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Discussions

Interactive comment on "Are seasonal calving dynamics forced by buttressing from ice mélange or undercutting by melting? Outcomes from full-Stokes simulations of Store Gletscher, West Greenland" *by* J. Todd and P. Christoffersen

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Review of "Are seasonal calving dynamics forced by buttressing from ice mélange or undercutting by melting? Outcomes from full-Stokes simulations of Store Gletscher, West Greenland" by Todd and Christoffersen

General comments

This paper outlines a model study of the terminus position and stability of Store

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Gletscher in West Greenland. A model is applied that solves the full-Stokes momentum equations along a central flowline of the glacier, with a basal friction field determined from inverse methods. The glacier is considered isothermal, and the geometry of the glacier is constrained using remote sensing data. The grounding line and the ice front position are allowed to freely migrate, and the model is capable of representing the development of a floating ice tongue. A novel flux convergence term is added to the incompressibility equation to account for lateral convergence or divergence in the flow. The stress field computed in the model is used to calculate the theoretical depth to which surface and basal crevasses would propagate, and calving is assumed to occur whenever surface and basal crevasses meet. Perturbation experiments are run with different combinations of undercutting by melting and buttressing by mélange to explore the calving dynamics of the glacier and its response to possible future climatic changes. The results indicate that the geometry of Store is principally responsible for its observed interannual stability, but that buttressing by mélange (which suppresses calving) is likely responsible for the seasonal advance of the glacier.

The perturbation experiments are well constructed, and a reasonable range of variability in mélange strength and duration and submarine melt strength and duration are explored. I think it would have been revealing to explore what conditions would be necessary to get Store to retreat into the overdeepening behind the basal pinning point and the constriction in fjord width. Even if you had to use unrealistic values of submarine melting or negative SMB or get rid of mélange altogether, it could be instructive to see what it would take to destabilize the glacier. This might also prove illustrative of the fidelity of the model setup. I find the discussion of the perturbation experiments a bit hard to follow in places (it's hard to keep track of all the different numbers and ratios of years being used), and some of the figures could be improved for legibility (e.g. coloring and scaling). Otherwise I think this is a novel contribution that will be well received by the glaciological community. Most of my comments are minor and can likely be addressed relatively easily.

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Specific comments

1. Were there any indications prior to your modeling work that the topography of Store was the principal reason for its interannual stability? If the fjord bottleneck and basal pinning point were known, then it's probably not surprising to find that geometry is the most important factor. This makes me wonder why Store was chosen, as there are surely other glaciers for which mélange and undercutting by melting might be much more important for determining glacier stability and terminus position. I do agree that it is a valuable result to demonstrate that glacier geometry is more important in this case, but you might give a bit more motivation for why Store was chosen (even if it is just for the availability of data to constrain the model). The sophistication of the model setup might also be used to find (or construct synthetically) a glacier for which it can be demonstrated that mélange or melt undercutting (or some combination of the two) are the dominant influences on tidewater glacier behaviour.

2. How much of the seasonal signal in ice front position is due to the imposed seasonal signal in basal friction? You might have attempted to partition the influence of this seasonality in basal friction by running some simulations with some kind of constant, annual-average friction at each point. The no-mélange results in Figure 6b seem to show evidence of this annual periodicity, which looks to be small here. However, the removal of mélange and the seasonal reduction in basal friction are likely (I'm guessing) to occur around the same time, and their combined influences may not necessarily be linear combinations of two separate effects.

3. The theory behind the crevasse depth models contains the assummption that crevasses are closely spaced, which will lead to stress shielding and reduce the high stress concentration that would otherwise surround an isolated crack tip. Since you are applying these calculations everywhere in the glacier domain, you are implicitly assuming that crevasses are closely spaced *everywhere*. You might

comment on how reasonable this is. It may not be too bad for surface crevasse fields, but what about basal crevasses? What would the implications be for basal crevasse penetration (and thus calving size/frequency) if basal crevasses form less frequently and are actually isolated rather than closely-spaced fractures?

4. You mention (p. 3541) that in some cases the terminus position during the melt season is actually more advanced. You don't mention how often this is the case, but you seem to brush off this result, suggesting that the calving dynamics appear unaffected by increasing melt magnitude. I think this point deserve more attention, however, as it seems like it could be important. Under what conditions do you see a terminus advance during the melt season? Does this depend on melt season length? What explains this behaviour?

