

Interactive comment on “Effects of undercutting and sliding on calving: a coupled approach applied to Kronebreen, Svalbard” by Dorothee Vallot et al.

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We first want to thank the referee for the constructive comments. Our answers to the questions are as follows.

Major comment:

The title of this paper and subsequent references to “coupling” are misleading. There is no coupling performed in this model. The different models used to represent the calving front processes are put together and outputs from one model are inputs of other models, but there is no coupling. Fig. 2 illustrates this very

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well: arrows all have the same direction and outputs from HiDEM are never used as inputs for other models. What this paper does is provide a comprehensive approach to the question of calving, and I think a title using “global approach” or something similar would be more accurate.

This is true and that is why we referred to it as a one-way coupling throughout the text. However, and the referee is right, the title is misleading and we change it to the suggestion: "Effects of undercutting and sliding on calving: a global approach applied to Kronebreen, Svalbard"

As mentioned above, the conclusions separating the impact of geometry, basal sliding and undercutting are not well supported by the results provided. Looking at Fig. 10, it seems that all parameters have an impact on both the location and extend of retreat, but they cannot be clearly distinguished without further experiments. It is true that more experiments are always better and we understand that the conclusion should acknowledge the fact that they are only valid for these cases. However, we do not agree with the statement in the summary "the impact of the basal friction on Fig. 10 (a and c only show the impact of friction) is not clear, as the calving rate is not very different for the high and low friction scenarios, and adding undercutting (Fig. 10c) has an impact similar to changing basal friction." Fig. 9 shows that there are large differences between calving rates when geometry is kept unchanged and basal sliding is changing. Of course more experiments would give a better picture but should be made in a smaller scale so that the cost of running the HiDEM is not as high. We have rewritten the discussion and the conclusion to express our results in a clearer way:

In the discussion:

"Because the imposed undercuts are the product of melt during the whole interval between observations, the model results should be treated with caution. Benn et al. (2017) compared HiDEM calving for specified undercuts of different sizes and showed that calving magnitude increases with undercut size. For small undercuts, calving

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simply removes part of the overhang, but for large undercuts calving removes all of the overhang plus additional ice. The mechanisms are different in each case: low-magnitude calving for small undercuts occurs through collapse of part of the unsupported overhang, whereas high-magnitude calving for large undercuts involves forward rotation of the whole front around a pivot point located at the base of the undercut cliff. The long time-step intervals (11 or 18 days) between the starting geometry and the HiDEM simulation in the present study might therefore bias the results towards higher calving events. Testing this possibility is beyond the scope of the present paper, but remains an important goal for future research. Despite this caveat, our results compare remarkably well with observations, and yield valuable insights into the calving process.

Firstly, the HiDEM results show that undercutting associated with meltwater plumes is an essential factor for calving during the melt season (t_4 and t_6). Surface melt leads to the formation of a subglacial drainage system that ultimately releases the water into the ocean from discharge points at the front of the glacier. Simulations without frontal undercutting at these subglacial discharge locations do not agree well with observed frontal positions and mean volumetric calving rates. In contrast, simulations with frontal undercutting reproduce the retreat reasonably well at these locations, particularly where the discharge is high such as at ND. The largest discrepancy between modelled and observed calving is in the region south of SD at t_4 . Here, the model predicts calving of a large block, whereas the observed front underwent little change. This largely reflects the rules used for calving in HiDEM: any block that is completely detached from the main ice body is considered as calved, even if only separated by a narrow crack from the rest of the glacier and still sitting at its original position. This is the case for the large 'calved' region south of SD at t_4 , where the block was completely detached but remained grounded and in situ. If this were to occur in nature, it would not register as a calving event on satellite images. The discrepancy between model results and observations at this locality therefore may be more apparent than real.

