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Response to reviewer two, Prof. Jason Amundson

We are grateful for all your comments on the manuscript, and have made many changes which we believe have led to considerable improvements. Please see below for responses to the comments.

The main changes in response to the comments are:

- New title, to reflect a focus on tides rather than frontal melt
- We have run three additional simulations to allow exploration of the impact of a different sliding law, the impact of a more gradual undercut geometry, and the impact of having no tidal fluctuations in the model.

The original comments are shown below in bold italics, with responses shown in normal typeface.

We hope you find the alterations satisfactory,

Yours Sincerely,

Felicity Holmes, on behalf of all authors

Major Comments

1. Basal friction: My major concern with the paper is the choice of using a linear friction law, which as I understand does not include effective pressure. Tidewater glaciers are typically close to flotation and therefore the basal shear stress is sensitive to small changes in thickness. Other modeling studies have suggested that tidal response should depend on effective pressure (e.g., Walters, 1989; see also Amundson et al., 2022). The closer a glacier is to flotation, the farther upstream a perturbation will be felt. It's possible that the modeled glacier is in a regime where that effect is small (compare to Fig. 6a in Amundson et al., 2022), but it will likely still affect the near-terminus stresses and the timing and magnitude of calving events. Effective pressure appears in the analysis of Amundson et al., which focused on nearly instantaneous stress changes from calving events, because of how changes in ice thickness propagate upstream. There is another effect that the authors don't consider which is that changes in sea level must affect the near-terminus subglacial water pressure. If the tides go up, the piezometric surface must adjust in order to continue to drive

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water out of the subglacial system and into the water. Thus, a rise in sea level results in a reduction in basal friction. I feel that an analysis of how tides affect flow and calving must take these things into consideration, which requires a friction law that depends on effective pressure.

Thank you for your comment about the choice of friction law. Our choice of a linear friction law was originally guided by results from Kronebreen by Vallot et al. (2017) who found that, whilst they were issues with a Weertman-type sliding law, this was a result of high spatio-temporal variation and meant that inverted friction fields from one season/year could not easily be applied to another season/year. We believed that, due to the short time period of our simulations (one month), using a friction field inverted from velocity observations from this same month would help circumnavigate these problems. However, Vallot et al. Found that errors were higher during the melt season (which we model) and, when taken in conjunction with your points about tides and effective pressure, we agree that a simulation with an effective pressure based sliding law would be an improvement. We therefore addressed the issue through two actions:

1) Conducting an additional simulation with a Coulomb type sliding law that includes effective pressure. This allows for an investigation of how sensitive the results from the model are to two different sliding laws. This goes hand-in-hand with the refocusing of the manuscript to look at how the model behaves (please see comments from Reviewer #3), rather than focusing on actual calving dynamics at Kronebreen. We believe that broadening the study to include the effects of different sliding laws on calving in the set-up is a valuable addition to the manuscript.

2) Add more discussion of the aforementioned points into the manuscript. In particular, a new section 'Model Limitations' was created

2. Undercut geometry: My understanding is that the melt parameterization that the authors use will essentially erode a "box" into the glacier with vary sharp corners. It at least produces a sharp overhang, as shown in the Figure 7, that appears sharper than observations presented in the paper and in other previous studies (e.g., Fried et al., 2015 and Sutherland et al., 2019). I worry that the sharp corners are producing especially high stresses and biasing the model results such that the undercutting appears to have a larger impact on calving than it might otherwise. One solution might be to test what happens when the boundaries of the plume are not so sharp — perhaps using some sort of gaussian melt profile, for example.

The undercuts in the manuscript are reasonably sharp, as a result of the simple melt parameterisations used. This can be seen in more detail by the new version of Fig. 7, made in response to comments from Reviewer 1 (and shown below).

Here, undercuts are shown from a plume area at the midpoint of the simulation, as well as from a non-plume area at the mid point. It is clear from these profiles that the undercuts are

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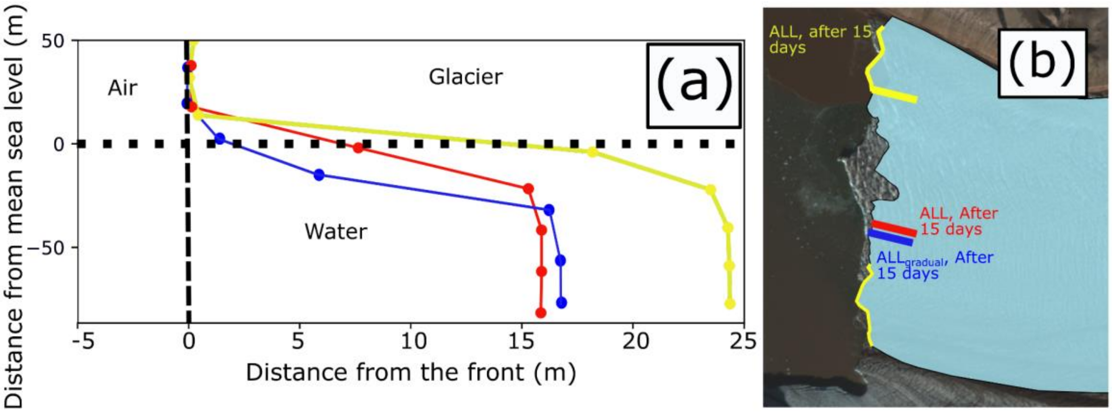
angular, and this may lead to an overestimation of calving. In order to investigate this in more detail, and to address your concern, we have:

- Run an additional simulation with a gradual undercut, achieved by having a submarine melt rate which increases from 0 at the waterline up to 500 m/yr at a depth of 50 m. The undercut from the midpoint of this simulation is also shown in the new Fig. 7.



