Review of ‘A 3D glacier dynamics-line plume model to estimate the frontal ablation of Hansbreeen, Svalbard’ by Muñoz-Hermosilla et al.

Editor

Thank you very much for your prompt response to the comments by the reviewers. I would like to highlight two topics that were lifted by the reviewers, that I think needs to be answered a bit more carefully:

1) Initialization/relaxation: Reviewer one and two ask about whether no initialization/relaxation has been done, and I understand from your response that you consider the 17 first months of simulation to be an initialization and relaxation. Then it would be fair to take these simulated months out of the results (e.g. out of the figures) and clearly describe this initialization/relaxation-procedure in a written paragraph.

After discussing this point, we thought that we did not really need an initialization since the comparison with observations starts at the 17-th month of the simulation. However, it is true that, if we consider all that period as an initialization, all those months should be out of the results, as proposed by the editor. When analysing the model outputs during the first months of the simulation, we see that the two first months, September and October 2008, present an anomalous behaviour. But, beyond that, the model stabilises. Therefore, we have decided to delimit the initialization period from September 2008 to March 2009, so we leave the model some time to stabilise beyond November 2008 and still we have two whole years of simulation (March 2009-2011).
Now, following the editor’s suggestion, we do not consider the results of that period anymore (*), and we have edited the figures to include a grey area indicating the initialization in the graphs (new Figure 4). Besides,, we have included a brief description about the initialization in the Model Design section (change 1).

2) Time-steps: Reviewer one asks about the one month long time-steps, and what would happen if you decrease them. This is an important question that should be tested experimentally (is time variability really only determined by fjord conditions data?). Doing so could potentially highlight even more the advantage of coupling many processes together.

A sensitivity analysis has been performed to address this question (included at the end of this document). When taking a smaller time-step such as two weeks (as suggested by Reviewer one) the results do not present significant changes while the computation time increases considerably.

A minor note is that the first sentence in the introduction was a little bit hard to read.

Edited for clarity (change 11).

Anonymous Referee #1

The article advances our ability to simulate ice calving in fjord environments, providing useful validation against observations. This is an active area of research seeking to address critical gap in the capabilities of cryospheric models.

Main comments

1) I feel the model methods would benefit from an expanded section on the implementation of the moving calving front, as the current section has some ambiguities on how exactly calving rates are applied as well as sub grid scale positioning.

As suggested, a more complete explanation of the moving calving front has been included in the Model Design section (change 1). Besides, more information about the calving model has been included in the ice-flow model section (change 3).

2) I feel the discussion could benefit from a section on the application of the model to situations outside the current case study. For example, if the model were to be run for a longer time scale what would the likely effect be of the overestimation of calving front position, how applicable would the setup be to other fjords, etc.

The Conclusions section has been modified taking into account this general comment (change 2).

Small comments

Fig 1: Some indication of which direction North is would help with orientation.

Applied. New Fig 1 now includes a north reference.

Fig 2(a): I am slightly confused by the velocity scale going to negative numbers. Surely this should be impossible, as the magnitude of speed is being shown?

There was a mistake in the colour bar that we did not notice. Indeed, there are no negative values of the velocity, so we just removed that part of the bar so now it starts from zero.

Fig 2(b): I think it would be better to include some indication of the month on the x axis so that it is easier to comprehend the seasonal cycle taking place. Replace number of months with a letter, or similar? Same comment for all similar plots in the manuscript.

We appreciate the comment and it has been something that we have largely discussed before to establish the current labels in this work. We believe that placing the names of the months on the x-axis will result in a cluttered figure. For that reason, we decided to include the yellow zones to highlight the summer periods and the vertical black lines to separate the years.

Fig 3: Just to clarify, here and throughout the paper, are all months assumed to have 30 days or do the days in a month vary?

Yes, all months are supposed to have 30 days. Following this comment, we have included this information in the Model Design section (change 1).

Fig 5: Plot (a) would indicate that the initial conditions in September 2008 are not an exact match to observations but instead a rough approximation to them. Is there a reason behind this choice? The figure also implies that there are fixed points on either side of the domain which the ice cannot retreat or advance from. This should be mentioned somewhere in the text if so.

Certainly it is a rough approximation, but it is the best contour that we have got. The reason for this choice is practical, i.e., we use it because this is the date when we have a surface DEM available. As for the fixed point, the model requires that the nodes corresponding to the front be labelled, because, as you indicate, these nodes will be the ones that are allowed to advance and retreat. Therefore, these fixed points have been chosen to determine which part of the glacier we consider to be the front and which part we no longer consider to be the front. We have included this information where we talk about the boundary conditions (change 4).

L111. Were any convergence tests done on the asynchronous time step for the coupling? Would going to 2 weeks instead of a month impose any significant changes, for example.

No convergence tests had been performed. The asynchronous time-step, one month, had been established regarding the availability of fjord conditions data. Since we barely had monthly temperature and salinity conditions, we did not think that using a lower time-step would impose significant changes. However, following this comment and the editor's suggestion, a sensitivity analysis has been performed to address this question. When taking a smaller time-step such as two weeks (as suggested by Reviewer one) the results do not present significant changes while the computation time increases considerably.

L145. I assume this is a fair assumption to make in line with observations?

Yes, it is. This assumption is based on observations.

