Author's Response

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Reply to Referee #1

First, we would like to thank Joe Todd for his constructive review. He raised many points and presentation details that are now fixed, and helped improving greatly the quality of the article. Some remarks deserve a specific answer and are given using red color.

General Comments:

- There is a good coverage of the literature with regards to measurement of frontal processes around Greenland and their observed relationship with calving. However, for a modelling paper, I don't feel that the modelling literature is sufficiently addressed. I think the theoretical basis for the application of LEFM to glacier calving (van der Veen, 1998) should be better represented, as well as the development of the crevasse depth model (Benn et al., 2007, Nick et al., 2010).

- Related to my previous comment, I think the methods section should be expanded and its focus shifted somewhat. Currently, half the methods section describes the Stokes equations and Glen's flow law; these are well established in glacier dynamic modelling and, given that you don't refer to them elsewhere, I see little reason to include them. Instead, I think the calving model and particularly the damage model should be presented in more detail. Currently, it is very difficult to follow this paper without also having Krug et al. (2014) open alongside. I don't think it's necessary to present it in full detail, but the reader should at least be able to determine the meaning of the χ term and understand how to stress intensity factor is computed.

We address here the two previous comments. It would clearly help the global understanding to have a deeper explanation of the damage and calving model. Following these advices, we changed the method section (Section 2.2) in order to develop the calving model and corresponding literature, including some references to other papers (e.g. crevasse depth model)

- You state that the damage scalar D 'tends to 1'. How is D prevented from reaching 1? By setting a maximum value or by a non linear source term? Are the model results sensitive to this choice? I think this should be discussed in the text.

You are right, as the ice damages, D tends to 1 and a singularity appears. To prevent this case, according to what have been done in literature, we set an upper bound to the value of D. Numerically, D cannot exceed 0.7, accounting for the fact that D=0.6 usually refers to a fully damaged ice (see Pralong and Funk, 2005; Borstad, 2013). Sensitivity to this parameter was studied in Krug et al., 2014.
We developed this specific point in the model part (Section 2.2), as mentioned above.

-Two climate forcing processes are investigated, both of which cause the glacier to advance into the fjord. Were you able to find any forcings which resulted in retreat? If not, do you believe that this is a realistic result? Based on my own modelling work, I suspect that the lack of basal melting may be a factor here.

In the experiments presented in this article, we did not manage to observed convincing retreat, unless the melt rate was abnormally high (>= 12 m/yr). It often went along with degenerating elements and unsatisfying results. Introducing a realistic melting distribution below the floating tongue would probably help the glacier to retreat, indeed. Moreover, it must be highlighted that we focused our study on seasonal variations of realistic amplitude, without any long-term trend in the forcing conditions.
We added a sentence in the manuscript to clarify this point in Sect. 5.2.

-The results presented in Fig. 10 and discussed at the bottom of p.198 suggest that, following a calving event, the stress intensity factor near the terminus is too low for the initiation of a surface crevasse field here. The physical interpretation of this would be of uncrevassed surface ice near the terminus of an outlet glacier. As far as I'm aware, this is rarely, if ever, observed on real outlet glaciers in Greenland. Assuming that the geometries generated in this study were to be representative of Greenland outlet glaciers, I think this should be addressed in the discussion.

The Fig. 10.c means that, following a calving event (i.e. solid blue curve), there are two possibilities:
- the damage criterion D=Dc is not reached and prevent the use of LEFM
or
- the damage criterion D = Dc is reached, but because of the stress relaxation, the stress intensity factor KI is too low for the initiating an immediate crevasse propagation.
However, in any case, the crevasse field (created upstream and advected downstream), defined by the damage variable, still exists in the vicinity of the terminus.
A sentence was added in Sect. 5.1 to detail this situation.

-Can you comment on the possibility of including the effect of basal crevasses into this modelling framework in future work? Do you foresee their inclusion altering any of your conclusions with regards to the importance of melting and mélange or calving glacier stability in general?

It is hard to say how the model would react when incorporating basal crevasses, because of the many feedbacks between the damage model and the stress field. For instance, including basal crevasses would probably facilitate the detachment of the terminus part of glaciers which are near floatation. But one can also imagine that, when a basal crevasse forms, the subsequent stress release could lower the tensile stress at the upper surface and then reduce surface damaging.

This kind of experiment would require an independent study, which is out of the scope of the current article. However, we mentioned that aspect in the conclusion section.