Secondly, the model results replicate the observed high calving rates at t_{11} , after the

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end of the melt season when there is no undercutting. At this time, the observed mean volumetric calving rate is $24.99 \times 10^5 \text{ m}^3 \text{ d}^{-1}$, which compares well with the HiDEM rate of $28.50 \times 10^5 \text{ m}^3 \text{ d}^{-1}$. These values are much higher than those at the start of the melt season, when there is also zero undercutting. This contrast can be attributed to the high strain rates in the vicinity of the ice front at t_{11} , which would encourage opening of tensile fractures (Fig. 11). In turn, the high strain rates result from low basal friction (Fig. 5d), likely reflecting stored water at the glacier bed after the end of the melt season. It is possible that geometric factors also play a role in the high calving rates at t_{11} , because the mean ice front height is greater at that time than at t_0 , reflecting sustained calving retreat during the summer months, which would have increased longitudinal stress gradients at the front (Benn et al., 2017). This interpretation is supported by experiments $C(g_0, \beta_6, 0)$ and $C(g_6, \beta_0, 0)$, in which the basal friction values are transposed for non-undercut ice geometries at t_0 and t_6 . Imposing low friction (β_6) at t_0 produces mean volumetric calving rates similar to (but smaller than) those observed at t_6 , whereas imposing high basal friction (β_0) at t_6 produces low volumetric calving rates similar to those observed at t_0 . The influence of basal friction on calving rates is consistent with the results of Luckman et al. (2015), who found that a strong correlation exists between frontal ablation rates and ice velocity at Kronebreen when velocity is high. Low basal friction is associated with both high near-terminus strain rates and high velocities, facilitating fracturing and high rates of ice delivery to the front. Our experiments do not include varying fjord water temperature, so we cannot corroborate the strong correlation between frontal ablation and fjord temperature observed by Luckman et al. (2015). However, our results are consistent with their finding that melt-undercutting is a primary control on calving rates, with an additional role played by ice dynamics at times of high velocity."

In the conclusion:

"Two factors impacting glacier calving are studied here using HiDEM: i) melt-undercutting associated with buoyant plumes; and ii) basal friction, which influences

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strain rates and velocity near the terminus. The performance of the calving model is evaluated quantitatively by comparing observed and modelled mean volumetric calving rate and qualitatively by comparing calved regions. Results show that modelled calving rates are smaller than observed values during the melt season in the absence of melt-undercutting, and that there is a closer match with observations if undercutting is included. Additionally, there is good agreement between modelled and observed calving before (t_0) and after (t_{11}) the melt season, when there is no undercutting. Both modelled and observed calving rates are much greater after the melt season than before, which we attribute to lower basal friction and higher strain rates in the near-terminus region at t_{11} . The influence of basal friction on calving rates is corroborated by model experiments that transposed early and late-season friction values, which had a large effect on modelled calving. These results are consistent with the conclusions of Luckman et al. (2015), that melt-undercutting is the primary control on calving at Kronebreen at the seasonal scale, whereas dynamic factors are important at times of high velocity (i.e. low basal friction).

In this paper, we have shown that one-way coupling of ice-flow, surface melt, basal drainage, plume-melting, and ice-fracture models can provide a good match to observations and yield improved understanding of the controls on calving processes. Full model coupling, including forward modelling of ice flow using a physical sliding law, would allow the scope of this work to be extended farther, including prediction of glacier response to atmospheric and oceanic forcing."

It is not clear if all the 11 time steps described in Tab. 1 are modeled, or if only a subset of these times are used. Results from t_0 , t_4 , t_6 and t_{11} are mostly presented, but Fig. 10 also shows results at different time steps.

The 11 time steps in Table 1 are used to model undercutting. We use all observations/modelled data (front position/runoff) to assess the undercutting for each time step. Thereafter, when using the particle model, we only model four time steps (t_0 , t_4 , t_6 and t_{11}) for their particularity as the comment in Table 1 shows.

To explain our strategy clearly, we changed a whole paragraph in this section based on the line by line comments which can help readers to understand how each model is separated:

"First, we infer sliding at each time step from surface velocities using an adjoint inverse method implemented in Elmer/Ice with an updated geometry from observations at different time steps. At each iteration, i , corresponding to an observed front position, F_i^{obs} , the front and the surface are dynamically evolved during the observation time (roughly 11 days) with Elmer/Ice. By the end of the time step, the front has advanced to a new position, F_{i+1}^{elmer} . Here we use $i + 1$ because this is the position the front would have at t_{i+1} in the absence of calving. Second, given subglacial drainage inferred from modelled surface runoff, a plume model calculates melt rates based on the subglacial discharge for each iteration, which are subsequently applied to the front geometry at subglacial discharge locations. At each iteration, the front geometry takes into account the undercut modelled at the former iteration. Finally, the sliding, geometry and undercut (when applicable) are taken as input to the calving particle model HiDEM for each iteration and a new front, F_{i+1}^{hidem} , is computed for four iterations, t_0, t_4, t_6, t_{11} , which represent interesting cases (see comments on Table 1). More details about each aspect of the model process are given in the following sections."

