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Response to reviewers

The authors are grateful for all the reviews of 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 style for Figs. 4 and 5
- New simulations to investigate the influence of a different sliding law, the impact of a more gradual undercut geometry, and the impact of having no tidal fluctuations in the model
- A modified title to place the emphasis more clearly on tides and model behaviour
- A reworking of the discussions, to ensure there is a stronger focus on model behaviour and a clearer structure

Specific changes made in response to the reviewer comments are detailed below, split up by review. The original reviewer comments are written in bold, and the changes made are detailed in normal typeface. Line numbers in the reviewer comments refer to the original submission, whilst line numbers in our responses refer to the revised manuscript.

We hope you find the alterations satisfactory,

Yours Sincerely,

Felicity Holmes, on behalf of all authors

REVIEWER 1

Line comments

L15: Maybe Svalbard should also be mentioned here, as the paper is about Svalbard.

It has been clarified that ‘glaciers non-peripheral to Greenland and Antarctica’ includes Svalbard in L16.

L19f: Strictly spoken, "frontal ablation" also contains subaerial melt and sublimation at the calving front and should be mentioned for clarity (see Cogley et al. 2011, Glossary of Glacier Mass Balance, for details).

The definition of frontal ablation in the manuscript has been expanded to explicitly include subaerial melt and sublimation (L21).

L24ff: Maybe it would be worth adding Svalbard's tidewater glaciers that have shown retreat over recent decades, which implies substantial calving (e.g. Braun et al. 2011, doi:10.1111/j.1468-0459.2011.00437.x).

This point has been added in, alongside a reference to Braun et al. 2011 (L26).

L64: "supra-glacial melt" is rather odd, "surface melt" would be the right term (also to be corrected in the following)

All instances of supra-glacial melt have been changed to ‘surface melt’ (L66, L67)

Fig. 1: left panel: name the most important currents in the map; right panel: no needs for three decimals (one is enough)

The suggested changes have been made to the figure, with both the West Spitsbergen Current and the Sørkapp current explicitly labelled on the map. Only one decimal place is shown for the right panel. (Fig. 1, page 4).

L105: I'm not sure if this is the right location, but in any case it needs to be noted that you do not consider subaerial frontal melt/sublimation in your modelling.

This has been clarified in the ‘Model Inputs’ section, where the forcings (e.g. submarine melt) are discussed (L162 – 163)

L106-109: Additional information about e.g. mean velocities and errors/uncertainties of the calculated velocities must be presented here. Given the nature of the study, velocities at the front should be given special attention.

Additional details have been given relating to the velocity fields, for example the estimation of the errors for frontal velocity (L120 – 123).

L179ff: The position should be mentioned from where the profiles were measured (maybe also indicate them in Figure 1).

Information on the location from which the profiles are measured has been added in to the Methods section. The multibeam data was taken from a research vessel in Kongsfjorden, and the LiDAR data was taken from the shore at 78°52’04.1"N 12°29’06.7"E (L91 – 94).

Table 2: The term "Large icebergs" should be quantified somehow in the caption so that it can directly be distinguished from "All icebergs".

The definition of large icebergs has been added to the caption for Table 2 (Table 2, Page 11).

Fig. 4b/c: I find it very hard to distinguish between "All size" and "Large" in those figures. The presentation of the data should somehow be changed so that a straightforward differentiation is possible. I also think that this kind of plot is inadequate to visualize the results, as +80 and â€ ‘ 80 cm are located in direct vicinity. A linear bar plot would be correct instead (it would also solve the problem that lines overlap frequently for small numbers of icebergs, which is in parts responsible for the problematic readability of the graphs).

The figure has been re-made so as to aid readability, with the new fig shown below and visible on Page 12.

