Anonymous Referee #3

Interactive comment on "Glacier dynamics over the last quarter of a century at Jakobshavn Isbræ" by I. S. Muresan et al.

This publication attempts to simulate with a 3d-flow model the recent rapid retreat and dynamic changes of Jakobshavn Isbrae and thereby explain the causing processes. Working towards larger scale flow models that capable of reproducing the dynamic changes of outlet glaciers in Greenland is crucial for more realistc future projections of ice sheet mass loss and ultimately sea level rise and in this respect the application of PSIM to model the retreat of Jakobshavn certainly timely and relevant. Given that this is really a first' attempt of using such a large scale model on a basin scale and with a fully dynamic terminus treatment, there are some interesting aspects and findings of this study, in particular from a modelling point of view (e.g. roughly right retreat pattern and mass loss despite relatievly coarse resolution) and I think is valuable even if not perfect. However, there are unfortunately a lot of issues in this manuscript which concern the explanations of the methods and in particular the discussion and interpretation. The discussion on the causes and processes related to the rapid retreat are in my view neither supported by the presented modelling results nor in line with existing literature/understanding. I described the major issues and also more minor technical comments in some more detail below. As a whole, and although in principle the work presents valuable aspects from a modelling perspective, the current version of the manuscript is in general lacking quality, is weak in the discussion and looses itself in trying to explain processes that are not supported by the modelling results. It is therefore as a whole not convincing and at times even misleading. I think this manuscript requires very substantial reanalysis and rewriting before being publishable but in general I think it is important to advance such modelling attempts (even if they are not perfect yet).

Authors: We appreciate the reviewer's comments and have made significant changes throughout the manuscript. A more detailed response is given below.

Major comments:

Focus of paper and conclusions In general, the basic attempt of trying to model/reproduce the retreat and compare/evaluate it with observations is useful and probably on its own enough for a paper but it should more carefully analyse and discuss and illustrate and support the arguments more effectively with figures of the model results. A stronger focus should perhaps be given to the general retreat trend/behaviour and it should better include/integrate the forcings and better consider and discuss it in context of earlier suggested causing mechanisms (thus better link to literature).

Authors: Significant changes have been made throughout the manuscript, especially in the discussion. The forcings and the sensitivity of the model to different parameters have been considered and are now included in SI, sect 1.

Currently the discussion looses itself in the detail of the 2012 speedup event with an explanation that is beyond the ability of the model and therefore unconvincing and misleading. Also the discussion on the seasonal flow variations is one sided on surface melt regarding forcing and alternative more convincing mechanisms (melange, ocean, calving rterat feedback,. . .) are only vaguely considered and the results

not shown well. In the discussion of the causes for the dynamic changes, I had the impression this paper almost ignored the research of the last 10 years and comes up with some rather confused explanations that cannot really be linked to the presented modelling results.

Authors: We agree and have changed the manuscript accordingly.

Methods, model description and forcing

The model description and in particular the calibration and forcing of it are not always clear to me, in particular:

Calibration of model: This should be better explained, currently it seems just a lot of experiments with different parameter settings have been run and the best fit (by eye and chance) been picked. But it is not clear which parameters have been varied for calibration of retreat, which ones relate to the flow model, which ones rather to the forcing and how the potential parameter space has actually been selected.

Authors: This is now shown in SI, section 1 (Table S1-S2) and page 2.

SI, sect. 1 (page 2, L10-24) is changed to:

"The PISM parameters are described in detail by The PISM Authors (2014), Winkelmann et al. (2011) and Aschwanden et al. (2013). We perform 50 simulations in which we vary during the regional runs different parameters with a focus on E_{SIA}, q, δ, F_{melt}, H_{cr} and T_o. The parameters or rather the range of the parameters (min, max) is shown in Table S2, 4th column. In order to match the overall retreat trend the parameters F_{melt}, H_{cr} and T_o were altered first. However, a finer tuning was required to match the observed front positions and to capture the two accelerations (i.e. 1998 and 2003) within the observed time frame. This fine tuning was done by altering some of the parameters that control ice dynamics (E_{SIA}, q, δ).