L182. I feel this section could benefit from a little more detail regarding the front evolution. Is a new mesh generated every day to allow for the changed front position or is its exact position being tracked on a sub grid scale? Is this process fully conservative of mass, etc. Are calving rates applied normal to the ice geometry at the front, or normal to the ice velocity at the front? Or are these considered one and the same? Figure 3 does seem to address a few of my comments but would appreciate the detail regarding remeshing in the main body of the text.

As mentioned before, the Model Design section has been rewritten and this clarification is included in the corrected version (change 1).

L215. I gather from these advance/retreat rates that in any single month the front will not move more than the mesh resolution at the calving front. I assume this is a deliberate choice, and mesh resolution would have change if there was a faster movement rate with the current model framework?

The selection of the mesh resolution is based on the shape of the glacier and the values of the velocities. So yes, it is a deliberate choice and, consequently, would have changed if glacier characteristics were different.

L 220. The simulations start during the period of the year with the greatest amount of calving. Does the model require or need an initial relaxation period, and would starting the model during the winter have any effect on the following calving season compared to starting in the middle of the calving season?

After discussing this point, we thought that we did not really need an initialization since the comparison with observations starts at the 17-th month of the simulation. However, it is true that, if we consider all that period as an initialization, all those months should be out of the results, as proposed by the editor. When analysing the model outputs during the first months of the simulation, we see that the two first months, September and October 2008, present an anomalous behaviour. But, beyond that, the model stabilises. Therefore, we have decided to delimit the initialization period from September 2008 to March 2009, so we leave the model some time to stabilise beyond November 2008 and still we have two whole years of simulation (March 2009-2011).
Now, following the editor's suggestion, we do not consider the results of that period anymore (*), and we have edited the figures to include a grey area indicating the initialization in the graphs (new Figure 4). Moreover, we have included a brief description about the initialization in the Model Design section (change 1).

L236. It is slightly unclear to me whether you are expecting there to be plumes in reality which the model is not representing, or whether modelled ocean conditions which agree with observations are not leading to the generation of plumes in this particular fjord, unlike other modelled fjords such as Cook 2020.

From observation we know that plumes can emerge at Hansbreen front even in winter. However, our model is not able to generate plumes during these months. Out of the winter period, on the other hand, modelled plumes do correspond in some cases to the zones where plumes have been observed.

Anonymous Referee #2

This article uses a 3D set-up of the numerical model Elmer/Ice coupled with a plume model to investigate frontal ablation at Hansbreen in Svalbard. Model results are then compared to observational data such as front positions from time lapse imagery. In general, the article is interesting, well-written, and scientifically sound. However, I have a few suggestions which I believe would improve the manuscript.

General comments

Methods

The CTD measurements used to create the time series of hydrographic conditions in the fjord are never shown. It would be interesting to see the time series of fjord temperatures and salinities plotted alongside the time series of subglacial discharge. In the discussions, it is mentioned how changes in discharge cannot alone be responsible for changes in the plume melting (and that therefore fjord ambient conditions are important), so I feel this data should be presented.

Figure 4 has been modified to include the averaged CTD data.

Was no spin-up/initialisation done between the inversion and the transient simulations? This step has been done by many previous studies using Elmer/Ice for studying calving in 3D (e.g. Todd et al., 2018; Cook et al., 2020; Holmes et al., 2023), so it would be pertinent to mention why a different approach is taken here and what the implications may be.

After discussing this point, we thought that we did not really need an initialization since the comparison with observations starts at the 17-th month of the simulation. However, it is true that, if we consider all that period as an initialization, all those months should be out of the results, as proposed by the editor. When analysing the model outputs during the first months of the simulation, we see that the two first months, September and October 2008, present an anomalous behaviour. But, beyond that, the model stabilises. Therefore, we have decided to delimit the initialization period from September 2008 to March 2009, so we leave the model some time to stabilise beyond November 2008 and still we have two whole years of simulation (March 2009-2011).
Now, following the editor’s suggestion, we do not consider the results of that period anymore (*), and we have edited the figures to include a grey area indicating the initialization in the graphs (new Figure 4). Moreover, we have included a brief description about the initialization in the Model Design section (change 1).

Results - Include calving volumes, velocity time series, and more comparison with observational data

I appreciate the figure showing the number of calving events through time and how this shows a strong seasonal signal (fig 4d). However, it is not clear how this

translates to mass/volume loss. Later on in the discussion it is mentioned that calving is the main contributor to frontal ablation but, without presenting some metric such as calving mass/volume loss which is comparable to the melt rate data shown in Fig. 4c, it is not clear how you know this.

Following this comment, Figure 4 has been modified to include calving mass loss instead of the number of calving events so it can be compared with submarine melting mass loss (*). Accordingly, it is fair to say, at least in this case, that calving is the main contributor for frontal ablation.

I think the manuscript would greatly benefit from the inclusion of a figure showing the observed front evolution through time. Specifically, Fig. 5b should be edited to include both the observed and modelled glacier length change rate. This would add an important extra datapoint for evaluating the model.

We agree that comparing the observed and modelled glacier length change rate can be meaningful, so it has been included in Figure 5b.

Furthermore, you should show modelled velocities and how these compare to observations. This could be done as a comparison between modelled and observed velocities from the inversions, or by showing modelled and observed velocities during the time period(s) of transient simulations (or both). As advance/retreat rates are a function of both frontal ablation and glacier velocities, it is hard to assess how well the model recreates frontal ablation without being able to compare modelled vs observed velocities.