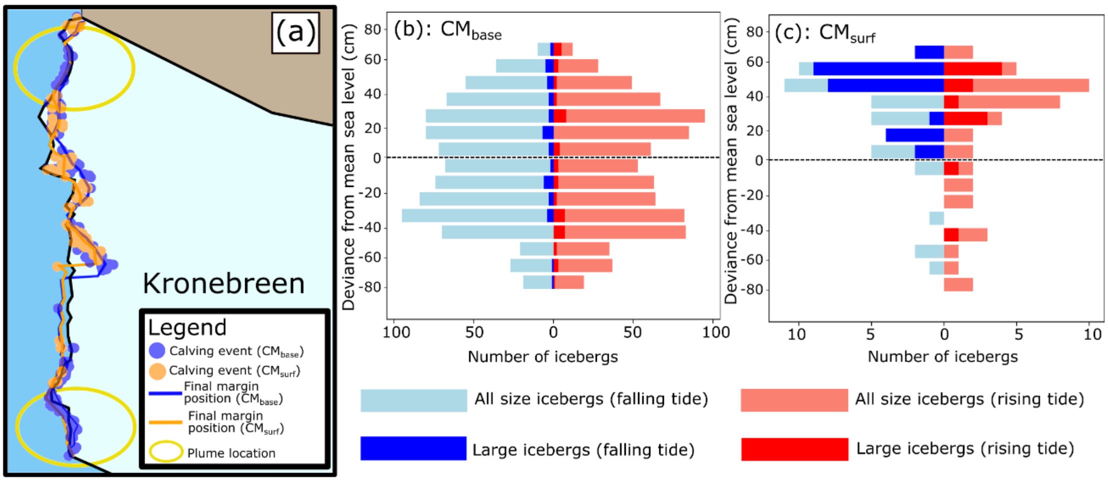
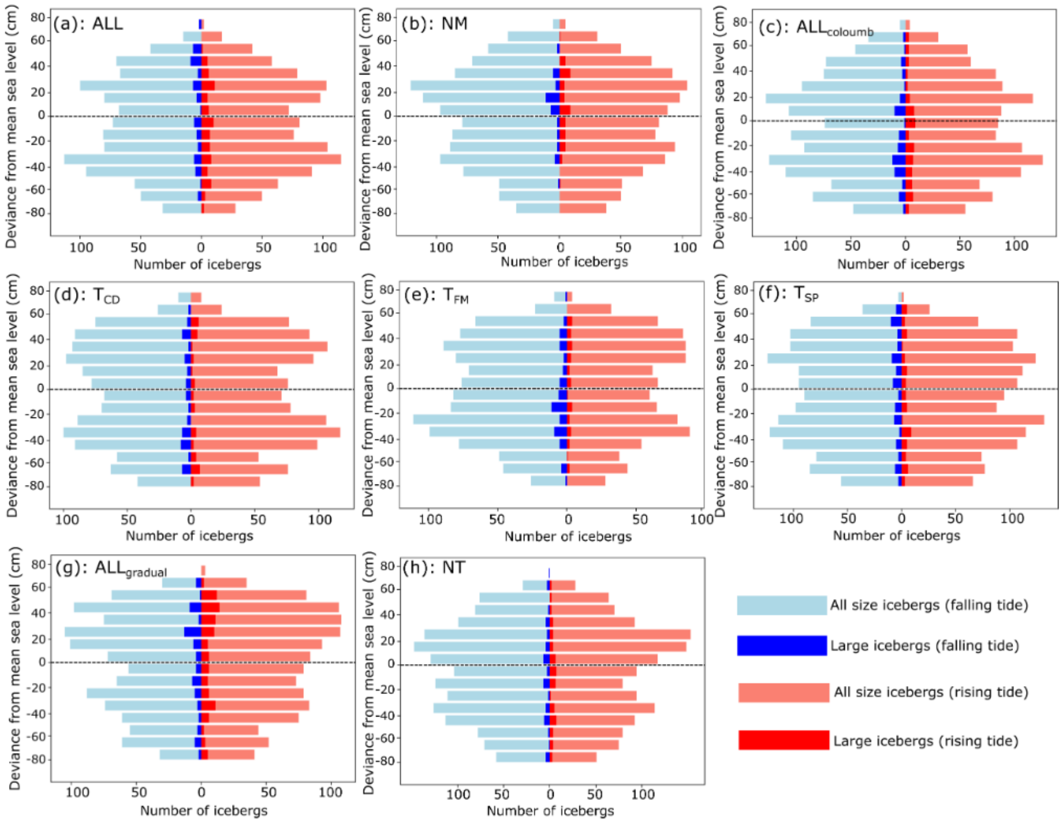


Fig. 5: Same comments as for Figure 4. This needs to be changed here, too.