-Results from 3 of your QSS geometries appear to be missing (Figs. 6,8,11).

Indeed. Geometries 4,5 and 7 are missing. They exhibited a QSS behaviour, but were unable to support the applied melt rates. The mesh degenerated and the simulations stoped. For clarity, we deleted them from Table 1. Now, the illustrative geometry (Fig. 4) is shifted to Geo. 9. Of course none of the results were affected by this change.

Line-by-line comments:

p.184.l.3: The authors might consider mentioning “water in surface crevasses”. While there is obviously debate about its importance, it has been suggested to be important by many previous authors.
We agree that the role of water in surface crevasses is a hot topic when talking about crevasse propagation and glacier dynamics. However, here we focus on processes that directly affect the calving front (i.e. the ice mélange and the frontal melting), and we believe that introducing this process in the abstract does not gives pertinent information to the reader.
However, we inverted the first sentence of the abstract to make it less peremptory. Also, we mentioned this specific point into the model section (Sect. 2.2).

p.184.l.5: “and glacier dynamics” no need for “the”
Done.

p.184.l.9: “pluriannual” doesn’t have much usage in English. Maybe “multiannual” or “decadal” would be better?
“Multiannual” sounds better.

p.184.l.9 & 11: Better to be consistent with use of “more than” or “>”. Also, perhaps “several kilometers” might be more readable.
Done.

p.184.l.15: Maintain present tense “Results also reveal”
Done.

p.184.l.16: “the largest forcings” or “most significant”. “heaviest” isn’t really appropriate here.
Done.

p.184.l.21: I think within the target audience of this paper, it would be sufficient to say “(Representative Concentration Pathways)” only. At any rate, repetition of “pathways”

should be avoided.
Done.

p.184.l.24 – p.185.l.1: It might improve the readability to refer to “Greenland Ice Sheet (GIS) mass loss” and then use positive rather than negative numbers. This also avoids the technical error of saying that a negative number ’increases’.
Yes, it is better.

p.185.l.22: “ice block”
Done

p.185.l.28: “Thus” here implies that the variation in summer melt rates is a logical result of the previous statement: “melting intensity is hard to measure accurately...”, but this is not the case.
Done

p.186.l.23: “used a fixed geometry”
Done

p.186.l.28: “2D flowline geometries” might be better here? When I first read this, I had the impression of the geometry being both 2D and simplified.
Done

p.189.l.3: “quantified by a scalar damage variable (D)” or “scalar variable D called the damage variable”
Done

p.190.l.19: I think it’d be clearer to state that the “total depth-integrated flux through the inlet boundary” is kept constant (if this is indeed the case, this is what I infer from your description here).
Definitely. Thanks.

p.190.l22-24: This sentence is a little unclear (multiple ’i.e.’s should be avoided, in my opinion).
Done

p.191.l.4: lower-case “compared”
Done

p.192.l.3: to me, the word “runoff” here implies water flowing supraglacially and falling into the fjord, as opposed to subglacial drainage into the fjord.
We changed the sentence into “[...] entering the fjord and mixing with fresh and cold subglacial freshwater flow”.

p.192.l.9: 0.6 to 3.8
Done

p.192.l.16: I think you are implying here that you also use a linear variation of melt from 0

at sea level to MMR at the base, but you should explicitly state this.
OK.

p.193.l.11 and p.193.l.14: Is it appropriate to state that ice mélange "freezes" or "melts"? Given that almost the entire mass of the material is made from icebergs rather than sea ice, I would think "freezes together" or "becomes rigid" would be more appropriate. As for the disintegration, satellite imagery shows that the material tends to lose its rigidity and then flow out of the fjord in pieces, rather than melting. The use of "collapse" or "disintegration" as opposed to "melting" also avoids any confusion with discussion of undercutting by melting.
Done.

p.193.l.17: "did not obtain"
Done.

p.193.l.22: Here you switch to a different convention for reporting ranges. In the melt section you use (x m day to y m day) but now you use [x;y]MPa. I think consistency would be better.
Done.

p.193.l.25: Why under tension? Surely if you're considering the effect of mélange, the system is under compression?
We meant that 1 MPa should be seen as an order of magnitude for the upper bound. It depends on temperature, salinity, etc. and these parameters are poorly constrained for the ice mélange.
In addition, we did not notice significant changes in results when increasing the back stress above 1 MPa, except a degeneration of the mesh around the interface between ice front and mélange layer.