We have included in Fig. 2b the modelled velocities after the inversion corresponding to the last stake, so modelled and observed velocities can be compared.

Discussion

Model assumptions should be more clearly discussed along with the impact of choice of e.g. sliding law – why was Weertman law chosen in place of a sliding law that includes effective pressure? This links to the previously mentioned point about including a plot of modelled vs observed velocities; how well did the model recreate the observations? Additionally, some discussion on different calving laws would be interesting; why was a crevasse depth calving law used for this location? Some previous studies have found a von Mises law to work best, whilst others have suggested crevasse depth is preferable (e.g. Choi et al., 2018; Amaral et al., 2020; Benn et al., 2023; Wilner et al., 2023). Crevasse depth seems like an appropriate choice here and, to my knowledge, is the only calving law implemented in Elmer/Ice - but it would be good to briefly discuss some other widely used calving laws.

A simple Weertman-type sliding law has been chosen for simplification (versus a Coulomb-type, for instance) assuming that at Hansbreen ice flows over hard bedrock (change 5), so that inversions are able to generate reliable modelled velocities. Looking at the new Fig. 2, where modelled and observed velocities are compared, we see that the values are close enough to consider a Weertman law as an appropriate choice for this work. As for the calving law, a brief discussion has been included in the ice-flow model subsection (change 6).

Specific Comments

L2: The relative contribution of calving and submarine melt to frontal ablation is variable (e.g. see Bartholomaus et al., 2013), so I think the statement about calving being the main contributor the frontal ablation should be edited

We have relaxed the meaning of the sentence by adding the adverb “often” (change 7).

L31: Here you say ‘ice loss from glaciers will contribute…around 56% of the total glacier estimation’. I guess the first mention of glacier corresponds to non-ice sheet areas only, but the second mention of glacier includes all glacier and ice-sheet areas? Please edit for clarity.

Edited for clarity (change 8).

Fig. 2: Please include location of stake used for time series (I know it is shown Fig.1, but this would prevent scrolling up and down).

The location of the stake has been included in Fig. 2 (blue dot).

L129: Maybe also mention that crevasses are assumed to be advected downstream e.g. you do not need to consider their opening in the model.

A small indication has been included in the last part of the Ice-flow model subsection (change 9).

L159: Could this assumption impact the hydrological model and be related to the higher model/observation misfits along the Eastern flank of the calving front?

We don’t think so. Actually, the SMW values in the Eastern flank of the calving front are of the same order as in the rest of the calving front. Therefore, the approximation could be discussed but we don’t see a direct relation with the higher model/observation misfits.

L160: Is frictional melt also considered?

No, it is not.

L179: My understanding is that step 2 of each cycle has a free top surface and no front evolution, and then step 3 of each cycle has a fixed top surface and a moving front? The text in this section could be edited for extra clarity, as I am not 100% sure I interpreted this correctly

To better explain the whole process, the Model Design section has been rewritten (change 1).

L232: How does the efficiency of the modelled drainage system look at this point? Do we see differences in the development of the modelled drainage system compared to other years?

Unfortunately, our model does not capture such a difference in the drainage system, just in the values. What we argue here is that reduction in the velocities has been observed to match with a change in the drainage system, and that could be an explanation.

L245: It would be preferable to give the maximum melt rates in the same units for ease of comparison (e.g 15 m3/week to be written as 66 m3/month [31 days in Aug]).

Edited (change 10).

L247: Should be ‘ALMOST’ exactly double (401 versus 201) as 401 is not exactly double of 201.

That “almost” has been added. However, this part is going to change anyway because the number of calving events have been replaced by calving volume.

L250: This goes back to one of the general comments, but it would be really interesting to include calving volumes here rather than just the number of calvings. Additionally, if you find a relationship between the number of calving events and the plume melt rate, why do we see more calving in July/Aug 2010 compared to July/Aug 2009, when the plume melt rate is higher during these months in 2009? I think more discussion around this relationship is needed.

Calving loss volume has been included instead of calving events (*). With these calving volume values, we see that calving in July/Aug 2009 is actually larger than in July/Aug 2010 (7742663,00149 vs 4087400,59207 m3/month), which is more consistent with plume meltrate values.

L254: Again, a reiteration of a general comment; you say calving is the main contributor to frontal ablation but have not actually presented the data points that allow for that comparison to be made

Data is now presented in Fig. 4.

L255: Did your model results provide any information on the processes by which submarine melt drives calving? E.g. does calving start occurring more frequently once some threshold is reached in terms of melt magnitude/undercut size etc

With the result of our model, one can only establish that submarine melting promotes calving since larger values of plume meltrate correlate with larger values of calving volume. We have not found a threshold, since it would probably require a sensitivity analysis, but we will take note for future work.

L265: Do you have any ideas for why plume melting may be underestimated by the model in this area specifically? An issue with gaps in the time series of fjord hydrographic data? Assumptions in the hydrological model? Something related to boundary conditions in Elmer/Ice? Please expand.

We propose two potential explanations for this observation. Firstly, the lack of CTD data for this region raises the possibility that our model may be underestimating submarine melting rates, which could contribute to lower calving rates. Secondly, the absence of stakes in the vicinity of this region limits our ability to constrain velocity values, potentially leading to an overestimation of observed (and therefore, modelled) velocities.