The plots in Fig. 5 have been changed in the same way as has been done for Fig. 4 and is shown below, as well as on Page 13 of the revised manuscript.



L264: correct to "... an impact ..."

The correction has been made but, due to the reworking on the discussions, the sentence is now on L403.

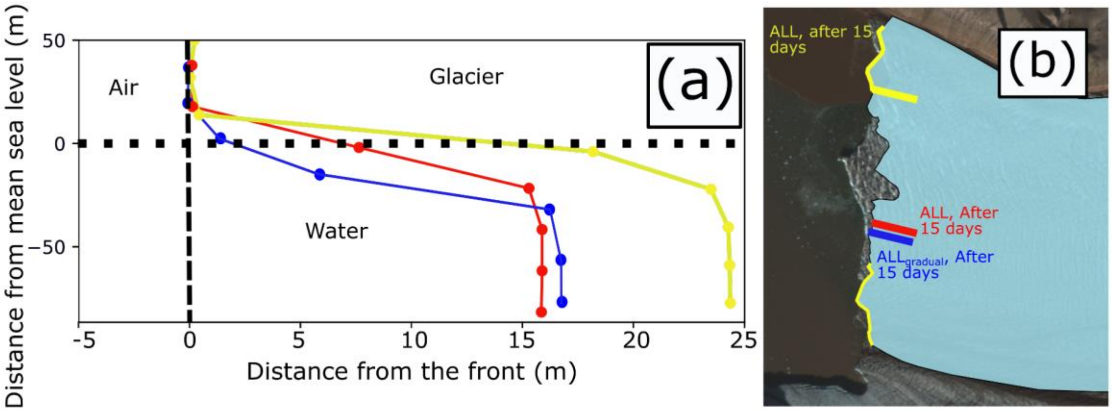
Discussion section: It might be worth taking a look at another study that analyzed calving at Kronebreen (Sund et al. 2011, TCD, https://tc.copernicus.org/preprints/tc-2010-104/), even if the paper was not accepted for final publication. Maybe it gives some additional insights into the discussion.

Thank you for this suggestion and link to the article

Fig. 7: I have some problems with this figure, too: While I like the idea of showing the profiles in perspective, I think this makes visual comparison of the undercut sizes almost impossible. In (a) sizes are given for profiles 2 and 3. It appears that the undercut in profile 2 is about four to five times as large as thin in profile 3, but this is by no means supported by the values given. Moreover, it is strange that profiles 1 and 4 are shown with their numbers, while undercut values are given for profiles 2 and 3. I suggest to change the presentation of the profiles in (a) to individual x (distance from calving front)

vs. y (depth) line graphs. This would allow the reader to get the right idea of the sizes of the undercuts.

The figure has been edited so that it is now a line graph, which we believe makes it much easier to see the sizes of the undercuts, as well as to compare them to the observed undercuts. Undercuts are shown from a plume location (midway point of the simulation), and a non-plume location (midway point of the simulation). Additionally, an undercut from the new 'ALLgradual' sim is shown, where a less angular undercut is modelled. The updated figure is shown below, and is found on Page 15 of the revised manuscript.



L345 "We note..." instead of "The authors of this paper note..." ?

Here, we refer to the authors of Vallot et al., rather than to ourselves. This is not clear in the original text and we have changed it to read: 'However, Vallot et al. (2018) note…' on L466.

L390: It needs to be discussed to which extent the fact that subaerial frontal melt was not considered in the model (only one single simplified overall frontal melt), has an impact on the results. I mean, frontal melt rates are different below and above the water line, which clearly also impacts the creation of undercuts.

