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Interactive comment on "Sensitivity of calving glaciers to ice-ocean interactions under climate change: New insights from a 3D full-Stokes model" *by* Joe Todd et al.

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The paper "Sensitivity of calving glaciers to ice-ocean interactions under climate change: New insights from a 3D full-Stokes model" by Joe Todd, Poul Christoffersen, Thomas Zwinger, Peter Råback, and Douglas Benn investigates the response of Store Glacier's ice front position to various forcings using a high-resolution full Stokes model. They examine the effect of an increase in undercutting by submarine melting, the effect of a concentrated vs distributed melt rates at the calving face, and a reduction in the backstress exerted by ice mélange. Overall, the authors find that Store is stable for a wide range of conditions, but starts to retreat dramatically for the strongest scenarios.

The authors also highlight the important role of bed geometry in ice front dynamics.

The paper is well written and easy to follow. Calving is a critically important process that needs to be better understood in order to reduce the uncertainty of projections from ice sheet models. This is an important and timely contribution that I recommend for publication in TC after some minor revisions. I have some suggestions that I hope will help improve the manuscript.

1 General Comments

I understand that the model setup is described in Todd et al. 2018, but I still think that some important model characteristics, such as mesh resolution, time stepping, or the undercutting parameterizations should be mentioned again. More importantly, one key aspect of this model compared to other existing models is that (if I understand correctly) the calving face *is not* assumed to be vertical. The effect of undercutting is therefore more rigorously simulated, which is a significant advantage over other models. This should be clearly stated in the text, as most models (including mine!) model undercutting as an extra calving rate as the calving face is always vertical.

Another important point is that some physical processes discussed in this paper are assumed to be fully established and validated, while they are actually still poorly understood or controversial. The effect of ice mélange for example: it has been proposed that ice mélange could prevent iceberg from overturning, thereby inhibiting calving. This has been shown by Amundsen et al. 2010. However, the jury is still out when it comes to the potential buttressing effect (i.e. mechanical stress) that ice mélange would exert on the ice stream. Some other studies suggest that the presence of sea ice is only a reflection of oceanic conditions that are actually the dominating control on calving. While it seems clear that ice mélange may prevent calving, it is still not clear whether ice mélange has any (direct) effect in terms of buttressing.

Another important process is how crevasses propagate. The Nye approximation remains a simple approximation and has not (to my knowledge) been fully validated. The presence of basal crevasses upstream of grounding line is not seen in most of the radar echograms that I have seen. Basal crevasses start to form at the grounding line where tidal bending occurs, but (to my knowledge) are not visible under grounded ice. The authors present the calving law used here as “the truth”, but it remains one possible (promising) representation of calving. I think the text should be less definitive in some places.

I obviously also have to say a few words about the comparison with Morlighem et al. 2016. The study presented here is undeniably more sophisticated but I still think that the authors have not rigorously demonstrated that a depth integrated model would respond differently. It is very difficult to compare the model from Morlighem et al. 2016 and the one presented here: they have a different initial state (this one is relaxed, so it may start from a different surface height), we use different meshes, possibly a different bed, different boundary conditions (I did not model lateral friction), etc. For example, I did not account for the presence of mélange: the inversion of basal drag is therefore expected to yield a slightly higher stress compared to the one of this model since the ice front includes here a stronger back stress from ice mélange. This will have an impact on the model sensitivity: my model having a stronger basal stress will be more stable. While both studies agree on many points (e.g., overall stability of Store, strong control of the bed geometry, etc), it is very difficult to disentangle why the models require different melt rates to be dislodged from their current position. We cannot claim that the model presented here is “better” simply because it is based on Full-Stokes. It is not what is shown. The only way to show that would be to collapse the model and run it with an SSA approximation (Elmer has this capability): that would make it possible to compare apples with apples. It would be great if this could be tested, but if this is not possible, the text needs to be less definitive in places (e.g., use conditional instead of present tense). ISSM also has a full-Stokes solver and from my experience, the results have almost always been very similar to results obtained with SSA. That being said,

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Discussion paper

this was for vertical calving fronts.

Finally, I found a bit unfortunate that the model breaks as soon as something interesting happens. I am surprised that reducing the time step does not fix the problem. Is there any possible way to improve the stability of the numerical implementation? At the end of the day, we want to be able to model ice front retreat, not just demonstrate its stability...

Again, I think this is a great piece of work, I just think that a few things need to be put in perspective and some statements need to be nuanced.

2 Specific comments

- title: the title is too generic. This paper is about one specific glacier, Store, I am not sure why the title says “Sensitivity of calving glaciers”, it should be “Sensitivity of Store Glacier to ...”
- p1 l38: Figure 2 is referenced after figure 1 (only mentioned page 2 line 26), maybe the order of the figures should be changed
- p3 l24: 162 m (space between number of m, also l37 and in other places)
- p7 l20: this statement is misleading, ISSM is not “depth-averaged”. We implemented several ice flow models in ISSM, including SSA and full-Stokes. In Morlighem et al. 2016, the depth-averaged SSA model was using, but ISSM itself is not depth-averaged.
- p9 l9: -400 m a.s.l. sounds a bit awkward, maybe replace by 400 below sea level?
- p9 eq3: the hydrostatic imbalance quantity is difficult to appreciate. I think using the height above floatation (multiplying this quantity by $H\ \rho_i$) would make it easier

to evaluate how much the ice has to thin to be floating. The figure should also be adjusted accordingly.

- p19 figure 3: all subplots have -2.132e6 at the top of the y axis, it is probably the offset for y, but it is not clear... It is a bit confusing, I am not sure it is necessary.

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