p.194.l.3 "of time"
Done.

p.194.eq.5 I don't think it's necessary or useful to have the "(mod 365 days)" in each case. Also, it took me quite a while to work out what was going on here; it might clear things up to mention in brackets on p.193.l.18: five months (150 days).
Regarding the modulo, we think that using (mod 365 days) avoids any confusion.
We mentioned the values in days as well (150 days).

p.194.l.19: "by more than a few tens of meters"
Done.

p.195.l.23: "cumulative loss" or "total loss"
Done.

p.196.l.13: The "position"/"advance" of the terminus was inversely correlated with velocity.
Done.

p.196.l.19: as it is, this seems to imply that there was an increase in velocity in the control

run, but runs S2-4 increased more. I think what you intend to say is that the ice flow accelerates to a faster speed than the maximum in the control run?
Yes. Your sentence is clearer.

p.196.l.23: "parallel to sea level"
OK.

p.197.l.5: The wording here implies that the maximum velocity observed by Walter et al. was 550m/a, as opposed to the magnitude of the speedup being 550m/a.
Done.

p.197.l.10: "does not have"
Done.

p.197.l.22: I don't think it's correct to say that no previous study has suggested an alternative effect of mélange on calving dynamics. To my knowledge, most studies investigating the effect of mélange (including our own) infer that ice mélange acts to reduce crevasse propagation and thus prevent or inhibit calving, rather than simply prevent the rotation of an already calved iceberg. Furthermore, you mention on p.186.l.12 that some authors argue that ice mélange directly resists ice flow. This would seem to contradict the statement that "no study has suggested another effect"
We deleted this sentence: it did not add any substantial information to the study.
p.198.l.18: "ice mélange event" to me implies that this is a short transient occurrence, whereas it actually lasts › half the year. Perhaps "during the ice mélange season"?
Done.

p.198.l.24: "propagation to sea level"
Done.

p.198.l.25-29: The description here seems like Figure 10 was previously oriented 90 degrees from its current orientation, and the description hasn't been updated.
Absolutely. We changed the description. Thank you. In addition, panel (a) and (c) were inverted, as suggested by reviewer #2.

p.199.l.8: I don't think it's correct to say the glacier "undergoes" ice mélange. Perhaps "when the ice mélange layer is present"?
Done.

p.199.l.21: The use of the word "directly" here may cause some confusion if it is interpreted as being the opposite of "inversely".
OK.

p.199.l.27: "although they simulated… … longitudinal extent of the crevasse field near the front".
Done.

p.200.l.3-5: Related to my comment on p.197.l.22, this seems to imply that this is a new hypothesis on the role of ice mélange, which is not the case.

We think that this point is quite different from your previous comment. Here, there is no remark regarding previous modeling work. Our model only suggests that the mélange layer prevents the fracture propagation at sea level. On the other hand, we showed that the fracture still initiates its propagation down to sea level, which is quite new, compared to previous modelling studies.

p.200.l.23: Our prescribed melting overlapped somewhat with the presence of the floating tongue, but it would be accurate to say that our "geometry was grounded for most of the melt season"
Thanks, we changed the manuscript.

p.202.l.6, 9: "pluri-annual": again, not sure if this is the best word. If used, be consistent with use of hyphen.
We re-used multi-annual (see comment above).

Table 1: "that starts damage" or "damaging"?
Typo. Thanks.

Table 2: "Name of run" or "Run name" rather than "name run"
Done.

Figure 1: Only a minor point: You have Ht and Hwt representing thicknesses of glacier terminus and water column, respectively. Why not have Hm for mélange thickness, rather than 'h'?
This idea behind is that capital "H" refers to glacier or its front when lower case "h" refers to the mélange.

Figure 3: "... melt rate imposed at the bottom surface of the glacier front" implies, to me at least, basal melting. I think it'd be clearer to say "melt rate imposed at the base of the calving front".
Done.

Figure 4: I think (a) should have a scale bar, and an indication of vertical exaggeration (if there is any).
The glacier is about 6000m long and 600m thick. There is no vertical exaggeration. We added these information in the caption.

Figure 6:
- Three of the model setups listed in Table 2 appear to be missing.
- "Cumulative ice loss"
- I am confused by the combination of "cumulative over 5 years" "summer vs. winter" and "normalized daily". Perhaps you could lay out more explicitly (here or in the results section) how these data were produced.
We changed the caption into: "Daily average ice loss over the winter and summer seasons for the five years of the simulation for the setups listed in Tab. 2 (experiment U2)." Technically, the volumes were (i) cumulated over the 5-years experiments and (ii) divided by the number of summer and winter days, respectively.
- "The area of the disks represent..." Done.