We have added in some discussion about the lack of subaerial melt/sublimation in the model set up, and how this may impact modelled undercut sizes (L343 – 346).

REVIEWER TWO

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 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. Information and discussion regarding this new simulation is detailed on L152- 156, L200-201, L228-229, Table 1 (page 11), Fig. 5 (Page 13), Sect. 4.4 (L305-312), L352-356, and Sect. 5.5 (L471-482).

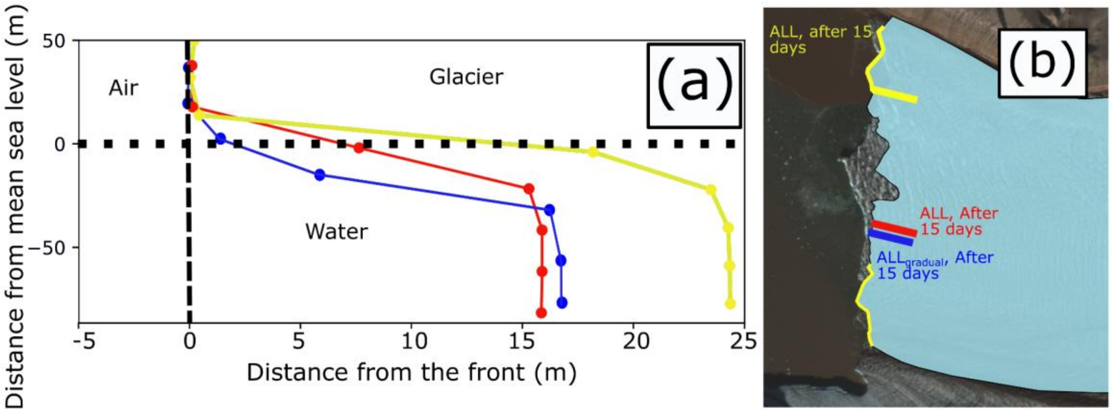
2) Add more discussion of the aforementioned points into the manuscript. In particular, a new section ’Model Limitations’ was created (Sect. 5.1 [L320-356])

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 found on Page 12 of the revised manuscript, as well as 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 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.

Specifically, information and discussion surrounding the undercut geometry and the new simulation can be found on L230-231, Table 2 (Page 11), Fig. 5 (Page 13), Fig. 7 (Page 15), L298-299, L346-351, L442-463.

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 have modified 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, where sections are created to explicitly discuss the impact of tides (Sect. 5.3, L371-406) and frontal melt (Sect. 5.4, L408-470)

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 (Sect. 3.1.2, L115-130).

This separates the observational data from the description of the model set-up. In addition, the results and discussions sections have been edited to 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?

· 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) – see L148-150.

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 (L150-155).

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. (Sect. 5.1, L320-356).

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 (L162-164, L179-184).

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. (L165-172).

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. Information about these new aspects can be found on L231, Table 1 (Page 10), Table 2 (Page 11), L377-379, Fig. 5 (Page 13), Sect. 5.6 (L484-493).

REVIEWER THREE

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-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. Information and discussion regarding this new simulation is detailed on L152- 156, L200-201, L228-229, Table 1 (page 11), Fig. 5 (Page 13), Sect. 4.4 (L305-312), L352-356, and Sect. 5.5 (L471-482).

2) Add more discussion of the aforementioned points into the manuscript. In particular, a new section 'Model Limitations' has been created. (Sect. 5.1 [L320-356])

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 (Sect. 5.1 [L320-356], broad changes to the discussions [L313-493]).

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 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 (Sect. 5.1 [L320-356]), as well as in the Methods (L138-140).

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 (Sect. 5.1 [L320-356]) 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). Additional adjustments to place emphasis on how the results should be interpreted are made in the Title, L85-86, L314-318, and L495-504.

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 (L206-207).

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 (L239-240, L248-249, L277-279, L290-291, and Sect. 5.6 [484-493]).