What is the rationale for keeping or removing the undercut in one case or another when the ice front advances or retreats (Fig. 4 and p.9)? Some explanations justifying these choices should be added as opposed to presenting the choices made without any justification. I cannot quite figure out why the undercut from the previous profile is not always considered.

The idea is to take into consideration the observations. One should keep in mind that the undercut estimation is done independently from the HiDEM simulations. The first iteration undercut is estimated from the advanced front, F_1^{elmer} (advanced from F_0^{obs}), by projecting daily melt rate during the time period t_1-t_0 . At the second iteration, t_2 , we know where the front would be if there had not been any calving between t_1 and t_2 :

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F_2^{elmer} , which is the advanced front from the observed position at t_1 , F_1^{obs} . So we can transfer the whole undercut from previous iteration to F_2^{elmer} if F_1^{obs} is situated in front of F_1^{elmer} (case a). Otherwise, the undercut would have been fully or partly calved away (case b and c). We then apply the new undercut on this new geometry given the melt rates between t_1 and t_2 .

We changed the text in consequence:

"When the first discharge occurs, the melt rate calculated with the plume model in 2D is summed for the period of time between t_0 and t_1 and projected to the advanced front $F_1^{elmer}(z=0)$ (advanced from $F_0^{obs}(z=0)$) at the location of the subglacial outlets and ice is removed normal to the front. This yields a new position of the front at depth z below sea level called $F_1^{elmer}(z)$. At the second iteration, t_2 , we know where the front would be if there had not been any calving between t_1 and t_2 : $F_2^{elmer}(z=0)$, which is the advanced front from the observed position at t_1 , $F_1^{obs}(z=0)$. So we can transfer the whole undercut from previous iteration to $F_2^{elmer}(z)$ if $F_1^{obs}(z=0)$ is situated in front of $F_1^{elmer}(z)$ (see Fig. 4b–c). Otherwise, the undercut would have been fully or partly calved away (see Fig. 4b–c). We then apply the new undercut on this new geometry given the melt rates between t_1 and t_2 .

At time t_i , the modelled front position at depth z (advanced by Elmer/Ice from the observed front position at t_{i-1}) is $F_i^{elmer}(z)$ and the observed front position is $F_i^{obs}(z=0)$. We advance this observed front with Elmer/Ice during $\Delta t = t_{i+1} - t_i$ to obtain the front position $F_{i+1}^{elmer}(z=0)$ at t_{i+1} . We want to determine $F_{i+1}^{elmer}(z)$ and depth z given the melt rate calculated between t_i and t_{i+1} and the state of the undercut from the previous front $F_i^{elmer}(z)$ updated by the observed front $F_i^{obs}(z=0)$."

Line by line comments:

p.1 l.17: "rigorous methods": the problem is not so much about rigorous methods but more about some processes impacting calving that we still don't understand, as well as small scale features (mm long cracks) that cannot be observed

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and included in models.

We change to:

"To a large degree, this uncertainty reflects the limited understanding of processes impacting calving from tidewater glaciers and ice shelves, and associated feedbacks with glacier dynamics. In particular, calving occurs by the propagation of fractures, which are not explicitly represented in the continuum models used to simulate ice flow and glacier evolution."

p.1 I.20: "impacting on submarine melt rate" → "impacting submarine melt rate"

Done!

p.2 I.1: "during the summer and the autumn" → "during summer and autumn"

Done!

p.2 I.3: "followed by ice-front collapse": not clear

We change to: "triggering collapse of the ice above"

p.2 I.12: The problem is actually not so much the representation of calving in models but the processes impacting calving that are not enough understood and therefore cannot be included into models.

We change to: "In addition to the lack of process understanding, continuum models cannot explicitly model fracture, but must use simple parameterisations such as damage variables or phenomenological calving criteria."

p.2 I.16: Again here, it is not really coupling but feeding the particule model with appropriate inputs from Elmer/Ice.