- The green looks to me like yellow, but perhaps it's just me.

Figure 8:
- As with Fig. 6, 3 of the model setups are missing.
We changed the caption into: "Daily average ice loss over the winter and summer seasons for the five years of the simulation for the setups listed in Tab. 2 (experiment M2)."
- Is it possible to remove the vertical lines from within the circles, as they don't serve any purpose here? Not really...
- "Cumulative" Done.

Figure 10:
- This may be slightly pedantic, but the x-axis label "front position" is only valid for (a). When applied to (b) and (c), it implies that we are not seeing profiles from a single instance in time, but rather that each curve represents the change in Ki and χ as the front advances (or retreats). Instead, I think the x-axis label should be "distance along flowline" and a legend should indicate that the black line in (a) represents front position.
Yes. Thank you for this suggestion. The modification was done.
- I don't think the glacier "undergoes" an ice mélange. Perhaps "for a glacier subject to an ice mélange backstress of..." Done.
- It might be easier to follow the caption if you had "Red diamond: day 46 (...), Blue Diamond: day 140 (...), ...". Done.
- "solid curves" rather than "thick curves". Done.

Figure 11:
- As with Figs 6 and 8, there appear to be three simulations missing here.
- To me it seems counter-intuitive to have 11(a) y-axis as (CR – U2). Given that you are comparing a perturbation to a control run, and you discuss an increase in Sxx for the U2 case, why not present the data as (U2 – CR)? Done.
- In the caption for (b), I think you could clarify: "Ratio between summer and winter event frequency (circles) and event size (crosses). Done.

Figure 12:
"Front advances because of increased velocity" Done.
"Front advances because of calving cessation" Done.

Reply to Referee #2

Before all, we would like to thank Martin Truffer for reviewing carefully our article and making suggestions which improved the quality of the manuscript. We took them into account and our point-by-point answer is given below, in red. When a remark deserved a specific revision in the manuscript, we modified it accordingly (using red color).

General comments

1) The geometry chosen is one with a positive bed slope, when it is well known that many of the interesting phenomena on tidewater glaciers occur on negatively sloped beds. What motivates this choice? Will the model simply lead to too many instabilities?

The objective of this article is to test the robustness of our modelling (melting and ice mélange forcings) onto many different geometries. To do so, setup have to exhibit a quasi stable position, which was only possible when dealing with prograde slopes. Studying the behaviour of retrograde sloped glacier would be interesting, but it would require case-by-case approach on specifics glacier configurations, which is out of the scope of the present study.

We add a specific sentence in the manuscript that motivates our choice. (Sect. 3.1)

2) I think floating tongues are not properly treated here. On temperate glaciers they are rarely observed. The reason, I believe, is hinted at in the paper: under floating tongues the melt rates are so high that it would quickly become unstable and fall apart. If near-glacier fjord circulation is driven by subglacial freshwater discharge, as seems reasonable, then the near grounding line melt rates would have to be very large. I therefore question the choice of a floating-tongue glacier as the control run; it is likely not a good representation of reality.

In our experiments, there is one Control Run (CR) per geometry. The CR is not forced by any mélange or melting (see Table 2 and Sect. 3.2)

Fig. 4 is not a Control Run, but an illustrative example only. This point was added in its caption. We chose this geometry in order to define the grounding line position X_G.

The absence of submarine melting in our modelling is a limitation (as we wrote at the end of Sect. 3.2.1), but this choice was motivated by the fact that the objective was to compare the different simulations. A deep analysis of the submarine distribution would have introduced supplementary differences between glaciers and would have make their comparison harder.

3) The basal boundary condition should be explicitly stated, rather than just via reference to Krug et al. (2014) (The flip side is that it made me go look at that paper, which is

impressive work!). Again, a lot of interesting tidewater glacier behavior is tied to sliding laws that are effective pressure dependent and thus lead to acceleration upon thinning (e.g. Pfeffer, 2007, JGR). Perhaps it is not so relevant in the positive bed slope, but it is important enough to clearly state in the paper.