Line-by-line Comments

- p. 3526, line 18: remove comma after factors

- p. 3527, line 5: "this process" is a bit vague here, perhaps be a little more specific

- p. 3528, lines 8–10: are you sure this is conclusive, i.e. is there still any debate about this in the literature? I still hear people question whether the advance and retreat of some tidewater glaciers coincident with the appearance and breakup of mélange, respectively, is simply coincidence. Could we be missing anything else physical here? This is more of a minor discussion point, but it might be worth adding a bit of nuance since this is introductory material that frames your work (which of course addresses this very issue, but not until the results are presented...).

- p. 3528, line 29: what about the last two decades? Your reference here from 1995 doesn't address what has happened since then, which is quite a long time...

- p. 3530, line 3: when I think of a "range," I think of two numbers that define some kind of upper and lower bounds. Do you mean 6600 ± 700 m a^{-1} here? This is indeed how you use the term "range" in a couple lines, but then you go on to talk about a range of 500 m for ice front position. Maybe a term like "variability" or something like that would be more appropriate in a few places?

- p. 3532, lines 5–8: Do you mean that for every date of the year, you take the average of the RACMO SMB for that date in every year from 1985 to 2008?

- p. 3534, line 4: this term is not really a creep closure term, but an overburden (or cryostatic) pressure term that *leads* to creep closure.

- p. 3534, lines 14–17: just because you interpolate something within your mesh does not make the results independent of the mesh, as the stress results themselves may have some mesh sensitivity (have you checked for this?). Furthermore, the interpolation depends on your choice of basis functions (linear, quadratic, etc.).

- p. 3534, line 19: the cryostatic pressure will be higher than other terms at the bed. There are no rate terms in Eq. 3.

- p. 3534, line 25: I'm confused here. Negligible difference in pressure at a given depth? i.e. between the open water and within a basal crevasse near the ice front?

- p. 3535, line 18: it seems like you could come up with some kind of geometric normalization of the sidewall friction near the terminus to account for the arcuate shape of the ice front. Or do you think your overestimation of friction in this zone is negligible?

- p. 3535, line 22: this is a bold statement, that a crevasse field "significantly" reduces bulk density. Of course the bulk density should be reduced, but it's not

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clear why this is necessarily significant. I would think that would depend on the specific geometric setting.

- p. 3539, line 20: I'm not sure what you mean here by "super-buoyancy," can you define this term? I think you describe what is going on here a little better in the caption of Figure 7.

- p. 3540, lines 9–11: this is confusing here. Velocity at a location is faster than a date?

- p. 3540, line 12: fix "with the a significant..."

- p. 3545, lines 12-13: the Krug reference was actually applied to Helheim glacier, not a synthetic glacier geometry.

- p. 3545, line 25: the presence of water in crevasses is not necessary for seasonal dynamics *at Store*.

- Figure 6: this figure is difficult to read. The colors are difficult to discern. I'm not sure it's necessary to show 5 years of results, as there isn't a lot of interannual variability. It might be better to just show 1 or 2 years, and work with the color scheme to aid in interpretation.

- Figure 9: perhaps clarify in the caption that the panel titles are in fractions of a year. It took me a while to figure this out. It might be worth labeling each sub-panel (a through f), as it took me a while to figure out what each panel meant and how the experiments varied left-to-right as well as top-to-bottom. There's a lot of good information in this figure, it just took me a while to get it!

- Supplemental Equations S3 through S5: in S3 and S4 you use U_x, but in S5 you use a lower case u_x. Is there supposed to be a difference?

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- Supplemental: proponents of XFEM would take issue with your claim that FEM is "inherently incapable of dealing with fracture...." It is possible to account for fractures with the use of suitable enrichment functions in XFEM.

- Supplemental S6 and thereafter: I was confused by the use of H as a surface elevation variable. I kept thinking of thickness in my head. Wouldn't it make more sense to use something like z_{bed} and z_{surf} in Eqs. S6 and S7 (and in the figure)? What you're trying to show (in words, and correct me if I'm wrong) is that the height variable on the bed is equal to the surface elevation of the bed, and same for the surface. It's kind of confusing the way you've written the equations.