From the simulations, we present the parameterization that best captures the full evolution of JI during the period 1990–2014: (i) in terms of observed versus modelled front positions for 1990-2014 and (ii) based on the correlation between observed and modelled mass changes during 1997-2014. While (i) is based on our visual interpretation, for (ii) we selected those simulations within a +/- 30 Gt threshold. We found 3 simulations to satisfy (i) and (ii). From these simulations, we chose only the one that captures the two accelerations in the observational record within a 1 year time frame difference and that has overall magnitudes similar with those in the observational record (i.e. the RMSE in point S1 is ~2236 m a^{-1}; see also Fig. 3). It seems relevant to highlight that all the simulations are able to capture the two accelerations of JI. "

Furthermore, to get a better overview of the sensitivity of the model to different parameters and to relate with the parameters that have been varied (e.g. for calibration of the retreat, which one relate with the flow, forcing etc.) a new section has been added in SI, sect. 1.2.

Could it be you get the 'right' behaviour for a very unrealistic forcing? Also the forcing (with time) is not well illustrated.

Authors: No. With unrealistic forcings (e.g. specifically the oceanic forcing as the dynamic changes at JI are triggered at the terminus) it is impossible to get a good fit to observed terminus position, mass loss estimates and timing of the accelerations. Fig. S6 shows the sensitivity of the model to the input oceanic temperature, and Figs. S5 and S12 show the sensitivity of the model to climatic forcing.

The climatic and oceanic forcings are better illustrated and discussed now in SI, section 1.2.5.

Similarly, it is not clear to me how the initial geometry has been built-up/created. Was the front position fixed or freely evolving when creating the initial state?

Authors: For the initial state (i.e. the paleo-spinup – whole GrIS simulation) the front position is held fixed to the 1990 observed position (the paleo-spinup procedure is similar with Aschwanden et al. (2013)).

"In our model, the three-dimensional ice enthalpy field, basal melt, modelled amount of till-pore water, and lithospheric temperature are given as simulated in a whole GrIS paleo-climatic spin-up. The paleo-climatic spin-up follows closely the initialization procedure described in detail by Bindschadler et al. (2013) and Aschwanden et al. (2013)."

We have added:

*"The paleo-climatic spin-up follows closely the initialization procedure described in detail by Bindschadler et al. (2013) and Aschwanden et al. (2013). **It is important to note that during the paleo-climatic initialization the terminus is held fixed to the observed 1990 position in the JI region, and to the present-day position elsewhere.**"*

The front is only allowed to freely evolve during the regional JI runs, i.e. the equilibrium simulation and during the forwards runs (5L28-30):

"The calving fronts and grounding lines are free to evolve in time both during the equilibrium and the forward simulation."

Regarding the initial geometry, an explanation/discussion of why the front is almost entirely grounded and has no 10km floating tongue would also be important.

Authors: We agree. The following has been added in the discussions (sect. 3.2):

" As introduced in Sect. 2.1.2, our approach here is to adjust the terminus in the JI region to simulate the 1990s metrics. For this reason, the 1990 surface elevation is reconstructed based on aerial photographs and available satellite altimetry observations (Csatho et al., 2008). The glacier terminus in 1990s is known to have been floating (Csatho et al., 2008; Motyka et al. 2011), but details regarding its thickness are not known. Motyka et al. (2011) calculated the 1985 hydrostatic equilibrium thickness of the south branch floating tongue from smoothed surface DEMs and obtained a height of 600 m near the calving front and 940 m near the grounding zone. In this paper however, we choose to use a more simplistic approach in which we compute the thickness as the difference between the surface elevation and the bed. This implies

that our simulations start with a grounded terminus. The geometry of the terminus plays an important role in parameterizing ice shelf melting, and therefore our choice could directly affect the magnitude of the basal melt rates (see SI. Sect. 1.2.8). As expected, the difference in geometry results in modelled basal melt rates slightly larger than those obtained by Motyka et al. (2011)."

Forcing:

how the model is forced with climatic and in particular oceanic data is not clear to me. How do this environmental forcing variables actually impact on the model, in which way, over which process? Is it just surface mass balance and therewith elevation change from it, or is there a coupling of melt water to basal sliding (I assume not), does it in anyway impact on calving?

Authors: It is just surface mass balance and the subsequent elevation change. We do not have any coupling of melt water to basal sliding. Changes in surface elevation (i.e. ice thickness) due to changes in SMB affect both SIA and SSA. In the SIA, this effect is weak as SMB related changes in elevation will not have a significant effect on the driving stress. In the SSA, the coupling is done through the effective pressure term in the yield stress (see SI, Eq. 3). Because the effective pressure is related to overburden pressure (i.e. ice thickness; see SI, Eq. 4), we expect this effect to be much stronger.

The climatic and oceanic forcings are better illustrated and discussed now in SI, section 1.2.5. The parametrization for ice shelf melting is better described in sect. 2.1.3.

