The Cryosphere Discussions

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Comment on tc-2022-114

Anonymous Referee #1

Referee comment on "Observed mechanism for sustained glacier retreat and acceleration in response to ocean warming around Greenland" by Evan Carnahan et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-114-RC1, 2022

Review of 'Observed mechanism for sustained glacier retreat and acceleration in response to ocean warming around Greenland' by Carnahan et al.

Summary

This manuscript presents a stress balance analysis for three tidewater glaciers that have a contrasting retreat history over the study time period (1985-2015). By analysing the stress balance throughout a period of retreat, and by contrasting the glaciers, the authors aim to elucidate the possible drivers of retreat and controls on its ultimate duration.

The manuscript aims to make the significant point that at least for the 2 studied glaciers that retreated, it was a terminus perturbation that initiated retreat (so that retreat led to acceleration and then thinning), as opposed to for example a reduction in basal drag (for which the order would be acceleration then thinning then retreat). The manuscript furthermore shows how the pre-retreat stress configuration determines the susceptibility of a glacier to long-term retreat.

The manuscript is certainly important and is appropriate for The Cryosphere. I feel that there remains much uncertainty on the drivers and controls of tidewater glacier retreat and this manuscript can make a significant contribution to this topic. In addition it was a pleasure to read because the manuscript is well-written and the figures are excellent.

Having said all this, I do feel that three significant points need to be addressed before I feel completely convinced by the argument; these relate to (i) being more precise about the time stamping of the data and the implications for the analysis, (ii) a more thorough and extensive treatment of the errors and consideration of how these errors affect the interpretation of the results, and (iii) a more thorough analysis or discussion to discount thinning as a possible driver of retreat. I detail these points along with some more minor comments below.

Major comments

Time stamping of data

I found the time stamping of the data to be a bit imprecise and in some cases inconsistent. For example, on L87 and in Fig. 1 the date of the ASTER DEMs is stated as "~2002", but in the figures and results the date of this DEM is sometimes said to be 2003 (e.g. legend on Figs. 2-4, L158). Could you be more precise at L87 about what "~2002" means? And then be consistent throughout the manuscript on this date?

In relation to the timestamping of the DEMS, can the DEM (and therefore the accompanying stress analysis) really be said to be pre-retreat for Umiamako? On L160 you look at thinning between 1985 and 2002 but Umiamako began retreating in 2001 (L55). Does this affect your conclusions at all?

What part of the season is the DEM appropriate for? I ask because if the seasonal surface elevation change from surface mass balance alone is significant (say 5m or something from late summer to late winter), then when you remove the surface mass balance in order to get the dynamic thickness change (L157) presumably it matters whether the DEM is timestamped to summer or winter. Perhaps this could affect the stress balance too. Similarly I note you used annual mean ice velocities – would seasonal variability in ice velocity (e.g. Howat et al. 2010) affect your results?

To sum up this point I feel the manuscript would be improved with a bit more clarity and explanation around the time stamping of the datasets, of how you have accounted for seasonal variability (or whether this matters), and how these details affect your results.

Treatment of errors

I found the treatment of errors to be a bit confusing and not sufficiently thorough. For example, on L108 it is stated that "errors in inferred basal drag using the force balance with BedMachineV4 are estimated to be <15 kPa", whereas on L114 you state a maximum error of 60 kPa in inferred basal resistance. Are these statements contradictory? In relation to the first statement, is it suggesting that the errors would be different with a different bed product?

In general, are you relying on Stearns and van der Veen (2018) for your error estimation as suggested by L114-115? But presumably your manuscript uses different input datasets (DEMs, velocities, updated BedMachine), and does different processing (the two sets of smoothing on L112 and L125), so aren't your errors likely to be different?

I also don't follow why the errors in inferred basal drag are necessarily "consistent in time" (L109). I can understand why this would be the case if an error is arising from your estimate of ice viscosity, and if ice viscosity is assumed to be constant in time. However, you have different inputs to your estimates at different times (DEMS, velocities) and so presumably these could give rise to different, time-variable errors?

Overall, I think it would be great if you could add to the methods a more thorough treatment and explanation of the errors, and I think it would be useful to add some shading or some sort of other indication of the error on the stress to Figs. 2-4.

Lastly, and depending on your response to the above points, I think it would be great to be more conscious of the errors when discussing the results. Two particular places I feel this could be important are: (i) on L167 when you talk about "a drop in longitudinal coupling resistance in the near-terminus region of 10 kPa" – is this outside of uncertainty?, and (ii) on lines 157-163, could you comment a bit on what the errors are on these dynamic thickness changes? Ideally you would have a +/- attached to each estimate. I ask because these are relatively small changes that are comparable to the uncertainties you describe in L91-93, and furthermore you have removed a RACMO surface mass balance signal that presumably itself has significant uncertainty. Therefore, can we say that these glaciers are dynamically thinning or thickening outside of uncertainty?