This is true and that is why the word "introduce" is used because it is just an introduction. But we agree that it is misleading. Therefore, we change to:

"In this paper, we use both the capabilities of the continuum model Elmer/Ice and the

discrete element model HiDEM."

p.2 I.32: "one of the fastest" → "one of the fastest glacier"

Done!

p.2 I.33: How much seasonal variation is this glacier experiencing?

See question below.

p.3 I.3: How large are the seasonal variations? What is the velocity in winter?

We add more information: "In 2013, averaged velocities close to the front ranged from 2.2 to 3.8 m d⁻¹ in the summer and fell to 2 m d⁻¹ directly after the melt season. In 2014, however, they stayed relatively high (around 4 m d⁻¹) throughout the summer and progressively fell to 3 m d⁻¹ in the winter."

p.3 Fig.1: Consider adding Kongsfjorden on the figure. Calving front position for 16 Octobre 2013 is not visible, consider changing it.

Done!

p.3 I.10 "by (Nahrgang et al., 2014)" → "by Nahrgang et al. (2014)"

Done!

p.3 I.11-15: Past tense should be used to describe measurements made in 2013.

Done!

p.3 I.11-15: How representative of the seasonal cycle are these values?

These values are only valid for the summer season.

p.4 Fig.2: There is no feedback and therefore no coupling shown on this figure. Outputs from one model are used as parameters/inputs for the next model.

We therefore refer to it as a one-way coupling approach. We change the first sentence

of the section though to:

"We use surface velocity and frontal position data described above to test the effects of sliding and undercutting on calving using different models in a global approach."

p.4 I.7: What about the other observations (geometry, ice temperature and viscosity)? Where do they come from?

Geometry was described in section 3.3 but we agree that all observations should be presented together so we moved it and changed the title of the section to "3.1. Observed geometry, surface velocities and front positions". There is no observation of ice temperature or viscosity and more details on the Elmer/Ice model description is given in Vallot et al. (2017), cited in section 3.3. To be clear, we added this information: "More details on the model (viscosity, ice temperature, inversion time-steps, etc.) are given in Vallot et al. (2017)."

p.5 I.6: Is the sliding inverted just at the beginning of the simulation or recomputed for each time step? In this case how is the change in the glacier geometry computed (or maybe observations are used)?

The sliding is inverted at each time step given the observations (velocity, topography, front position). Thereafter the surface elevation is relaxed using Eq. 2 in a transient simulation. This new surface is then used in the next iteration. This is done independently of the HiDEM and is only using front position observation. This is what should change in a full coupling. To avoid confusion, we add "at each time step".

p.5 I.9: How are the front and surface evolved? Are they run from the previous iteration and therefore the 12 time steps are run with the model? In this case, why only show results for 6 cases and not the entire melt season? If not, how are the front and surface evolved? Also, modeling ice front changes in ice flow model is not an easy task and is currently the subject of active research. How is the front evolved with the Elmer/Ice model? There is no reference or explanation of

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how the ice front migrates and no Elmer/Ice paper describing such an evolution to my knowledge. This has to be better explained.

The front and surface are evolved in a transient simulation using Elmer/Ice, as explained in the following section (3.3). We add more information on the front evolution:

"The front is advanced by imposing a Lagrangian scheme over a distance equal to the ice velocity multiplied by the time step. We do not account for the melting during the advance because we only have observations at the beginning and the end of each timespan. Instead, we lump frontal melting by applying an undercut after the advance as explained hereafter."

Since it is not a full coupling, the results from the HiDEM are not used in Elmer/Ice where we only use front position observation. This section was intended to be more a presentation of the modelling concept with more explanation to follow. Is it misleading? We add a sentence referring to the next paragraphs for more details:

" More details about each aspect of the model process are given in the following sections."

Also, the sentence about the front and surface evolution is misleading since it seems that the front evolves from one observation to another, which is not the case. So we change it to:

"the front and the surface are dynamically evolved during the observation time"

p.6 I.1: "coupling": same as above

Here we explain that we use "one-way coupling" with input from a model is output from another but not vice-versa.

p.6 I.1: If the front position is not used as inputs for the Elmer/Ice initial front position, what is used then?

We use the observed frontal position for Elmer/Ice simulations. We add this informa-

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tion: "In Elmer/Ice, we use the observed frontal positions."

p.6 Eq. 1: Consider using vectors. Also u is used both here for the velocity, and later (e.g. Tab. 2) for the undercutting. Change one or the other.