L300: Can you give a bit more context for the wider application of your study? Why does it matter than we can recreate the past terminus evolution of Hansbreen/ what can we do we this information/ are your findings broadly applicable to other areas? To clarify, I think the study is interesting and see important future applications for the work, but think this could be more clearly demonstrated in the conclusions.

The Conclusions section has been rewritten following the comments of the other referee. Here I include that new version with some other modifications addressing your suggestion too (change 2).

Technical Comments

L8: 'data are available for both THE glacier and fjord'

Corrected.

L15: '..change context affects to the dynamics and mass balance of glaciers, AS WELL AS the oceans…'

Corrected.

L33: '…terminate in the sea, with THEIR terminus either…'

Corrected.

L74: '…we include atmospheric PROCESSES…'

Corrected.

Here we include the changed parts of the text (in italic), so you can easily check whether the problems have been solved or not, and the modified figures.

Changes

1.Model Design (L179):

This model is implemented in 30 days monthly cycles that are run sequentially to cover the total simulation time, September 2008 to March 2011. The selection of this period is determined by the observational data. The simulation starts in September 2008, which is the date of the available surface DEM, but the first six months are considered to be the initialization of the model.Beyond March 2011, there is no surface velocity data available.

Every cycle is divided into 3 steps (Fig.03):

- Inversion for slip coefficient: an inversion using adjoint methods (Gillet-Chaulet12) is performed to adjust the slip coefficient to the changing mean velocities for a given month. This is done by minimising a cost function for the velocities, running monthly steady-state simulations. It is done monthly to account for the changes in the velocity field while keeping a reasonable computational cost.
- Dynamical and hydrological models, and free surface evolution: f*rom the inversion results, the hydrology is computed in a 30-day transient simulation with a 1-day time-step.* Subsequently, the daily subglacial discharge values are averaged over the month. *At this point, the glacier surface is left to evolve freely, whereas the front remains fixed.*
- Calving and plume models activation: the monthly-averaged values of subglacial discharge and the fjord ambient conditions are the required input for the line-plume model. The dynamic model is run again for a month with a 1-day time-step, but now with the modules for calving and plume enabled. *In this case, the hydrology is not computed, the glacier surface is left to evolve freely and the front is allowed to evolve as a combination of ice-flow, submarine melting and calving. Therefore, each of these time steps results in a new glacier geometry and a new front position. To avoid a mesh degeneration that could cause critical problems to the model, a remeshing is performed either when calving occurs or after 8 consecutive no calving time-steps.*

The first cycle starts from the geometry described in Sect. 2.1, while every new cycle will start from the resulting geometry of the previous one.

2.Conclusions section (L300):

Calving and frontal ablation are essential processes to understand tidewater glacier dynamics. We have developed a 3D glacier dynamics model that, in addition to solving calving and subglacial hydrology, accounts for oceanic (by a plume model) and atmospheric (by surface mass balance and surface meltwater) factors too. Subglacial hydrology provides discharge values that, in combination with appropriate fjord ambient conditions, are high enough to generate plumes at the calving front except for the coldest months, i.e., from November to April. The results for the hydrology are consistent with other studies using a similar model (Cook20), while the results for the plume melt rate are in agreement with other works on the Hansbreen-Hansbukta glacier-fjord system (DeAndres18).

The model is able to predict the evolution of the front position in terms of advance and retreat following a seasonal cycle with steep retreats in summer months and steady advances during the rest of the year. However, there are still differences between observed and modelled positions, especially in the eastern margin, where the longitudinal difference reaches 150 m in November 2010. In fact, when taking only the central part of the glacier front, the results improve significantly and the modelled positions become, on average, 40 % closer to the observed ones. In general, the difference between the modelled and observed front positions increases during the calving period, and we assume that the cause is an underestimation of calving by our model. Even so, the difference between the modelled and observed front positions decreases in some months, such as May, June, and July. In these months, the model is able to predict the front position with a very good level of agreement. In the eastern margin, our model is not producing enough calving events, which is causing that large differences. *The time-scale of this model is limited by the available data. We cannot say whether a longer simulation will result in a better agreement with observations, however, it would give us information on some results of the model that we cannot currently confirm. For example, do the differences in the eastern margin continue to increase or do they start to compensate at some point? As the results seem to suggest, is there a seasonality in the differences, such that they grow during the calving period and then decrease until reaching a minimum at the end of spring?*

Changes in SMW alone are not able to explain plumes behaviour, turning fjord ambient conditions into a key factor in this process. And plume-induced melting has proven to be an essential factor for calving to occur. Consequently, a logical next step would be to use a fjord model to obtain better estimates of ambient conditions.

Finally, Hansbreen is a largely studied glacier, becoming an essential context to test and to constrain our model. But, provided that we can count with the required input data, this model could be applied to any other tidewater glacier or glacier-fjord system.

3. At the end of the ice-flow model subsection (L135):

This improved criterion specifies calving to occur either when surface crevasses reach the waterline or when surface and basal crevasses meet. *To determine crevasse propagation, the calving model uses a separated 2D mesh representing the frontal area of the glacier. This mesh extends 1850 m up the glacier and has a resolution of 30 m. When calving occurs, the model calculates a calving vector which is normal to the calving front and maps pre-calving to post-calving node positions. In order to maintain the mesh quality, calving events require subsequent remeshing of the main mesh.*

4. At the beginning of the Boundary Conditions subsection (L137)

The calving front is determined by a series of nodes between two fixed points in the lateral margins of the glacier. Consequently, these nodes will be the ones that are allowed to advance and retreat. At the head of the glacier, the ice divide, horizontal velocities and shear stresses are set to zero. No flow is allowed...

