that stress changes largely occur subsequent to, and because of, terminus retreat.

L202-203: would it be more accurate to say "Ingia experienced an acceleration in ice flow velocity and associated two-fold temporal increase in lateral drag"?

Yes - thanks!

L234-236: "If these observations are representative of the ice sheet as a whole..." is a big assumption. Greenland has more than 200 outlet glaciers, and they are quite heterogeneous in terms of geometry, surface climate, ocean access, etc.—and I don't mean to lecture the authors on this, as I know they have published papers on the heterogeneity of Greenland outlet glaciers. Anyway, I suggest toning down this generalization, or else including several more sentences of interpretation.

We acknowledge as written the claim is speculative. We have attempted to tone down this generalization. We now say, "These observation suggest that one potential mechanism for the widely observed acceleration of outlet glaciers around Greenland (Murray et al., 2014; King et al., 2018, King et al., 2020) is a response to coupled changes in lateral drag and near-terminus longitudinal backstress initiated by terminus retreat. However, glaciers around Greenland inhabit a wide range of geometries, climate regimes, and fjord geometries (Morlighem et al., 2017; Catania et al., 2021; Felikson et al., 2021) so future study is likely necessary to understand the prevalence of the proposed dynamic connection between retreat and acceleration."

L239: can you specify *what* about the fjord geometry permits low basal drag extending inland?

The region of low basal drag occurs where the submarine bed topography is shallowly retrograde extending far inland to 15 km. We will added this to L239 in the discussion.

L261-262: please look for a few more references to support the claim that low basal drag conditions "are not restricted to these well-studied glaciers, but occur around Greenland". Shapero et al (2016) considers only the three outlet glaciers that I would argue are the most well-studied: Kangerlussuaq, Helheim, and Sermeq Kujalleq. That is not sufficient support for the more general statement that follows in L262-264. It would be very interesting to know how generalizable your findings are to other outlets, but we need evidence from more outlets than the "Big Three" for that generalization.

We have added more references here, removed the reference to "well-studied", softened the general claim in the following sentence, and backed up the generalization with a mechanistic explanation for low basal drag conditions at or near glacier termini in Greenland, i.e., "Conditions of low basal drag throughout the near-terminus region of glaciers are not restricted to the glaciers in this study, but occur around Greenland (Shapero et al., 2016, Sergienko et al., 2014, Seddik et al., 2018, Bartholomaus et al., 2016, Nick et al., 2012, Meierbachtol et al., 2016, Stearns et al., 2019) as many glaciers approach flotation conditions at the ice-ocean boundary. Thus, one potential explanation for the ongoing acceleration and retreat of outlet glaciers in Greenland, despite a pause in ocean thermal forcing (Wood et al., 2021), is the continued dynamic evolution of glaciers with sustained low basal drag conditions extending far inland."

Abstract lines 9-11: This claim is related to the manuscript, but not supported by the evidence you present. See above. Please remove, rephrase, or provide more evidence in the main text.

The last sentence of the abstract was re-written to align more closely with the evidence presented, i.e., "Glaciers with similar basal stress conditions occur around Greenland. Our results suggest that for such glaciers, dynamic mass loss can be sustained into the future despite a pause in ocean forcing."

I was surprised not to see any discussion of this manuscript's findings in context with those of Felikson et al. (2017, 2021), especially given the overlap in authorship. It would be helpful to me as a reader if the authors would discuss those works.

We would have loved to add more about this work here, however, as far as direct comparison, only the Ingia glacier area in our study contains the Peclet limit (Peclet running maxima of three), ∼15 km from the 1985 terminus (Felikson et al., 2017). For Umiamako and Rink the Peclet limit is >40 km inland (Felikson et al., 2017). This lack of ability to analyze more than one glacier largely precluded us from anything but basic comparisons. For Ingia, the flow regime from the terminus to the Peclet limit is characterized by low driving stress and basal drag. At the Peclet limit both driving stress and basal drag increase by nearly a factor of two. This suggests that the ability for thinning waves to diffuse up glacier is linked to the stress state of the glacier and potentially the ability for stress changes to be transferred upstream (Bondizo et al., 2017). Fundamentally, both the pre-retreat stress state that we identify and the Peclet thinning limit identified by Felikson et al., 2017 highlight the importance of the glacier geometry in determining the dynamic response to retreat. Furthermore, we find here that thinning is subsequent and in response to retreat. This ordering is consistent with, and helps to fill in, the chain of events suggested by Felikson et al., 2017. We will add these points to the discussion as well as highlight the interesting, but somewhat anecdotal, results we find for the Ingia Peclet limit to motivate further study.

Technical corrections

L24-25: "heterogeneous changes in elevation (Csatho et al., 2014; Felikson et al., 2017) and velocity (Moon et al., 2020)."

L25: "This means..." - what is "this"?

L37: "circumnavigate elevation data scarcity" —¿ "circumvent scarce elevation data"

L61 replace parenthetical citation with in-text citation

L78-80: "Such observations... (Shapero et al 2016)" — I suggest removing this sentence. I want to know more about what you did, not necessarily what others have done before.

L84: "Zoet and Science, 2020" —¿ "Zoet and Iverson, 2020"

L103: "two additional data products are necessary. . . " should have a colon rather than a comma

L116: "model's" missing apostrophe

L138: why not reference the relevant figure directly after mentioning each glacier? That is, "16 kPa for Rink (Fig. 4); 25 kPa for Umiamako (Fig. 3); and 11 kPa for Ingia (Fig. 2)"

L147: "buoyant"-¿ "buoyancy"

L150: remove semicolon

Sections 3.2-3.4: check verb tenses. "We observe" in the present tense makes sense to me, but in describing the results you use both past and present tense. For example, "Umiamako experiences. . . driving stress substantially increased" appear together in one sentence.

L184: consider "not only lateral drag" rather than "just"

L244: "maxima" is plural. Try "A maximum. . . is" or "Maxima. . . are".

L250: colon rather than comma when starting the list of glaciers with observed or modeled stress fields

L251-254: please use the official name of Greenland's largest outlet: Sermeq Kujalleq. For clarity, you might consider "Sermeq Kujalleq (also called Jakobshavn Isbrae)" or similar. See Bjørk, Kruse Michaelsen (2015).

L269: "dictates"-¿ "determines"

Figure 1: Please annotate a bit more and/or include more description in the caption. For example, what do the green and white regions indicate on each plot? Can you include arrows to show the direction of ice flow on one of the plots? I suggest glossing the abbreviations you use (e.g. "Ingia (Ing)") in the caption, even if you think they are obvious.

All fixed, clarified, or expanded on - thank you!

References in this review

Felikson, D., Bartholomaus, T., Catania, G. et al. Inland thinning on the Greenland ice sheet controlled by outlet glacier geometry. Nature Geosci 10, 366–369 (2017). https://doi.org/10.1038/ngeo2934

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Bjørk, A. A., Kruse, L. M., and Michaelsen, P. B.: Brief communication: Getting Greenland's glaciers right – a new data set of all official Greenlandic glacier names, The Cryosphere, 9, 2215–2218, https://doi.org/10.5194/tc-9-2215-2015, 2015.