Thank you for this suggestion! We chose to change the velocity to v and we change to vectors.

p.6 I.8: Again here, is the friction optimized at each time step or just at the beginning of the simulation?

The friction is optimised at each time step for which we have observations. We add this information: "The basal friction coefficient, β , is optimized at each time step to best reproduce observed velocity distribution at the surface of the glacier as described in Vallot et al. (2017)."

p.6 I.9: "the self-adjointness" \rightarrow "a self-adjoint algorithm"

Done!

p.6 I.10 and I.11: Consider adding older references that first used such methods.

We add references:

"This is done by using a self-adjoint algorithm of the Stokes equations for an inversion (e.g. Morlighem et al., 2010; Goldberg and Sergienko, 2011; Gillet-Chaulet et al., 2012) and implemented in Elmer/Ice (Gagliardini et al., 2013)."

p.6 I.13: This paragraph could be put in the data section (section 3.1) to improve consistency.

Done!

p.6 I.20: "The front position is also able to advance": How is it able to advance? See point above

As described above, we add the information: "The front is advanced by imposing a

Lagrangian scheme over a distance equal to the ice velocity multiplied by the time step."

p.6 I.20: " $F_i^{obs}(0)$ ": I would imagine that observations show the front position on the surface of the glacier and not at sea level.

We assume that the front is vertical above sea level. We add this information: "We assume that the front is vertical above the sea level so that the observed front position (at the surface of the glacier) is the same at sea level."

p.6 I.20-21: There are several front positions observed and computed. The authors should start by listing all the front position computed (F^{elmer} , F^{HiDEM} , ...) and explaining where they come from. That might be something to add on Fig.2.

Thank you for the suggestion! We added this information in the section 3.2:

"First, we infer sliding at each time step from surface velocities using an adjoint inverse method implemented in Elmer/Ice with an updated geometry from observations at different time steps. At each iteration, i , corresponding to an observed front position, F_i^{obs} , the front and the surface are dynamically evolved during the observation time (roughly 11 days) with Elmer/Ice. By the end of the time step, the front has advanced to a new position, F_{i+1}^{elmer} . Here we use $i + 1$ because this is the position the front would have at t_{i+1} in the absence of calving. Second, given subglacial drainage inferred from modelled surface runoff, a plume model calculates melt rates based on the subglacial discharge for each iteration, which are subsequently applied to the front geometry at subglacial discharge locations. At each iteration, the front geometry takes into account the undercut modelled at the former iteration. Finally, the sliding, geometry and undercut (when applicable) are taken as input to the calving particle model HiDEM for each iteration and a new front, F_{i+1}^{hidem} , is computed for four iterations, t_0, t_4, t_6, t_{11} , which represent interesting cases (see comments on Table 1). More details about each aspect of the model process are given in the following sections."

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F^{obs} and F^{elmer} are already in Fig. 2.

p.6 section 3.3: What is the resolution (horizontal and vertical) of the model, especially close to the ice front? What are the time steps used for the continuum model?

We add this information:

"We use an unstructured mesh, with spatial repartition of elements based on the mean observed surface velocities in the horizontal plane (roughly 30 m resolution close to the front). Vertically, the 2D mesh is extruded with ten levels (roughly 10 m resolution close to the front)."

Later in the section (after the surface equation), we add:

"We use a time step of 1 day."

p.7 l.8: convention for the reference (twice)

Corrected!

p.7 l.29: "five kilometers" → "five kilometers away"

Done!

p.8 l.1: How long does it take to reach a steady-state?

We have clarified this in the text, p.8 l.1 now reads: "The model is spun-up for 1000 model seconds until the turbulent kinetic energy in the region of the plume reaches a steady state..."

p.8 l.4-7: So my understanding is that the discharge varies but not the ocean conditions. Ocean conditions are reported quite accurately on p.3, so why not use these conditions instead of uniform ambient ocean properties? Also, in all these cases, the ice front is assumed to be vertical, why not try cases with pre-existing undercutting? I understand that it might not be possible to test all these

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cases, but at least assessing the uncertainty caused by such assumptions would be important.

