The Cryosphere Discussions

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Comment on tc-2022-141

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Referee comment on "Timescales of outlet-glacier flow with negligible basal friction: theory, observations and modeling" by Johannes Feldmann and Anders Levermann, The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-141-RC2, 2022

In this study, the authors derive a scaling relation which provides a simple prediction for the characteristic timescales of outlet glaciers in Greenland and the Antarctic. They assume the flow can be described by the SSA and neglect basal friction in the momentum conservation equations. Based on the width of these outlet glaciers being an order of magnitude less than the length of the flow, they determine the leading order terms in the momentum equations and show that the driving stress is balanced by the lateral shear stresses for these geometries. This leads to a dimensionless relationship which compares ice flows with similar properties. It is a clean and simple formulation which, by focussing on similitude, abstracts many of the complex interactions which govern the ice flow. The resulting scaling relation only requires the geometrical properties of the outlet glaciers (depth, width and length) together with ice softness, to determine the characteristic flow timescale. The derived relationship does not make absolute predictions, but instead makes predictions relative to other glaciers with similar properties.

The authors then go on to test this relationship thoroughly. First, they compare the predictions against the timescale inferred from velocity measurements of various Greenland outlet glaciers which exhibit similar topographic properties. These experimental results are promising. Second, they compare the predictions against the retreat timescale from idealised numerical simulations performed within the PISM ice sheet model. This comparison requires the assumption that the flow timescale and retreat timescale are proportional. In this idealised simulation that does indeed appear to hold true, and the authors find excellent agreement to their scaling relation. Finally, they use the derived scaling relationship to make predictions of the relative retreat timescale for a number of Antarctic outlet glaciers with a retrograde bed that may be prone to MISI-type retreat.

I enjoyed reading the paper. The manuscript was well laid out, with the complexity of the mathematical derivations, and technical details of data extraction and experimental set-up nicely compartmentalised into separate appendices. The results are well explained, with helpful interpretations of the intuition behind a number of the mathematical results. I also appreciated the comparison in Sec 5 to their earlier work (Levermann and Feldmann,

2019) which considered a 1-D flowline with basal shear stresses included, but lateral drag neglected. Taken together, the conclusions from these two studies can be considered to give a range of predictions under the differing assumptions.

I have some comments and questions about the study, but I'm hoping these can be addressed through adding a bit more discussion to the manuscript rather than requiring any major changes to the results. I would fully recommend publication with these additions.

Specific Comments:

- This study neglects the contribution of basal shear stresses, which the authors justify through reference to various papers (L81-L83) which infer a low basal friction coefficient for the rapidly sliding ice stream outlets of Antarctica and Greenland. Since a low basal friction coefficient, if combined with rapidly flowing ice, doesn't necessarily translate to negligible basal shear stress, it would be good if the authors discussed this decision a bit more in the manuscript. Maybe they could discuss the expected dominance of lateral drag in deep, narrow confinement channels. And/or the results from the idealised Antarctic simulation which includes basal friction, could be included as a post-hoc justification?
- It was only after reading the paper fully that I understood the distinction between the *flow timescale* which is inferred from the surface velocity of the ice stream and used in the derivation of the scaling relation; and the *retreat timescale* for the speed of grounding line retreat, which is that simulated and predicted for the Antarctic outlet glaciers. I think it would be helpful for the reader if that distinction was emphasised in the introductory section of the text, and added as a fourth point in the potential limitations listed in Sec 5.
- Related to the above point, the excellent agreement between the timescale for grounding line retreat in the idealised Antarctic simulations, and that predicted theoretically by the flow timescale, is perhaps surprising. It suggests a mathematical relationship which holds true in this idealised set-up. The authors allude to this on L156: "*Grounding line retreat depends on the divergence of grounding-line discharge, i.e., on the divergence of the flow speed at the grounding line. If we were to seek a relation fo the grounding-line retreat , we could make the assumption that the retreat speed of an outlet glacier if proportional to its flow speed.*" Could the authors include the mathematical reasoning for this? This would also help explain under what conditions the assumption that "retreat timescale = flow timescale" is correct and thus how those conditions are being met for the retrograde slope in the idealised simulations. Related to this I think the commentary on L11. and in Fig 1 caption that, "*the flow velocity and its spatial derivaative are proportional*" may be misleading. If I have worked out this relationship correctly, I believe it should be that $\partial h/\partial t$ ☐ $h \times \partial u/\partial x$? Not u ☐ $\partial u/\partial x$?
- In the experimental testing of the predicted scaling relation to the grounded Greenland outlet glaciers, the authors take the average over 60km-0km upstream of the grounding line for the estimated width, and 60km-20km for the estimated velocity. I

understand the authors’ comment that the velocity is being cut off to avoid pollution from the ice-ocean interactions for the last 20km, but the velocities generally seem to increase in the last 20km coinciding with the width narrowing. Therefore, would it not make more sense to also exclude the last 20km from the width estimation so that you are comparing like-with-like? How sensitive is the fit of the data to these choices? The good fit of the scaling relation to the Greenland outlet glaciers lends confidence to the similitude approach being valid across real word glaciers, i,e. that glaciers exhibit enough similarities that this simple scaling can be applied across them. It therefore seems important to make sure the conclusions are robust, and not sensitive to these slightly arbitrary choices.

- For the plots in Figures 3, 6 and S4, would it make sense to use a log plot so that the predicted scaling relationship gives a straight line (with the gradient equal to the exponent in the scaling law)? Deviation from the expected behaviour would then perhaps be easier to see by eye. It would also be helpful to plot the OLS estimate from the data and compare the gradients.

Technical Corrections:

- There are a number of places in the text which refer to the scaling relation being linear in the ice temperature: L8, L129, L253. However, my understanding is that the linear relationship being referred to is to the ice softness A. The ice softness is temperature dependent, but not linearly dependent on temperature, I believe?
- L465-L468. I found this description of extracting the length scale confusing. Could it be rewritten? Is it just a justification for setting L=1/S in the scaling equations, or is something else going on here?
- Is there a reason for picking p1 to p1+20km as the distance over which to estimate W? Naively I think I would have expected the estimate to be the average over p1 to p2.
- Clarify that these multiple combinations are coming from the multiple flow lines for each outlet glacier. (Unless I have misunderstood, in which case even more clarification needed!)
- Could you elaborate on why you have chosen the 17^{th} and 83^{rd} percentiles, rather than using the 5^{th}-95^{th} percentile range?
- Delete “respectively”.
- This reads as if the uncertainty range in Table 3 reflects the breakdown in the similitude requirement. My understanding was that the uncertainty range still assumes geometric similarity and that the scaling relation holds; instead it reflects uncertainty in the appropriate average value to take for the different geometrical quantities due to topographic variation in the outlet. Are those two things the same?
- On its own this explanation of running a separate set of experiments with reduced C is a bit confusing. I would make it clear that the first set of simulations had a non-negligible C, but in both cases the scaling relation held, which suggests the conclusions in this paper are unaffected by ignoring the basal friction in the derivation.
- Should this be 0.07 not 0.7?