Repsonse to Reviewer Comments by Matthieu Morlighem on “Sensitivity of calving glaciers to ice-ocean interactions under climate change: New insights from a 3D full-Stokes model” by Todd et al.

We are very grateful to Matthieu Morlighem for his feedback on this manuscript. We provide responses to reviewer comments in red below. Page and line numbers refer to the updated manuscript.

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.

Good point. We have added a sentence describing the mesh resolution (P5L8), a couple of sentences briefly describing the timestepping and pointing the reader to Todd et al. (2018) for additional details (P5L18), and a more detailed description of our plume melting implementation (P3L15). We have also added a paragraph to Section 2.4 (P5L21) to highlight the fact that the calving front can be non-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.

We agree that the effect of ice mélange on calving remains an open question in glaciology, though a growing body of both observational and model evidence suggests that the effect is significant. We have modified the introduction to explicitly state that this effect is yet to be fully established (P2L10).

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.

This statement seems to imply that our model predicts basal crevasses in grounded ice, but this is not the case. In fact, basal crevassing begins at the grounding line (Fig. 6b, white line) due to bending, just as described by the reviewer. To clarify this, we have modified the text (P11L18) to state that the southern side of the terminus is floating. We have also added a statement to justify our use of Nye as opposed to LEFM (P4L35). Finally, in the conclusions, we have modified the statement on the importance of basal crevasses (P12L16) to indicate that this is only what the model predicts, rather than a definite fact.

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, this was for vertical calving fronts.

We agree that a rigorous 'Calving MIP' would be a useful and illuminating exercise. Given the effort that would be required to reformulate our calving model to operate with SSA, we believe this is beyond the scope of the present study. We also agree, therefore, that we have not *rigorously* shown that full-Stokes is required to properly model calving. However, in the present study and in Todd et al. (2018), we have demonstrated the importance of several factors which cannot be properly represented in SSA: the grounding line transition, vertical bending due to buoyant forces, ice cliff force imbalance, and undercutting by submarine melting (i.e. non-vertical fronts). Conversely, we argue that similar behaviour in ISSM does not undermine the importance of full-Stokes unless the calving law, frontal boundary conditions, grounding line dynamics and submarine melting are adapted to take advantage of the full-Stokes 3D solution.

We have tempered our conclusions with regards to SSA by changing 'is likely' to 'may be' on [P8L11], and removing the final sentence of this paragraph: "However, the inability of the ISSM model to reproduce the present-day terminus position highlights the need for fully 3D calving models equipped with physically-based calving laws." We have also modified P11L26 'it would not' to 'it may not', and P12L27 'will fail to capture' to 'may fail to capture'. Finally, we have added a couple of sentences to the end of the discusion [P11L36] to indicate that we expect long-term predictions of calving glaciers and ice sheets will be derived from models implementing simpler physics (e.g. SSA, higher-order approximations), and that we hope that insights from 3D full-Stokes simulations can feed into those models.

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.

Yes, it is annoying that the model breaks following significant retreat. This is a remeshing issue – we are planning to completely overhaul the remeshing algorithm, which will make the model significantly more robust. In response to this point, and a comment from the other reviewer, we have expanded the explanation of this problem in Section 2.2 [P4L24].

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 ..."

We have changed the title of the article to "Sensitivity of a calving glacier to ice-ocean interactions..."

• p1 l38: Figure 2 is referenced after figure 1 (only mentioned page 2 line 26), maybe the order of the figures should be changed

Good point – order changed.

• p3 l24: 162 m (space between number of m, also l37 and in other places)

Fixed, thanks.

• 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.

Apologies – we were not aware of this. We have updated the text accordingly (P8L6), to state instead that "In previous work, the Ice Sheet System Model (ISSM) has been used with a vertically-integrated SSA solver…".

• p9 l9: -400 m a.s.l. sounds a bit awkward, maybe replace by 400 below sea level?

Good idea – done.

• p9 eq3: the hydrostatic imbalance quantity is difficult to appreciate. I think using the height above floatation (multiplying this quantity by H ρi) would make it easier to evaluate how much the ice has to thin to be floating. The figure should also be adjusted accordingly.

We are purposefully trying to distance this analysis from the concept of height above flotation, because we know from the model (and observations) that the hydrostatic assumption is incorrect over short horizontal scales. Much of the ice in the southern side is significantly *below* flotation (hence the upward bending force), and the grounding line does not coincide with the HI=0 contour. In other words, over the spatial scales relevant to greenlandic outlet glaciers (few kms), height above buoyancy does *not* accurately predict the location of the grounding line. We have added a statement [P10L36] explaining the relationship between hydrostatic imbalance & height above buoyancy, and noting that this does not accurately predict grounding/flotation.

Looking into this further, we realise we have the wrong equation for what is shown in Figure 6c. It should read: $1 + (z_b/H)(\rho_{sw}/\rho_i)$. We have fixed this.

• 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.

Yes, this is the coordinates in rather unwieldy NSIDC Sea Ice Polar Stereographic North. We have reworked this figure based on comments by the anonymous reviewer, and have replaced the x/y coordinate scale with a scale bar instead.