The model uses temperature and salinity profiles collected from Kongsfjorden, as described on p7. l.26–30. They are uniform in the sense that the same conditions were used in the different discharges tested, we have rewritten to clarify this. Edited p.8. l.5 to "Instead, representative cases M_d using the ambient ocean properties described above and discharges d of 1, 10, 50 and 100 $\text{m}^3 \text{s}^{-1}$ were tested and the melt rate profiles for intermediate discharges were linearly interpolated from these cases."

The reasons for and implications of not varying the ocean properties and the ice front angle are discussed in Section 5.1. However, it is important to give the full picture and we have replaced the paragraph on ocean variability in the discussion (p.19 l.3-12) with the paragraph below:

"By using ambient temperature and salinity profiles that do not vary in time, we neglect the inter- and intra-annual variability in Kongsfjorden. This variability can affect the calculated melt rate in two ways: i) the three-equation melt parameterisation explicitly includes the temperature and salinity at the ice-face, and ii) the ambient stratification affects the vertical velocity and neutral buoyancy height of the plume. The direct effect of changes in temperature and salinity on the melt equations are well tested. Past studies using uniform ambient temperature and salinity conditions have found a linear relationship between increases in ambient fjord temperatures and melt rates, with the slope of the relationship dependent upon the discharge volume (Holland et al., 2008b; Jenkins, 2011; Xu et al., 2013). Salinity, on the other hand, has been shown to have a negligible effect on melt rates (Holland et al., 2008a). However, with a non-uniform ambient temperature and salinity, the effects of changes in the stratification on the plume vertical velocity and neutral buoyancy are much more complex. The stratification in Kongsfjorden is a multi-layer system, with little or no direct relationship between changes in different layers (Cottier et al., 2005). Therefore, testing cases by uniformly increasing or decreasing the salinity would not be informative for understanding the true effects of

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inter- and intra-annual variability. The high-computational expense of the plume model used here means that it is not yet feasible to run the model on the timescales necessary to understand this variability, nor to run sufficient representative profiles to provide a useful understanding of the response. Previous work has suggested that intra-annual changes in the ambient stratification are small enough that plumes are relatively insensitive to these changes (Slater et al., 2017) and that plume models forced with variations in runoff and a constant ambient stratification can qualitatively reproduce observations (Stevens et al., 2016). For these reasons, we highlight this as a limitation of the current implementation, and suggest that this should be addressed in future investigations of plume behaviour. A model based upon one-dimensional plume theory (e.g. Jenkins, 2011; Carroll et al., 2015; Slater et al., 2016) would be less computationally expensive and may allow some of these limitations to be addressed. However, such a model would not capture the strong surface currents driven by the plume which are important for the terminus morphology studied here."

p.9 I.6-10: What is the rationale for keeping or removing the undercut in one case or another? Some explanations justifying these choices should be added.

See answer to major comment.

p.9 I.19: How many broken beams are added and how was this number chosen? What is the impact of increasing or reducing this number on the results? Also does the number of broken beams increase during the melt season as the ice gets more damaged?

This must be some kind of misunderstanding on how the model is constructed - we do not add broken beams at any point. What the referee probably refers to is the small fraction of broken beams at the beginning of the simulation. As long as this fraction is small and broken beams are spatially uncorrelated it has only a minor influence on calving. It is a good suggestion by the referee to increase this fraction to mimic melt and to investigate how that would affect calving. However, in our opinion it would not

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be useful to add even more results to this paper and yet another calving variable in a model which already has a lot.

p.9 I.30: How long is the HiDEM model run for at each time step? And how long does it take to run it?

We add this information: “The model run for 100 s, which takes two days of simulation physical time.”

p.10 I.2: What kind of instabilities are developing and why?

We add more information to make this statement clearer:

"HiDEM reads a file with surface and bed coordinates on a grid and a file with surface and basal ice (to take into account the undercut) coordinates. When simulating with an undercut at a discharge location and in order to avoid complication in the HiDEM (position of the basal ice), we remove particles below the maximum melt (no ice foot)."

p.10 I.7-11: What is the rationale for decreasing the friction? How is the choice of friction impacting your results?

