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Response to reviewer three, Prof. Douglas Benn

We are grateful for 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 are:

- New versions of Figs. 4 and 5
- A specific 'Model Limitation' section
- A re-focusing of the manuscript on model behaviour

In addition, 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, with the responses shown in normal typeface.

We hope you find the alterations satisfactory,

Yours Sincerely,

Felicity Holmes, on behalf of all authors

Technical and general comments

1. **First, I agree with the point made by Jason Amundson about the basal friction law used in the model. Indeed, a detailed study by Vallot et al. 2017 (J. Glac. 63,1012-1024) demonstrated that a linear friction law is inappropriate for Kronebreen, and indicates that a regularised Coulomb or similar would be a better approximation. Basal slip may well mediate glacier response to tidal fluctuations, with the implication that the model is missing an important process.**

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-

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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 the simulations (one month), using a friction field inverted from velocity observations from this same month would help circumnavigate these problems. However, errors were still found to be higher during the melt season (which we model) and, when taken in conjunction with points about tides and effective pressure (see also comments from Reviewer 2, Prof. Jason Amundson), we agree that a simulation with an effective pressure based sliding law would be an improvement. We have addressed this issue through two actions:

1) Run 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 different sliding laws. This goes hand-in-hand with the refocusing of the manuscript to look at how the model behaves, rather than focusing on actual calving dynamics at Kronebreen – done as a result of your suggestion and discussed in more detail under point 8). We believe that broadening the study to include the effects of different sliding laws on calving in the set-up will be a valuable addition to the manuscript.

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

2. **Second, on tidal timescales pressure fluctuations on the front will be propagated via elastic strain, not viscous deformation. This means that the decision not to implement the elastic stress solver in Elmer/Ice has potentially serious implications. It would be far better to conduct shorter runs with elasticity on than long runs with elasticity off.**

We agree that the lack of elastic strain related processes is a shortcoming of the model set-up, and raised this point in the discussion section of the original manuscript. Our argument was that we wanted to be able to simulate an entire tidal cycle (including both spring and neap tides), something which is difficult with a more expensive visco-elastic/elastic model. However, we see that more discussion of the limitations, as well as discussing the results with a greater emphasis on how the model works, would be an improvement to the manuscript. As such, the aforementioned ‘Model Limitations’ section makes the limitations of the viscous model clear, whilst a re-focusing of the manuscript ensures that the results are interpreted according to model behaviour.

3. **Third, Elmer/Ice respresents ice as a homogeneous, continuous medium, whereas Kronebreen is deeply crevassed in the terminal zone. Taken together with the point about elastic response, this means that ice response to tidal cycles will likely be dominated by brittle-elastic processes, such that the model will be incapable of representing processes likely key in triggering calving.**

We understand that Elmer/Ice models the glacier is a homogenous and continuous medium, and that the fact that Kronebreen is crevassed near its terminus can therefore lead to some

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problems. However, Todd et al. (2018) presented the Calving3D model in Elmer/Ice and stated that *'..This zero stress formulation ignores the yield strength which must be overcome to initiate fracture (Cuffey & Paterson, 2010), and we justify this on the basis that ice near the front of calving glaciers is already heavily fractured (i.e., extensional stresses propagate existing fractures)'.* As such, a heavily crevassed terminus such as that at Kronebreen is necessary to fulfil the assumptions of the calving model in Elmer/Ice. We have therefore included a discussion of the fact that a crevassed terminus is both necessary and potentially problematic in the 'Model Limitation' section.

4. **Fourth, calving is implemented in the model when crevasses penetrate (a) from the surface to the waterline or (b) the full thickness of the glacier, with crevasse depth calculated using a 'zero-stress criterion'. This is, of course, a great simplification of how crevasse penetration and calving actually works. Although Elmer/Ice predicts individual calving events, there is no reason to believe that it can be trusted to deliver reliable results at that scale. I believe that the CD law implemented in Elmer/Ice is the best method for modelling calving in a continuum model (by a long way), but I am also very aware of its limitations. CD in Elmer/Ice performs well in modelling overall ice-front position (such as the seasonal fluctuations Store Glacier modelled by Todd et al.), but it was not designed to predict individual calving events (HiDEM is much better suited to this).**

Thank you for this comment; we tried to avoid looking at any individual calving events due to the issues you mentioned above, but focused on broad trends derived from thousands of calving events (the mean number of calving events in each simulation was around 2000). We now use the 'Model Limitations' section to make issues such as this clearer in the manuscript, so that any given reader can easily understand what should/should not be read into the results (e.g. model behaviour vs glacier behaviour).

5. **Fifth, there is the issue of the ice temperature derived from model spin-up. Does this include firn warming by refreezing of meltwater? Firn warming is a very important process on larger Svalbard glaciers, making them some degrees warmer than they would otherwise be. Indeed, large Svalbard glaciers are often near-temperate, with only thin cold surface zones in their ablation areas and lower accumulation areas. This probably does not affect the model results in any material way, but it is worth reflecting on.**

We did not include firn warming by refreezing of meltwater, but have included a mention of this into the 'Simulation workflow' section, where the thermo-dynamical spin-up is discussed.