5. Boundary conditions subsection (L140)

Considering that ice flows over hard bedrock at Hansbreen glacier, a simple Weertman-type sliding law is applied at the base following:

6. Ice-flow model subsection (L122):

The model solves the full-Stokes equations for ice flow, with rheology defined by Glen's flow law (e.g., Cuffey and Paterson, 2010) and uses the calving implementation described by Todd et al. (2018, 2019) and Cook et al. (2020, 2022), following Otero et al. (2010) and Todd and Christoffersen (2014). This implementation is an improved formulation of the crevasse depth *(CD)* calving criterion postulated by Benn et al. (2007) and Nick et al. (2010) for use in a 3D framework. *This calving criterion has been chosen because it is the one implemented in Elmer/Ice, but, when compared with other calving models such as the height above flotation model (HAF; Van der Veen, 1996), the fraction above flotation model (FAF; Vieli et al., 2001), the eigencalving model (EC; Levermann et al., 2012), the von Mises criterion (VM; Morlighem et al., 2016), and a calving relation based upon the surface stress maximum (SM; Mercenier et al., 2018), the results indicate that the crevasse depth calving model provides the best balance of high accuracy and low sensitivity to imperfect parameter calibration (Amaral et al., 2020). Moreover, a recent study of Benn et al. (2023) shows that the CD calving law reflects the glaciological controls on calving at a tidewater glacier (Sermeq Kujalleq) and exhibits considerable skill in simulating its mean position and seasonal fluctuations*

7. At the beginning of the abstract (L2):

Frontal ablation is responsible for a large fraction of the mass loss from tidewater glaciers. The main contributors to frontal ablation are iceberg calving and submarine melting, *with calving being often the largest*. However…

8. Ice-flow model subsection (L122):

The entire volume of water stored in Arctic glaciers, if melted completely, could raise sea level around 0.3 m (AMAP, 2017). *The projections for this region through the 21st century show that its contribution will be significant (Meier et al., 2007; Church et al., 2013; Hock et al., 2019) so, by the end of this period the estimated ice loss from Arctic glaciers would contribute between 3.9 and 9.2 cm to the sea level rise, around 56 % of the global glacier estimation (Edwards et al., 2021).* In the case of Svalbard, the contribution to sea level rise is estimated between 0.75 and 1.25 cm (Edwards et al., 2021).

9. Ice-flow model subsection (L129):

This term is here considered to be zero for surface crevasses because they are capable of opening without water pressure. For basal crevasses, on the other hand, water pressure is controlled by the subglacial hydrological system and at the calving front can be expressed as:

$P_w = (Z_{sl} - Z)\rho_w g$

being ρw the density of water at the calving front and Z the elevation with respect to sea level. Zsl denotes the sea level and is set to 0 m. This improved criterion specifies calving to occur either when surface crevasses reach the waterline or when surface and basal crevasses meet, *and its formulation disregards the formation of new fractures. Such a simplification is justifiable given the extensively fractured nature of ice near the calving front, leading extensional stresses primarily serve to propagate existing fractures.*