There is a clear separation of timescales between the velocities of sliding (\sim m/day) and calving ice (\sim m/sec). This means that as long as sliding is slow enough to be negligible during a single calving event, we can change it without much effect on any single calving event. As an approximation we can assume that fast processes are at equilibrium when we consider slower timescales. However, a rescaling speeds up the frequency of calving, and we can thus 'speed up', within reason, the few minutes of HiDEM simulation to effectively model calving which would otherwise take tens of hours or days, and thus be practically impossible to simulate with HiDEM. By applying scaling, the calving events modelled during the simulation of HiDEM (few minutes) correspond to the sum of calving events that would happen during the time scale of sliding. The scaling factor that we use is the same for the whole domain and for all simulations. We use a friction scaling factor for β equal to 10^{-2} (or sliding velocity scaled up by 10^2),

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and simulations run until calving stops and a new quasi-static equilibrium is reached. This is now better explained in the text:

"There is a clear separation of timescales between the velocities of sliding ($\sim m \text{ day}^{-1}$) and calving ice ($\sim m \text{ sec}^{-1}$). This means that as long as sliding is slow enough to be negligible during a single calving event, we can change it without much effect on any single calving event. As an approximation we can assume that fast processes are at equilibrium when we consider slower timescales. However, a rescaling speeds up the frequency of calving, and we can thus 'speed up', within reason, the few minutes of HiDEM simulation to effectively model calving which would otherwise take tens of hours or days, and thus be practically impossible to simulate with HiDEM. By applying scaling, the calving events modelled during the simulation of HiDEM (few minutes) correspond to the sum of calving events that would happen during the time scale of sliding. The scaling factor that we use is the same for the whole domain and for all simulations. We use a friction scaling factor for β equal to 10^{-2} (or sliding velocity scaled up by 10^2), and simulations run until calving stops and a new quasi-static equilibrium is reached."

p.10 l.19: It should be mentioned that this is volumetric ablation rate (same for volumetric calving rate in the rest of the paper). Many people use calving/ablation rate as changes per unit area (in m/yr), which can be confusing.

We add "mean volumetric" in front of ablation rate and calving rate in the whole paper.

p.10 l.20: Integrals over Gamma usually refer to contour intervals and not surface integrals, using S or Σ instead would be more consistent with literature.

Thank you for the suggestion. We change Γ for Σ .

p.10 Eq.5: What is $z\Gamma_w$?

This was meant to be the vertical dimension of (now) Σ . We change it to $z \in \Sigma$.

p.10 l.28: "parameterisations" → "parameters"

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Done!

p.10 I.30: u was already used for velocity (see above)

We change the velocity for v and kept u for undercut (see above).

p.10 I.30: Only 4 time steps are mentioned here. What happens to the other ones, are they just excluded? In this case, what is used for the prior undercut?

Undercuts are computed from for each timestep for which there are observations, so independently from theand HiDEM simulations were conducted for four time steps (t_0 , t_4 , t_6 , and t_{11}). This is now clearly expressed in the text and Tables.

p.11 I.1: Only a subset of $(i, j) \in [0, 4, 6, 11]$ is covered, not accurate.

We remove this.

p.11 I.6: configuration C_k is not defined and not used anywhere else, should be consistent with the rest of the paper

That is right. We changed the names of the configurations at a later stage but the table had not been updated. This is now done!

p.11 Tab. 2: Configuration is here a function of time (t_i) as opposed to geometry (g_i) in the rest of the paper

See above.

p.12 Fig.6 caption: "data gaps corresponds" → "data gaps correspond"

Done!

p.13 I.Tab.3: Discharged should be provided in m3/s to be consistent with the rest of the text. No data between t_{10} and t_{11} , this should be added even if the values are just zero. Also, how are the melting rates for each case computed based on Fig.7? Is an interpolation between the four cases been performed? Or something else?

In Table 3, we wanted to show the total volume discharged during the time period, hence a volume in m^3 . However, if it is irrelevant, we could provide an averaged discharge in m^3/s . We added t_{10} to t_{11} . Yes, melting rates are interpolated from the four cases and this is mentioned in section 3.5:

"discharges d of 1, 10, 50 and $100 \text{ m}^3 \text{ s}^{-1}$ were tested and the melt rate profiles for intermediate discharges were linearly interpolated from these cases"

p.13 Fig.7: How different are the results if there is undercut introduced in the geometry?

