The Cryosphere

Open Access

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

J. Todd and P. Christoffersen

jat71@cam.ac.uk

We are very grateful to Dr Borstad for his thorough, technical and insightful review of our paper. We address the points in turn below, with original comments in black and our responses in red.

C1903

1 General Comments

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

C1904

(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.

2 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.

Store Gletscher is the target of an extensive, ongoing field investigation by our department, in collaboration with Aberystwyth University. This field campaign aims to answer questions both about the calving dynamics and the basal conditions of Store. The availability of this data is one of the main reasons for choosing this glacier.

Additionally, we feel that if we seek to use new models to understand and reproduce long-term changes in the dynamics of calving glaciers, we should first attempt to investigate the *seasonal* changes onto which these longer term trends

are imposed. Store Gletscher is ideal from this perspective, as it displays a large seasonal range in dynamics while maintaining long term stability. As such, we are able to focus on investigating the "normal" behaviour of a fast-flowing outlet glacier, before attempting to investigate how long term change throws these systems into disequilibrium. We have added a brief statement about the suitability of Store (p.3528,l.27).

We agree that an investigation into various synthetic geometries would be interesting and we would consider this for future work, but we believe it to be outside the scope of this investigation.

Related to this comment, and to the general comments above, we experimented with unphysically large values for melt rate in order to force retreat into the trough. We found that melt rates larger than velocity were required to force this retreat. We chose not to include these results as they are not representative of a real climate scenario. Furthermore, following the commencement of rapid retreat through the trough, we found that the model breaks down after ~25km of retreat, before reaching a stable pinning point. This is because the model currently doesn't include the ability to fully remesh the glacier geometry; rather, we manipulate the location of the nodes following a calving event. This works well for all but the most extreme changes in geometry. This is something we hope to improve upon by undertaking full remeshing in future work.

We have updated the text at p.3544,l.1-3 to mention this result: **"We found that, by forcing the model with unphysically large values for submarine melt rate (not shown), we were able to force the terminus back off its pinning point, which led to rapid retreat through this trough."**

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 Fig-

ure 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.

Seasonal changes in basal friction have an effect on velocity at the terminus, but appear to have a negligible effect on terminus position. The blue lines in Fig. 5a,b show terminus position and velocity, respectively, for model simulations where changing basal friction is the only imposed perturbation (Also shown in Fig. 6). While the effect on velocity is cleary discernible, front position remains constant throughout the year. As such, we maintain that changing basal friction has no effect on calving front position in our model.

3. The theory behind the crevasse depth models contains the assumption 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?

This is a good point. For surface crevasses on Store Gletscher, we are confident that this is a good assumption; aerial photography over the terminus of Store presented by Ryan et al. (2014) show that surface crevasses are indeed closely spaced. If basal crevasses were found to be more sparse, the stress concentration effect would be larger, and so these crevasses would penetrate further upwards into the glacier. However, there is no data available as to the spacing of the basal crevasses, and so we choose to include them within the same theoretical framework for simplicity.

C1907

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?

The graph in Fig. 9 shows the location of the surface of the terminus, as opposed to the depth-averaged front position, or the location of the terminus 'toe'. We chose to present the data in this manner to maintain consistency with observational records of front position. The variability of the front position between different melt perturbation experiments is of the order of 200m. Somewhat counter-intuitively, these ~200m advances occur as a result of progressive undercutting. Calving appears to more strongly dictate the location of the 'toe' than the surface, and so, as progressive undercutting occurs, the toe remains in the same position, and the surface advances. A higher melt-rate is more rapidly able to undercut the terminus, and so the surface is able to advance further away from the toe before calving occurs. We have added the following to the text to better explain this (p.3541,l:23-27):

The response of the modelled terminus to increasing melt *magnitude* appears somewhat stochastic. **It should be noted, however, that the positions shown in Figures 5, 6 and 9 represent the terminus at the surface, which is able to advance into the fjord when undercutting takes place, due to the fact that the glacier's topography exerts a control on the position of the grounding line.**

C1908

3 Line-by-line Comments

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

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

- 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...).

  This is a good point, and is a good justification for why a modelling study is needed! We have updated the text to reflect this. (p.3528,l.7,l.15)

- 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...

  True, we have added a reference to Howat et al. (2010), which demonstrates stability over the past decade.

- 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?

  Yes true. Changed where appropriate

C1909

- 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?

  No. Because we do not investigate the effect of seasonal variability in SMB on calving (assuming it to be negligble), we impose a constant annual SMB throughout the simulations. This average annual SMB was found by averaging the entire record 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. True, changed

- 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.).

  You are right that the results are not independent of mesh resolution. We have changed **"independent of the model's mesh resolution"** to **"reasonably insensitive to the model's mesh resolution"** on p.3534,l.16-17. The reason for saying this was that, prior to implementing the interpolation, calving wouldn't occur until an individual node experienced both surface and basal crevassing. This setup meant that the occurrence of calving was *totally* dependent on the distribution of nodes in the mesh.

  When experimenting with model setup, we tried different mesh resolutions at the terminus, and chose a resolution which we were confident was sufficiently high to capture the near-terminus stress field and beyond which little was gained.

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

C1910

- 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? Yes, or in other words, negligible difference in theoretical borehole water level at any point near the front. We have changed the text to clarify this, thanks.

- 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?

  It is not completely clear to us what exactly the reviewer has in mind with regards to geometric normalization and have left the text unchanged.

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

  We agree that crevassing may not always lead to "significant" change in bulk density, so we have changed "significantly reduces" to "may significantly reduce" on p.3535,l.22.

- 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.

  We have updated the text to clarify. By "super-buoyancy" we meant that the ice is being forced below the flotation level, and is then progressively forced back up out of the water by buoyant forces acting on the base.

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

C1911

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

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

- p. 3545, line 25: the presence of water in crevasses is not necessary for seasonal dynamics *at Store* (my emphasis). Fair point, changed.

- 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.

  In producing this figure, we were faced with the challenge of maintaining readability whilst also convincing the reader that our model is interannually stable. Taking your feedback into account, we've opted for 3 years, as 5 was probably unnecessary.

- 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!

  Thanks for the useful feedback on this figure. We agree it is quite complicated and could be clarified. We've labelled each sub-panel as you suggest and included explanatory titles above the fractions of a year.

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

  No, this was a mistake, thanks for pointing it out.

C1912

- 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.

  This is interesting, and wasn't something we had come across before. When thinking about how to modify the text accordingly, it occurs to us that it's not the inability to deal with fracture that's the problem, it's the instantaneous change in domain shape. The method outlined in this section would still be required even with a proper treatment of crack propagation. We have changed the text to reflect this and to avoid the claim that FEM can't handle fracture.

- 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 zbed and zsurf 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.

  Yes, we should have avoided capital H, due to its typical use to define thickness.

  However, we want to highlight the distinction between the height *variable* and the z *coordinate*, so we opt to change to lowercase "h" rather than zbed and zsurf. This also emphasises the fact that "h" is the same variable through the domain, and we simply set its boundary conditions based on the z-coordinate of the surface and bed.

Interactive comment on The Cryosphere Discuss., 8, 3525, 2014.

C1913