6. **The above questions about model formulation highlight an issue that runs through the paper. That is, the model results are taken at face value and discussed as though they provide insights into calving behaviour on the glacier, whereas there is no indication that calving does occur in the ways predicted by the model. If the paper also included a detailed time-series of calving observations (such as that presented by**

How et al. for Tunabreen), it would be possible to determine whether the modelled patterns mirror the real world. But as it stands, the paper shows us how the model behaves, not how the glacier behaves.

We agree that a corresponding time lapse data set would be beneficial, but regret that, to the best of our knowledge, this is not available. We instead now compare the mass loss in the model to previously published frontal ablation rates from a few different summer periods (as suggested by you in a later comment) alongside a refocusing of the paper on model behaviour.

7. **For both 'full-penetration' and 'waterline' criteria, calving depends on the stresses in the ice near the front; for the 'waterline' criterion, it additionally depends on the distance to the waterline. The considerations discussed above imply that the model will be rather insensitive to back-pressure fluctuations on tidal timescales, in turn suggesting that the modelled calving is mainly responding to (a) varying freeboard height (potentially encouraging calving at high tide, because crevasses do not have to penetrate as far before calving is implemented); and (b) undercut development, which modifies the tensile stresses at the surface. These effects may be sufficient to explain the observed patterns in the model output.**

Yes, it appears from the results that higher water levels and larger undercuts lead to more calving on the model. In some simulations, both of these effects can be seen (e.g. more calving events when the water level is higher, but also on a falling tide where we get a kind of 'max' undercut/ accumulated frontal melt). We propose to discuss these patterns in a greater detail, alongside how they relate to the model and the calving mechanisms.

8. **Despite these shortcomings, the paper contains much of potential interest about the behaviour of the Elmer/Ice calving model, and could be developed into a valuable contribution if it is reconfigured to emphasise that it is an exploration of model behaviour, with more circumspect discussion of it applicability to real glacier calving. In addition, considerable value could be added if model output was compared with bulk frontal ablation rates at Kronebreen, which are available for summer 2016 (Adrian Luckman has data covering this period). This would provide much-needed ground truthing against which model results could be evaluated.**

Thank you for your comments; we are grateful for the suggestions for improvement and the refocusing of the manuscript on model behaviour. In addition, as previously mentioned, we have now used observationally-derived frontal ablation rates as a way to evaluate the model-set up.

9. **The data on the submerged part of the Kronebreen glacier front are an important element of the paper. I understand there are good reasons why the shallowest 20m are not imaged, though unfortunately this is perhaps the most significant part of the submarine ice cliff. As noted in the review by Amundson, the form of the front just below the waterline has big implications for the stresses in the ice, and the style of**

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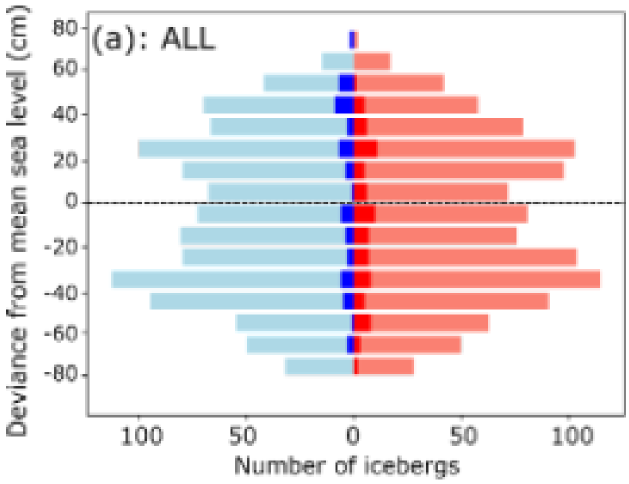
calving (see Slater et al., 2020), so it is to be hoped that future technical developments will allow this portion to be visualised. The authors may wish to discuss the prospects of this.

We have added in some discussion about how the use of uncrewed vehicles could allow for a greater proportion of the ice cliff to be imaged

10. **Figures 4 and 5: the rose diagrams contain a great deal of information, but I find them very difficult to read. This is because the top of the roses indicate both extreme highs and extreme lows, and one has to mentally 'unwrap' the diagram to grasp the patterns in the data. I think it would be better to present these data with a single vertical axis running from extreme highs (top) to extreme lows (bottom), and events during rising and falling tides shown as bars to the left and right of the axis, respectively.**

Thank you for your suggestion about Figs. 4 and 5, an issue that was additionally raised by Reviewer #1. We have re-made the figure in a similar way to your suggestion, and believe it to be much improved. An example of the new type of plot, shown here for the ALL simulation, is below:



11. **Figure 6 is puzzling. The caption tells us that calving events are indicated by dots and that the blue-shaded area represents a period of lower calving frequency. But the blue-shaded area contains a large number of dots, far more than in the pink-shaded area. This discrepancy needs to be resolved.**

Thank you for pointing this out, the text has been corrected to state that the blue shaded area denotes higher calving activity.