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 (L360-364, L378-379, L420-431, L496-499).

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 (L239-240, L248-249, L277-279, L290-291, and Sect. 5.6 [484-493]).

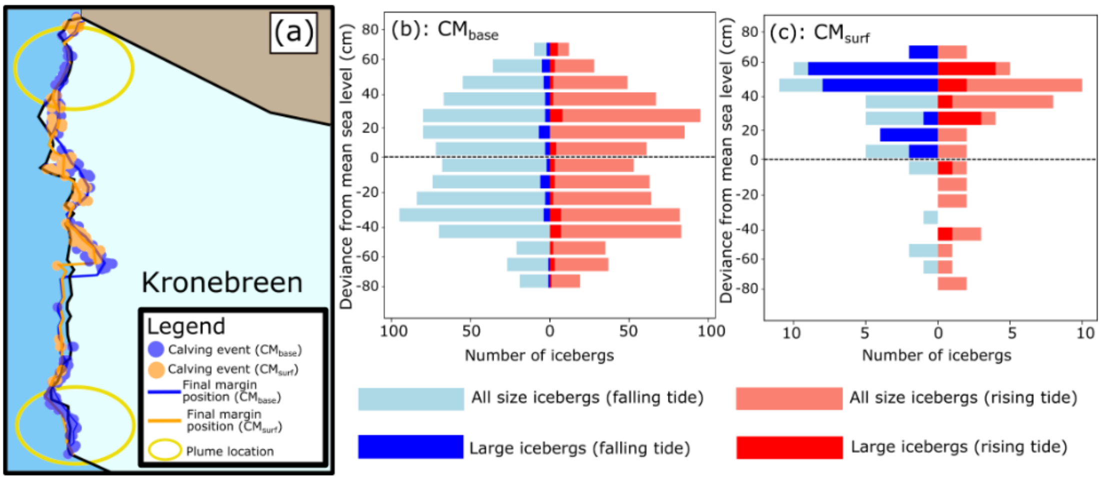
9. **The data on the submerged part of the Kronebreen glacier front are an important element of the paper. I understand there are good reasons why the shallowest 20m are not imaged, though unfortunately this is perhaps the most significant part of the**

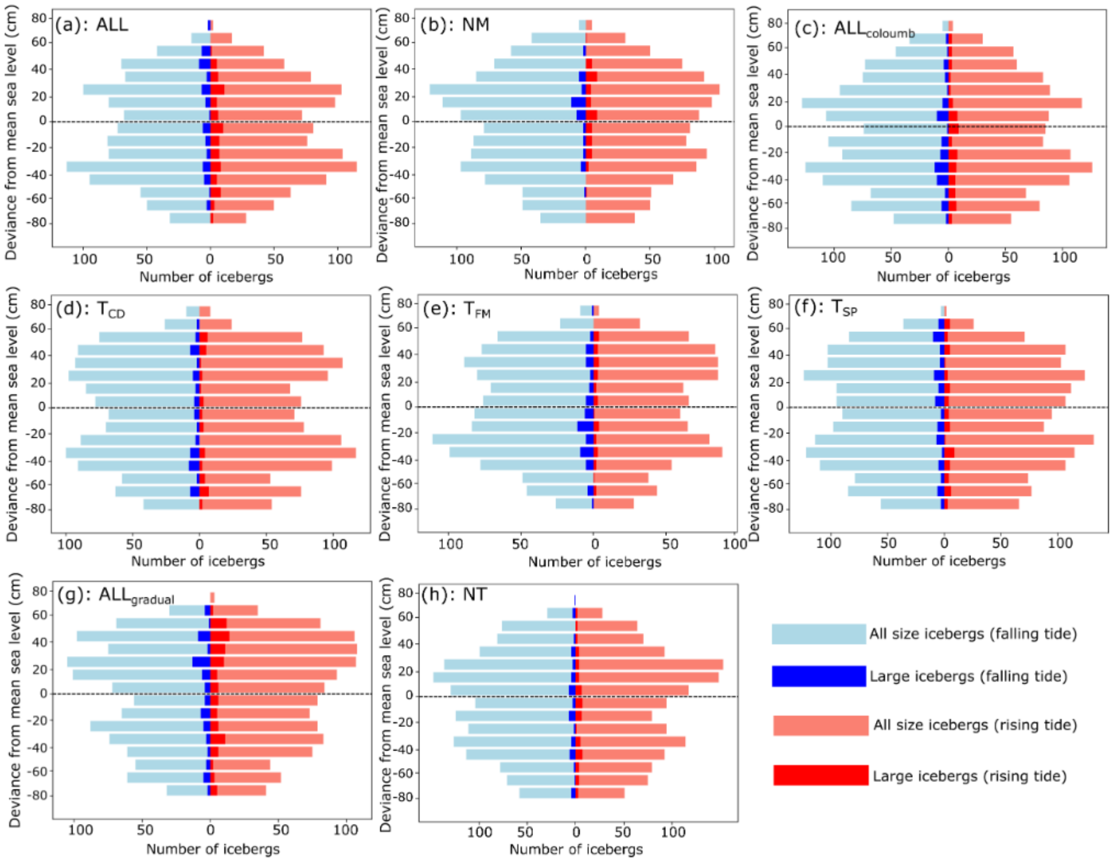
submarine ice cliff. As noted in the review by Amundson, the form of the front just below the waterline has big implications for the stresses in the ice, and the style of calving (see Slater et al., 2020), so it is to be hoped that future technical developments will allow this portion to be visualised. 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 (L452-454).