Thinning versus terminus perturbation-induced retreat

One of the principal take-homes from the manuscript is that the retreat of Ingia and Umiamako is initiated by a perturbation at the terminus, because little thinning is observed prior to retreat (e.g. L162). I largely agree, but if we are going to be really rigorous, I feel we should ask what threshold of thinning we consider to be insufficient to drive retreat. For Umiamako, there is some thinning of 5-10 m prior to retreat (if we take the 1985-2002 time period to be prior to retreat – see above). Clearly this is a small amount of thinning relative to what you get once full-on retreat is initiated, but that doesn't completely rule it out as the perturbation that started the retreat. Is the height above buoyancy for Umiamako (Fig. 1e & 3a) such that 5-10 m of thinning could unground a significant portion of the terminus? Based on Fig. 3a it does look like the bed deepens inland in the first few km such that the glacier might be approaching flotation there in 2002/2003. I feel that a bit more quantitative analysis and discussion is needed here to fully back up the idea that thinning prior to retreat is not the driver of retreat. Perhaps adding a plot of height above buoyancy along the flowline in the near terminus region would help?

Minor comments

L23-25 – This may be preference, but I feel this sentence would read better if all the references were put at the end.

L70 - Could you add a bit more detail (particularly including equations) for how you calculate the resistive stresses from the velocities? I see that you have written it in words around L70 but for clarity it would be great to see "R_xx = …"

L104 – can you clarify whether "all years in our study period" means every year from 1985-2015 or just 1985, 2002, 2007 and 2015?

L105 – putting the last part in parenthesis might be better grammatically?

Fig. 2 caption – suggest "terminus at that time" would be better than "current terminus".

Fig. 1d could possibly benefit from a different color scale because it's difficult for the reader to evaluate the statements in L157-163 when much of the glacier looks to have a surface elevation change of approximately 0.

L163 – "ice-ocean processes" – I'm wondering if perhaps this should more accurately be "calving front processes", because I guess in theory something like increased calving driven by hydrofracture would be consistent with your observations but is not an ice-ocean process.

L165 – "along-flow gradients in longitudinal stresses support driving stress" – is the "gradients" part necessary here? Wouldn't it just be longitudinal stresses supporting driving stresses?

L176-177: "The Ingia terminus region does not experience a significant change in driving stress or basal drag even as the terminus retreats" – in Fig. 2c the basal drag does change significantly. Although the values do become unphysical, I feel that this statement needs modified. Presumably since all stresses must sum to 0, the negative basal drag values are telling us that one of the other stress components is slightly out too?

L208 – "compressional flow" – to me, compression, at least in the along-flow direction, is when the velocity is decreasing along-flow, which would be between 8-11 km on Fig. 4a. I don't really follow how "the compressional regime is evidenced by large gradients in longitudinal stress" because this does not follow my understanding of compressional flow. If we were only consider the along-fjord longitudinal stress (ignoring the across-glacier direction) then wouldn't compressional flow be associated with negative longitudinal stresses rather than gradients in longitudinal stresses? Or put another way, a flow can still be entirely extensional even in the presence of gradients in longitudinal stresses. Perhaps the authors can expand/explain?

L213 – on L127 you state that "calculated stresses can not be interpreted at length scales below the stress coupling length of each glacier" while here you state that "the proportion of the driving stress supported by longitudinal resistance increases upstream of the terminus region". In making this statement are you not interpreting the stress at length scales below the stress coupling length? The increase in longitudinal resistance looks to be within the stress coupling length, and then further upstream the longitudinal resistance decreases for a bit.

L240 – perhaps "little resistance from basal drag" would be more appropriate since it is non-zero.

L237-249 or another relevant place – I wonder if you could discuss to what extent your findings on how the geometry/stress state determine the susceptibility to retreat relate to those of Felikson et al. (2021)? I feel the work of Felikson et al. (2021) should be referenced and discussed in relation to your results.

L250-265: Relating to the wider significance of their results, I wonder if the authors could include a short discussion on what sets the basal drag? From a subglacial hydrology perspective, one might expect basal drag to depend on conditions at the bed – i.e. the presence of water or sediment and the state of the hydrological system. On the other hand, in your results (Figs. 2-4), there is a strong imprint of the overall stresses on the basal drag, and the overall stresses respond to the geometry of the bed and glacier, suggesting that the large-scale geometry of the system plays a role in setting the basal drag. How do we reconcile these two different viewpoints? This feels important because how the system responds in the future might be different depending on what is setting the basal drag. I understand that addressing this properly is beyond the scope of your results, but I think some discussion would be very helpful.

L279 – "reductions in heat delivery to termini" – I feel this needs some qualifying or further specification. I guess that you are probably referring to the Wood (2021) paper, but as written I feel there is a danger of someone thinking that in general we expect reductions in ocean heat delivery in the future. This also applies to L11.

Typos

L61 – no need for parenthesis on reference

L109 – "affect" rather than "effect"

Literature cited

Felikson, D., Catania, G. A., Bartholomaus, T. C., Morlighem, M., & Noël, B. P. Y. (2021). Steep glacier bed knickpoints mitigate inland thinning in Greenland. Geophysical Research Letters, 48, e2020GL09011