10. Discussion section (L245):

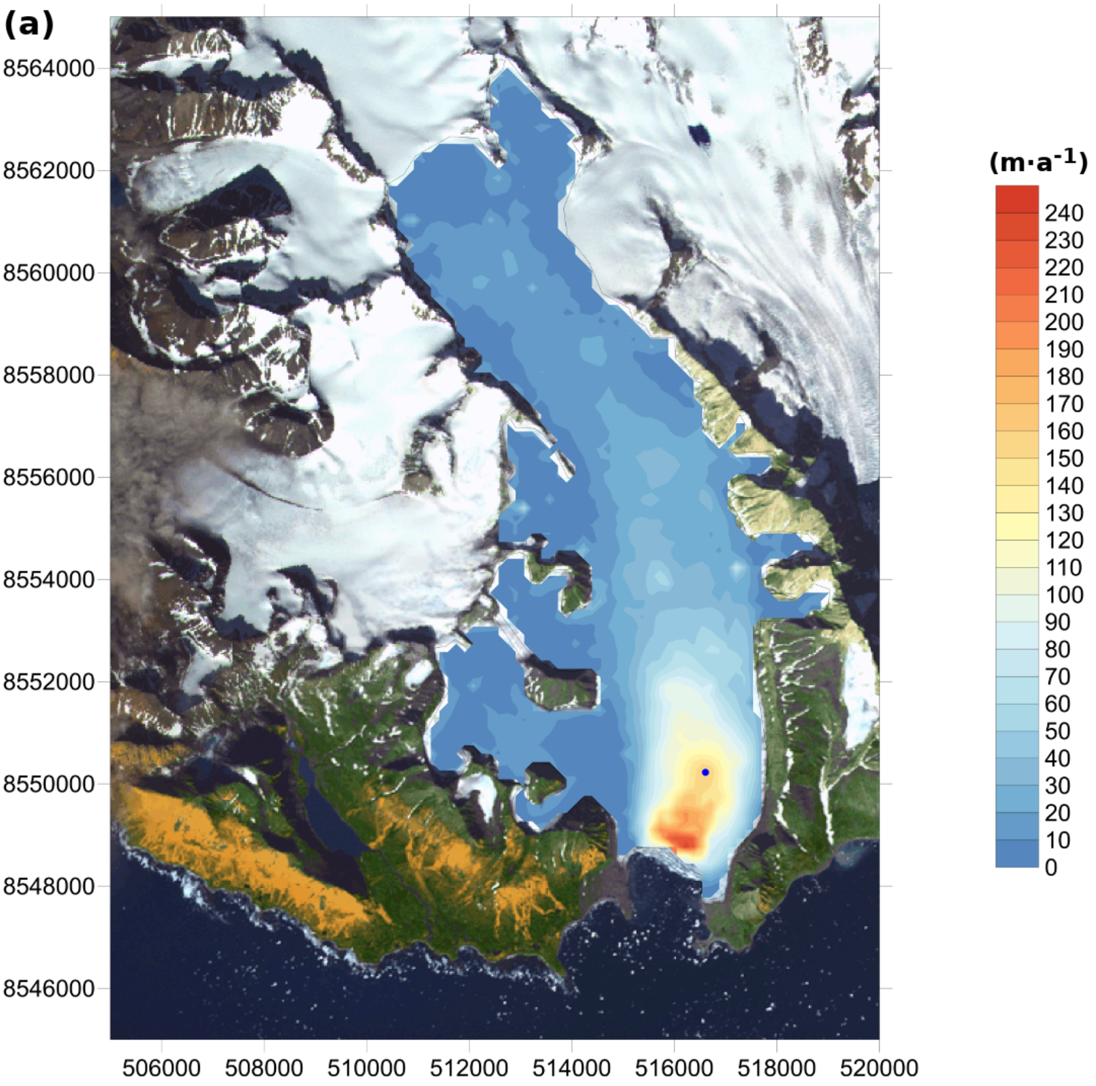
Comparing with other authors working on the same glacier-fjord system, the maximum melt rate values obtained for August 2010 are consistent with the ones obtained by De Andrés et al. (2018) *(58 m3/month versus 64.28 m3/month (15 m3/week))*.

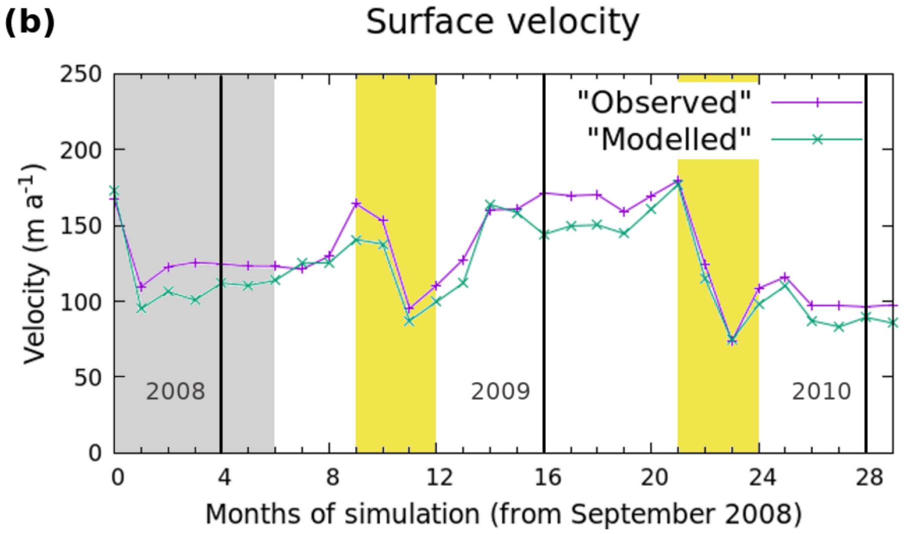
11. At the beginning of the Introduction (L14):

The ongoing climate change context *affects the dynamics and mass balance of glaciers, as well as the ocean's thermal and dynamical processes. This leads to* hydrological and ecological effects at regional and global scales, including sea level rise.