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. The new figures are shown below and are also seen on Pages 12 and 13 of the revised manuscript:





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 (Fig. 6, Page 14).

REVIEWER FOUR

Technical and general comments

1. **My first recommendation is one that I always provide for all numerical studies. I humbly suggest that the authors consider a numerical convergence study with different element sizes and time step sizes. A few years ago, Brandon Berg and I ran into some subtle issues with the standard Elmer Ice implementation of no-penetration boundary conditions (Berg and Bassis, 2020). The effect was subtle and only manifested itself after re-meshing when we removed calved blocks of ice.**

> **However, the fix that we proposed was (I think?) incorporated into Elmer-Ice. Nonetheless, an important lesson for us based on that is to always do numerical convergence studies to make sure things behave as expected.**

Thank you for raising this issue. We have indeed run the same set-up with different time step sizes and element sizes, and not seen any noticable impacts on the solution suggesting that it has converged. However, many of these runs were conducted as part of a manuscript that is currently in preparation - also focusing on Kronebreen - and where we could address the convergence issue in slightly more detail. We also believe the fix to be implemented in Elmer/Ice, through reintroduction of the acceleration term in the force balance. This, too, could be a valuable addition to include in our forthcoming manuscript.

2. **The study we were trying to do when we discovered the numerical artifacts was to see if advection of crevasses was important in the process (Berg and Bassis, 2022). The Nye zero stress crevasse model assumes that glaciers have no fracture memory and that if a crevasse cannot form the detachment boundary of an iceberg, crevasses immediately close leaving no trace. When we look at glaciers, we clearly see crevasses have initiate upstream and propagated downstream. Where this is relevant is because, as other reviewers pointed out, the stress near the calving front depends on the shape of the imposed melt profile along the calving front, a small amount of crevasse advection from just upstream of the calving front could have a significant effect on the predictions any crevasse depth model. One of the conclusions from Berg and Bassis, 2022 was that advection *sometimes* mattered. The fact that advection *might* be important could be a worthwhile caveat because I wonder if you will end up with slightly different conclusions is you include advection and/or different melt profiles.**

Thank you for this comment; it is definitely interesting to consider how crevasse advection may impact the model results. As you stated, the calving model in Elmer/Ice assumes no fracture memory, but also ignores the yield strength required for fractures to initiate. We have added some discussion of this into the manuscript (Sect. 5.1, L335-336).