Thank you for your appreciation of Krug et al. (2014).
This comment is similar to the one of reviewer n°1. We expanded the description of the damage and calving model in Sect. 2.2.
The basal friction is kept linearly decreasing along-flow, and it is the same for each simulation. We did not study the feedbacks on basal friction arising from changes in front position. Studying its effect would be interesting, but in this case, we should focus toward a realistic approach, rather than a global and theoretic approach.
We explained the formalism of the friction coefficient in Sect. 3.1.

4) Is it important that the upstream flux is fixed? I am wondering whether the glacier can be 'supply starved', i.e. higher terminus fluxes would occur if the glacier could supply them? This is perhaps outside the scope of this paper, but is an important question for ice sheet evolution.

We made several tests for this boundary condition (with constant velocities and constant fluxes). We did not observed any sensitivity in our conclusions related to these tests. Thus, for sake of simplicity, we chose to keep the inlet flux constant.

Line-by-Line Comments

Below is a list of detailed comments in order of occurrence:
p.184, l.16: heaviest -> strongest
Done

p.184, l.21: delete the second 'pathways'
Done

p.185, l.13: what do you mean by 'unavoidable mechanism'?
We meant significant. The word has been changed.

p.185, l.15: there is no force imbalance, is there? Forces are always balanced. What are feedbacks between ice discharge and ice flow, isn't that the same thing?
We meant the feedbacks between the calving processes and ice dynamics. We changed our sentence.

p.189, l.16: Can you specify exactly how calving happens? If a crevasse penetrates to full depth, that ice becomes part of the melange and the glacier boundary jumps back, correct? This might be nice to clarify in light of the discussion 5.1 where a comparison with Amundson et al's results are made. Those authors calculated a force balance on an ice berg that is fully separated from the glacier, but prevented to rotate by the ice melange.

We expanded calving model description, as stated before.
Calving processes and ice mélange are completely independent. When an iceberg calves, the

boundary “jumps” back. The new iceberg does not contributes to the mélange. This is why the comparison with Amundson was limited to the order of magnitudes only. We do not compare our processes. The manuscript was modified to improve the explanation (see Sect. 5.1).

p.190, l.7: It might be worth mentioning shortly what LHS is or give a reference.
Done

p.190, l.19: This sentence is hard to understand. Are you simply stating that the up-stream boundary is that of a constant flux?
Yes. We clarified it.

p.192, l.6: Motyka et al. (2013, GRL) is also a good reference for showing the dependence of melting on subglacial discharge
You are right.

p.194, eqn (5): The onset of freezing on Jan. 1 seems rather late, no?
Yes. In fact, as there is no dependancy between melting experiments and mélange experiment, the starting date of each forcing does not really matter. But for clarity, we changed the Fig. 2. Of course, it does not impact our results.

p.197, l.10: ’do not’ -> ’does not’
Done.

p.199, l.8: I don’t understand that sentence (’when the glacier undergoes the melange layer’)
It just mean: “when a mélange layer is present”. The sentence was changed accordingly.

Fig. 6 and 8: What is the daily normalization you refer to?
The length of the forcing period (mélange or melting) is always shorter than the rest of the year (4-5 months compared to 8-7 months). Thus, in order to compare the ice losses, we divided the quantity of ice lost by the length of the corresponding period. This is why we called it a daily normalized period.
For mélange experiment, Winter = 5 months and 20 days; Summer = 6 months and 10 days
For melting experiment, Winter = 8 months; Summer = 4 months

However, we simplified the descriptions of these captions:
Fig. 6: “Daily average ice loss over the winter and summer seasons for the five years of the simulation for the setups listed in Tab. 2 (experiment U2).”
Fig. 8: “Daily average ice loss over the winter and summer seasons for the five years of the simulation for the setups listed in Tab. 2 (experiment M2).”

Fig. 10: why the strange upside down order? What does ’undergoing an ice melange’ mean?
We inverted panels (a) and (c). The upside down order had no reason for being.
“undergoing an ice melange” just means: “when a mélange layer is present”. The caption was modified into “a glacier forced by an ice mélange”.

Fig. 11: the caption could be clarified a bit. Currently it only becomes understandable together with the text. What is a 'ratio between summer and winter events' or 'the mean length of the front retreat ratios'?

"The ratio between summer and winter events" corresponds to the ratio between the frequency of calving event occurring during summer and the frequency of calving events occurring during winter time.

"The mean length of the front retreat ratios" corresponds to the ratio between the length of calving front retreat occurring during summer and the length of calving front retreat occurring during winter time.

We changed the caption accordingly to your and reviewer #1 comments.