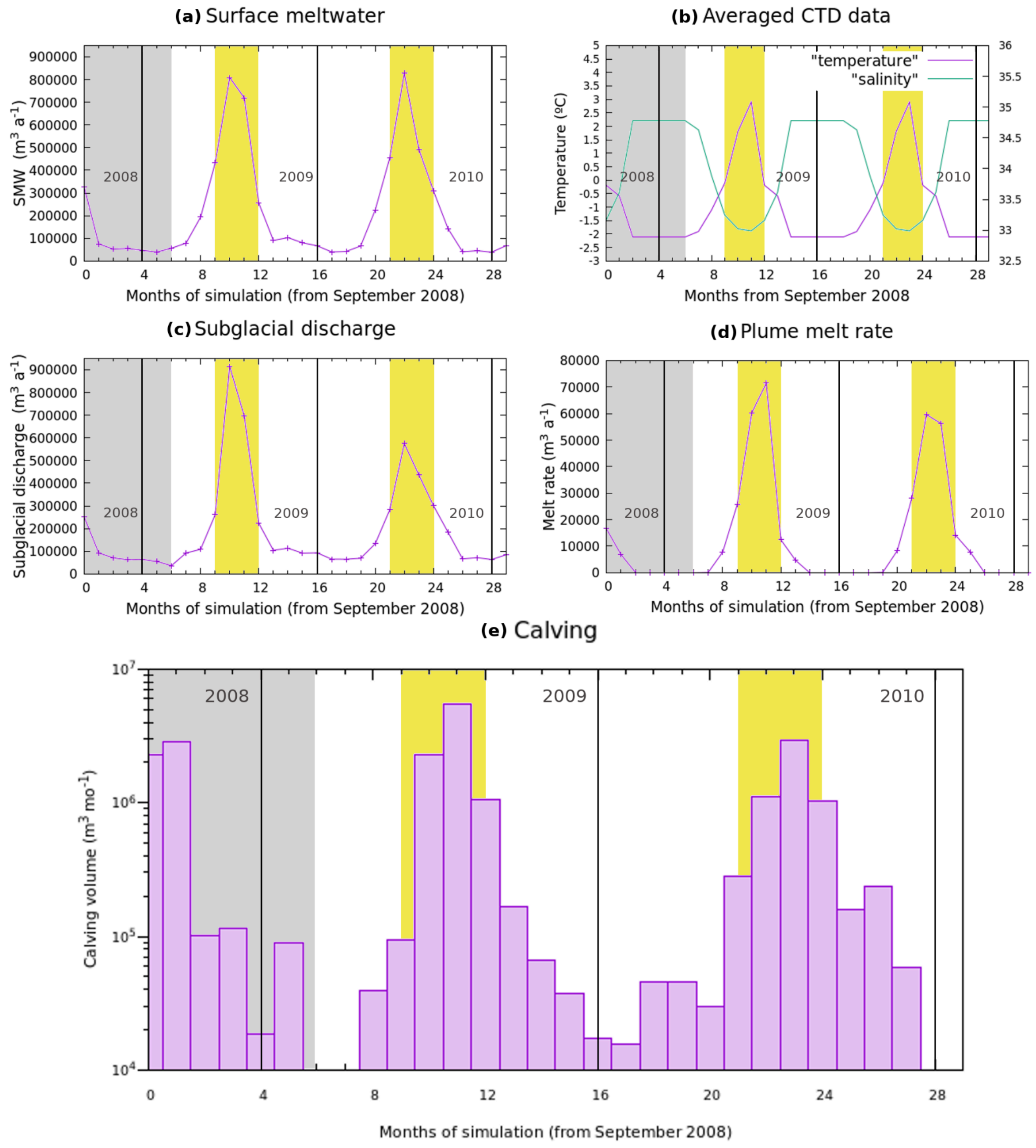
***. Some more modifications have been done in the Result and Discussion sections due to the change in Fig 4 that includes the calving volume instead of the number of calving events. Besides, references to the data corresponding to the initialization period have been removed. We do not include such modifications in this document because we think that it is easier to follow them in the marked-up manuscript version showing the changes.**

New Figure 2: last stake position included in (a); modelled velocities included in (b) so that they can be compared with observed velocities (in the last stake).