We discuss the impacts of this in Section 5.1. Slater et al. (2017) show that undercuts only have a weak effect on plume dynamics, and therefore our projection of the melt rates onto the terminus is a reasonable way to address this, based on research to date. This is an area which requires further investigation with high-resolution plume models; however, as this is not the primary focus of this paper we suggest that once other studies have addressed this, their results, models and methodologies can be incorporated into future development of the approach we present here.

P.14 Fig.8: It is the only time in the paper, where results from times other than t_0 , t_4 , t_6 and t_{11} are presented. Are the other time steps computed? And what is the rationale to only present some ice front positions here?

The undercut estimation from melt rates is independent from the HiDEM simulations. We use daily runoff and observed front positions for it. So in order to get the undercut for t_6 , we need to know the undercut state from previous steps. We do not present the front positions after t_6 since we do not use them for the HiDEM simulations. For t_{11} , we consider the front vertical (the remained undercut from last iteration with melt being calved away).

p.14 Fig.8: If z is the height above sea level, Fig.8 b and c are for $z = -3\text{m}$ and $z = -42\text{m}$, and the stars in Fig.8 d and e indicate the plan view elevation, why are

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the start not aligned at the same height on Fig.8 d and e? Also it might be more clear to use "Elevation from sea level" or something similar instead of "Distance to the bed" on Fig.8 d and e as this is what is used in the rest of the figure.

The stars are not aligned because the sea level is actually not at the same position compared to bed elevation for each iteration. We chose to use "distance to bed" instead of "elevation from sea level" because the plume starts at the bed elevation and we wanted to compare from there.

p.14 l.11: What do the authors mean by "smoother"?

We mean the undercut is not as abrupt as ND: We change the sentence to:

"In the first 50 m from the surface, the undercut at the SD is not as abrupt as at the ND and is also smaller"

p.14 l.12: "stretching" → "stretches"

Done!

p.15 l.2: Why not do it more often? How long does it take to run the model?

The runoff is available everyday and thus the discharge. Undercutting is therefore applied everyday based on the observed front position. Unfortunately, we only have observed front every 11 days and that is why we do not do it more often. We add this information:

"One should keep in mind that our modelling approach neglects the change of the front during the period of interest between two observations of frontal positions (11 days for most cases)."

p.15 Tab.4: "modelled" → "estimated": the melt is estimated from observations, not modeled. What is \dot{a}_m^o bs ? Also add zero values where appropriate instead of leaving empty spaces.

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This was a mistake, this should be \dot{a}_m .

p.16 Fig.9: $\dot{a}_c = \dot{a}_{c,u} - \dot{a}_{c,L}$ **on p.15 (minus not plus), so it's not clear what is shown on this figure.**

$\dot{a}_{c,L}$ is actually negative so what is shown on the figure is $-\dot{a}_{c,L}$. We change the figure accordingly.

p.19 I.5: "Due to this, there is some uncertainty" → "There is therefore some uncertainty"

Done!

p.19 I.7: Add references (or something else) to justify this statement that is not supported.

We have rewritten and added references to support this statement:

"Previous work has suggested that intra-annual changes in the ambient stratification are small enough that plumes are relatively insensitive to these changes (Slater et al., 2017) and that plume models forced with variations in runoff and a constant ambient stratification can qualitatively reproduce observations (Stevens et al., 2016)."

p.19 I.24: Is that what is observed by the authors?

Yes.

p.20 I.1: "no retreat at all": this is not supported by the model results, there is some retreat in the southern part of the domain.

We meant no retreat at all at SD. We add this information.

p.20 I.9-10: I don't think that the experiments made and results support such a conclusion, the role of sliding and geometry cannot be clearly separated.

See answer to major comment and new rewriting of the section.

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p.20 I.19: "the velocity higher" → "the higher velocity"

Done!

p.20 I.19: "seem" → "seems"

Done!

p.20 I.30: "reproduces" → "reproduced"

Done!

p.21 I.12: "in 2D" → "with a simplified 2D geometry"

Done!

p.21 I.14: How do the melt rates compare to previous results?

There are no previously published frontal melt rates for this glacier (modelled or observed), so no comparison is possible.

p.21 I.23: What is referred to as the calving model?

We mean the discrete particle model. The text has been changed to make this clear.

p.22 I.5: "would be implemented" → "were implemented"

Done!

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