By comparing the results from this simulation to the ensemble of results presented in the original manuscript, it is possible to see whether the modelled calving patterns (timing and total calving flux) are sensitive the geometry of the modelled undercut. This will additionally be discussed in more detail in the paper, with regards to other observations of calving front geometry. Unfortunately, we do not have the observational data from Kronebreen for the top c. 20 m of the submarine ice cliff and thus it is hard to directly compare the geometry just below the waterline between observations and model output. However, through this extra simulation, we can make some inferences of how sensitive the model is to this geometry.

Overarching Comments

3. Message: Maybe I was thrown off by the title, which suggests that this paper is investigating the impact of submarine melting on calving in general (like Mercenier et al., 2018 and Ma and Bassis, 2019), but it wasn't until I was pretty far into the paper that I realized that this paper is really primarily about the impact of tides on the timing of calving events. Submarine melting only really comes into the analysis because it acts to diminish the impacts of tides (at least according to the model). I suggest focusing the message of the paper more on tides, starting with the title.

Thank you for this observation; we propose to change the title to: 'Impact of tides on calving patterns at Kronebreen, Svalbard - insights from 3D ice dynamical modelling. This shifts the focus onto tides and also incorporates some of the comments by Reviewer #3 by stating that that these are model derived insights and not based on observations. In order to further address your comment, additional changes are made to the other sections of the manuscript, in particular the discussions.

4. Structure: I felt that the paper jumped back and forth between the model and observations too frequently, especially in Section 3. The paper is guided by observations, particularly glacier geometry and flow, which I appreciate. However, the authors are unable to make really detailed comparisons of the model output and data due to the nature of the modeling, and so the jumping back and forth just makes things a little confusing. Personally, I think a better approach would be to lay out the data, say that you are using it to motivate a modeling study, and then describe the model and model output. At a minimum, it would be worth considering how sections 3.2 and 3.4 are structured. Both include observational data.

We are grateful for your suggestion, and have moved around some material to better make the distinction between the observations which guide the study, and the results from the model. Specifically, a subsection ‘State of Kronebreen and surrounding areas during 2016’ has been created, which sets out the observational data and uses it to motivate the modelling component. This separates the observational data from the description of the model set-up. In addition, the results and discussions sections have been edited to keep improve the flow, with a greater focus on model behaviour and output, followed by a short discussion of how this relates to observations of real-life calving and associated metrics.

5. Model details: I think the model description needs more details. For example:

· How was the ice-ice boundary at the confluence of Kronebreen and Kongsvegen handled?

· Was the ice-rock boundary a no slip boundary?

· Did the friction law depend on effective pressure? (I think not; see comment above.)

· What was the width of the melt plume / region of submarine melting?

· Was there no ambient melting outside of that region?

The authors state that they are investigating several different tidal impacts in their, but the connection between tides and the impacts is not made clear. I would like to see them clarified. As I understand, the impacts they include are:

· sea pressure: this just relates to the stress on the glacier face, which has a direct impact on ice flow

· crevasse depth: Are you just saying that crevasses don’t have to penetrate as deep (in the crevasse-depth model) to produce a calving event when the water level is high?

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· frontal melt: Is there more frontal melt when the water is high because more water is in contact with the face? I think you need to be careful here because tides could also affect ocean heat transport toward and away from the glacier, which you are not modeling.

We have added in extra information on the model set-up, particularly with regards to how the different boundaries were treated (ice-ice, ice-rock etc). The friction law has been described in more detail, alongside its limitations. In addition, information about the Coulomb sliding law (now also used in the study) has been added in. The differences between the two sliding laws are set out in the new 'Model Limitations' section, and the differences in results generated as a result of the two sliding laws are discussed in the manuscript. Extra information and clarification about the extent of the plumes and the lack of any subaerial melt in the model-set up has been included.

The tidal impacts which we investigated are, indeed, the ones that you have mentioned. However, we are grateful for your feedback that this was not clear and have added in extra explanation and clarification of the impacts that tidal fluctuations have on the model. In addition, we have explicitly stated that we do not model ocean heat transport and so the possible impacts of tides on ocean heat transport to/from the glacier is not included.

6. Implications: I'm wondering if the authors can go farther with their discussion of the impact of tides on calving. Most of the focus is on the timing and location of calving events—which is itself interesting. But can the authors also say something about fluxes? For example, in the absence of submarine melting, how does the calving flux change if you turn the tides on or off? Is it important to include tides if you want to get the fluxes correct over longer time scales?

We have now also run a simulation with no tidal fluctuations, to investigate whether the overall frontal ablation/ calving flux is sensitive to the inclusion of tidal fluctuations. Additionally, we have compared the modelled frontal ablation rates to observations of frontal ablation rates at Kronebreen.