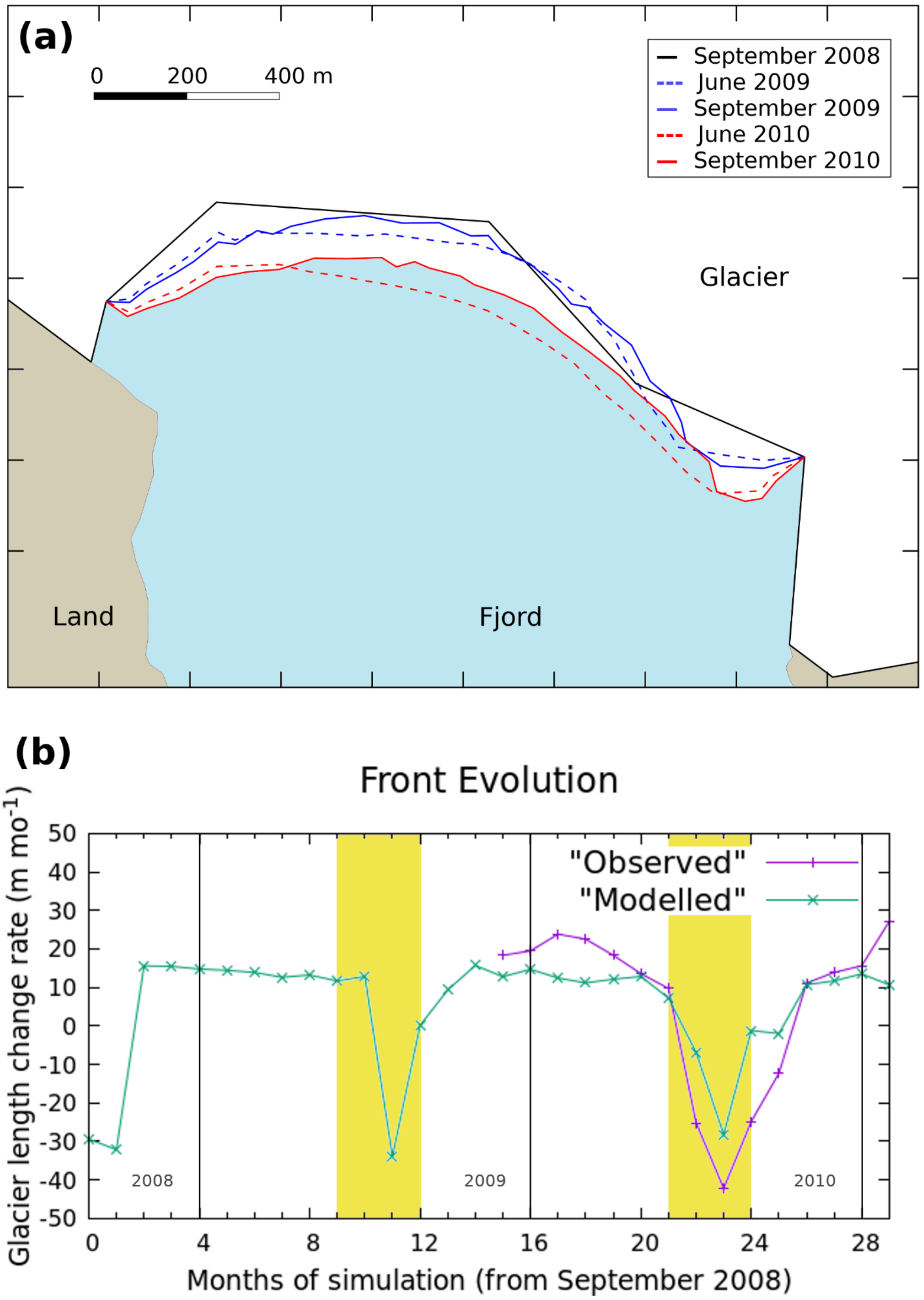




New Figure 4: CTD seasonal data included (b); number of calving events replaced by calving volume (e).

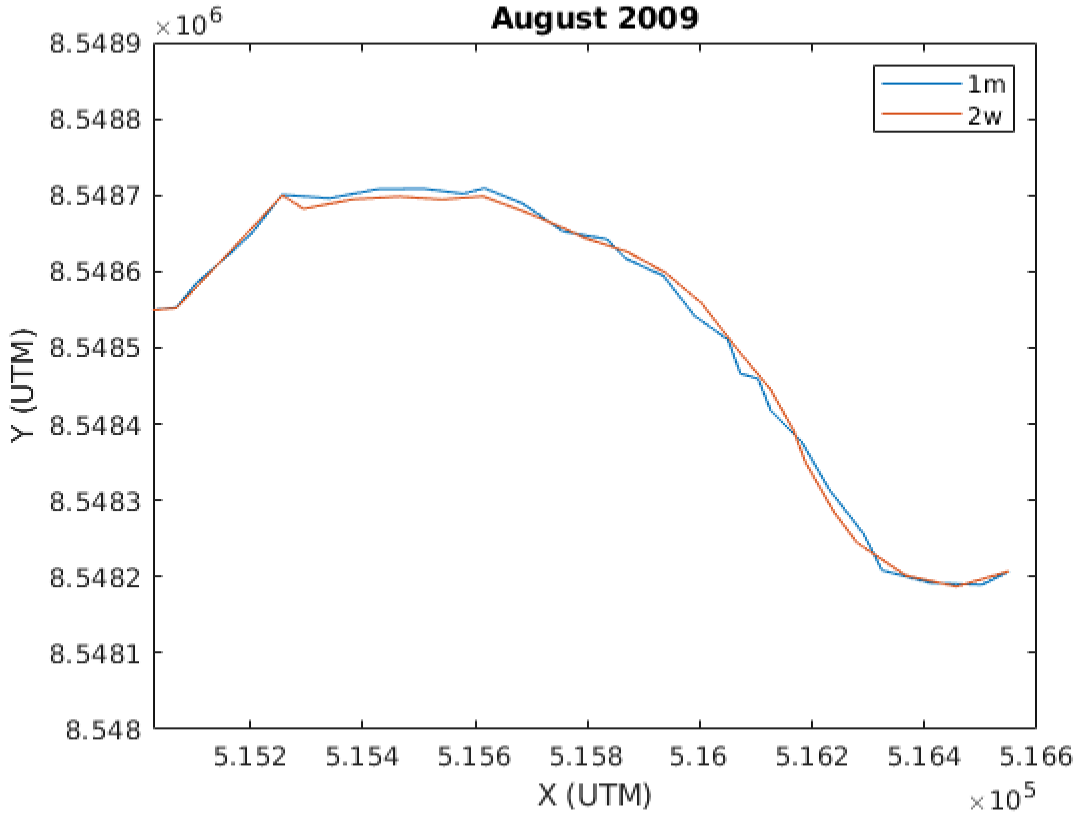


New Figure 5: observed glacier length change included in (b).



Sensibility analysis of the hydrology coupling

Following the suggestion of the Referee #1 and the editor, it has been tested a two-weeks time-step for the hydrology coupling. It has been run a simulation from April 2009 (the beginning of the plume activity period) to August 2009. Comparing the results between the one-month time-step and the two-weeks time-step, there is a small difference between the modelled front positions, while the computational cost increases significantly.



1m stands for 1-month time-step; 2w stands for 2-weeks time-step. By the end of the four months simulation, the longitudinal difference between the two positions is 1.2835 so, on average, the 1m front position is barely more advanced than the 2w.