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Author's response to reviews

We are grateful for the comments from two reviewers on the revised manuscript, and have addressed all the comments in the following ways:

Yours Sincerely,

Felicity Holmes, on behalf of all authors

Comments from Reviewer Prof. Douglas Benn (bold) and author responses (italic text):

Changes implemented by the authors in response to the first round of reviews have resulted in a much-improved paper. The text is much clearer, and the distinction between model and glacier behaviour is carefully maintained. I have only a couple of points where I believe a little further clarification is needed. These are:

Line 225: With regard to the TSP and TCD simulations, it is unclear what was actually changed in the model set-ups. How was the impact of crevasse depth and sea-pressure isolated in the two runs? This should be described briefly in the text and in a bit more length in the Appendix.

We have added in more clarification of what is changed in the TSP and TCD simulations into the 'Model Inputs' section of the manuscript, and have then referenced this section in Line 225.

Line 396: With regard to the lack of correlation between TSP and calving events, there should be some discussion here about the absence of elastic strain in the model. The model prescribes calving on the basis of crevasse propagation, which is predicted from the stresses in the ice. These stresses arise from deformations, which pull or push on the ice. On tidal timescales, viscous processes are too slow for these stresses to vary significantly in response to fluctuations in back-pressure – changes in pressure at the ice front simply cannot be transmitted fast enough by viscous flow to be 'felt' by ice upglacier. (This point could be investigated by the authors by comparing modelled stresses at high vs. low tides.) On the other hand, elastic response is much faster,

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allowing rapid transmission of back-pressure variations to potential crevasse locations. So, I think that the lack of correlation between TSP and calving events reflects the fact that variations in sea pressure (on the timescales of interest) actually have no effect on the glacier stress regime (in a purely viscous model), and hence have no impact on predicted crevasse depth or calving. In other words, the results likely reflect a missing process in the model rather than actual calving mechanisms. A few words should be added to the text to highlight this point.

Thank you very much for this comment – we have added in the following text which we believe summarises the point(s) that you raise above:

'The lack of correlation is likely partly a result of the absence of elastic strain in the model set-up used for this study. This is due to the fact that changes in stress regime as a result of changes in the exertion of sea pressure over a tidal cycle are transmitted too slowly by the purely viscous model used here to have an impact on crevasse propagation and, as such, calving.'

Line 416 (and 421): It is stated that: "When the undercuts are largest, there is also the greatest propensity for calving via the promotion of basal crevassing." Why should this be the case? Undercuts promote forward bending of the upper part of the glacier (less support at the base), which should promote surface crevassing and suppress basal crevassing. Modify these statements or provide better justification.

These comments have been edited to refer to how basal crevassing was found to be greater where sections of the glacier front are undercut/ near floatation in a previous study due to higher basal water pressures (Todd et al., 2018).

In addition, I spotted two typos:

Line 109: reference to Fig. 2 needed here.

This reference has been added in.

Line 351: 'where' should be 'whether'

This typo has been corrected.

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Comments from reviewer Prof. Jason Amundson (bold) and author responses (italic text):

The authors should be commended for the improvements that they made in response to the reviewers' comments. The paper reads better now; however, I personally think that it would benefit from another round of major revisions.

As pointed out by Doug Benn, the crevasse depth calving criterion was developed with the intention of modeling long timescale variations in terminus behavior, not individual calving events. That said, the crevasse depth calving criterion, like other calving parameterizations, is essentially a function that transforms stresses into calving rates. I think everybody agress that calving rates should depend on stresses, but there is significant uncertainty in the form of that relationship. If the paper focused more on stress (and velocity) variations, then it could be written in a way that would be somewhat independent of the chosen calving parameterization and would therefore be more impactful. Focusing more on stresses may also help to highlight what is really new about this study. There is a fair bit of literature on tidal response of glaciers, looking at both changes in ice flow and calving. The authors cite some papers, but I think there are more that they should include. I've listed some below. My list is definitely not exhaustive. These papers include both observational and theoretical/modeling studies. Previous observational studies didn't have information on terminus morphology (and certainly not on changes in terminus morphology) and as far as I'm aware the theoretical studies have assumed that the terminus is vertical. This study builds on that previous work by addressing the question of how terminus morphology affects glacier response to tidal variations.

Thank you for these comments, we have made changes to the text and added in additional references such as those detailed in your review.

So essentially, I'm suggesting the following:

1. Motivate the study by more extensively discussing the impacts of tides on glacier flow and calving, and point out that previous studies haven't considered the impact of terminus morphology on tidal response.

We have added in more information about velocity response to tidal forcings, including new references, in the introduction. In addition, a sentence has been added to the motivation to add additional emphasis onto the discussion of the impact of terminus geometry on calving/tidal response (L49-50, 59-61, 413-414).

2. Use the model to demonstrate how terminus morphology affects tidal response. You can then discuss in general terms how tides might be expected to affect calving rates since calving is a function of stress.

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The section headers for Sect. 4.3 and 5.4 have been changed from 'Impact of frontal melt on calving' to 'Impact of frontal melt and terminus geometry on calving', and additional discussion has been added to Section 5.4 to highlight the importance of terminus geometry for modelled calving

3. Then, to demonstrate the potential impact of tides in a coupled glacier-ocean model, you use the crevasse-depth calving criterion and look at changes in calving with and without tides (which you've already done). Given that the crevasse-depth calving criterion is not designed to model individual calving events, the most important result might be the impact of tides on average calving rates in a coupled model and not the timing of calving events (falling vs. rising tide, etc.)

The results from the different simulations are discussed both with regards to the timing of calving events and with regards to the average calving rate/ frontal ablation rate (Sections 5.3 and 5.6)

It's not clear to me whether changes of this magnitude would require additional simulations or if this can just be done through re-structuring of the current text and figures. There is also still the issue about the importance of elastic stresses over timescales of a few hours. I don't know how difficult it would be to set up a simulation that included elastic stresses, but it would be interesting to compare the tidal response of a viscous rheology to that of a viscoelastic rheology to one or two tidal cycles just to get a sense of the error in ignoring elastic stresses. Perhaps the papers by Christmann et al. and Mosbeux et al. would be helpful (they address a slightly different set up than this study)?

It is not possible within the time constraints of the project to run a viscoelastic model, but we have added extra discussion related to how these may impact the results. We mention that the lack of an elastic component may be responsible for the lack of tidal signal in the T_SP simulation and linked this to velocity changes.

I also have one specific comment related to Figure 2. Has the multibeam data been presented previously? It would be interesting to see how the entire terminus looks instead of just at a couple of profiles (If it has been published elsewhere, then that should be cited in Section 3.1.1.)

Currently, the two profiles presented in the manuscript have not been presented previously and so is not cited. In addition, there is no further publicly available data that can be used to show the entire terminus.

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REFERENCES ON TIDAL RESPONSE

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