Has oceanic forcing actually been varied?

Authors: The input constant ocean water temperature (T_0) is further scaled by the ice shelf melting parametrization spatially and temporally based on the depth below the ice shelf (through the virtual temperature, Tf) and the ocean water salinity. This implies that although the input ocean temperature is constant, the heat flux supplied to the shelf is not constant in time and varies through T0-Tf based on the geometry of the shelf.

More information about the ocean forcing has been added in the new version of the manuscript (see sect. 2.1.3) and in SI, sect. 1.2.5

Importantly, if the dynamic behaviour is investigated for potential causes and forcings it would be vital to also show these forcings against some representative variables of dynamic change (ocean/air temp, oceanic/surface melt along front position, calving rate, flow speed, thinning, . . . and with time). Right now, it is almost impossible to relate forcing to the dynamics and hence the discussion on potential forcings and triggers can not be evaluated and followed by the reader and is therefore largely redundant. This is certainly true regarding the short-term velocity variations (peaks, seasonal) and therefore a clearer presentation of results against forcing is needed.

Authors: Done. See Figs. S2, S4, S7, S8, S9, S12, S13, S14.

Oceanic forcing:

related to the above, in particular the oceanic forcing is currently ignored in the analysis and discussion and not shown at all, however, the literature indicates it a as crucial triggering/forcing factor. At least it should be clear what the forcing is, how it changes over time (even if it was set as constant). Overall, for better understanding the modelling results and improving the discussion I would suggest to show the forcing along side some of the response variable.

Authors: The ocean forcing is now explained in more details (sect. 2.1.3 and SI, sect. 1.2.5) and also included in the discussion (sect. 3.2). In our simulations, and similar with previous studies (Nick et al, Vieli et al), the ocean forcing is the triggering factor for JIs retreat. This has been made clearer in the new version of the manuscript.

Calving model:

As the calving is crucial here, because of calving retreat feedbacks and related dynamic speedup and thinning, its functioning and related dependence on model parameters or forcing should be introduced in more detail. Currently it is not clear how the forcing (which seems to be SMB only) actually impacts on calving, is it just through thickness changes near the terminus, why is it so sensitive then? Are there other parameters linked to forcing that play in? in particular I wonder how calving has been made to increase at the beginning (what parameter adjusted, if any?). This link of calving to forcing just needs better explaining and also illustration.

Authors: A better description of the calving rate has been included in the new version of the manuscript (see sect. 2.1.2). The average calving rate (c) is calculated as the product of the principal components of the horizontal strain rates ($\dot{\varepsilon}_{\pm}$), derived from SSA velocities, and a proportionality constant parameter (k) that captures the material properties relevant for calving (see eq. 1 in the new version of the manuscript). Through eq. 1 the strain pattern is strongly influenced by the geometry and the boundary conditions at the ice shelf front. The parametrization for ice shelf melting has been discussed in detail in the new version of the manuscript (see sect. 2.1.3). See also Fig. S9.

We did not adjust in any way (i.e. any dynamic related parameters) that could increase the calving rate at the beginning. The calving rate is driven by the strain rates which further depend on the evolution of the geometry and the boundary conditions in the terminus region.

Further, the eigenvalue-calving has been developed for large floating ice tongues or ice shelves in Antarctica, so it is not obvious that for the case of a narrow outlet (3-max 5 grid points wide) and actually works for a close to or fully grounded front. For this reason this should be mentioned and, the 'functioning/performance' of this model should be analysed and discussed further in the discussion.

Authors: The calving law is better described in the new version of the manuscript (sect. 2.1.2) and briefly discussed in the discussion (section 3.2). However, the eigen calving performance should not be analyse *alone but rather together with the shelf parametrization. The eigen calving rate will be close to 0 for a grounded terminus.*

This would also require a clearer presentation of retreat positions and calving activity with time (which would clarify a lot of things). Looking at the very strong temporal variations in flow speed in fig 3, unless there is any direct coupling of melt water with basal sliding (which would be questionable as well), I would think these can only result from phases or large events of rapid calving and the related terminus retreat (and reduced buttressing). If so (and from given results I see no other possibility) this means calving is really at the heart of the dynamic changes and needs therefore to be analysed and discussed in detail (also on its link to the forcing). Again a calving rate, retreat, speed plot against time would help).

Authors: A better discussion regarding sub-annual terminus retreat, variations in flow and driving stresses has been included in the discussion section and SI (see sect. 1.4). A clearer presentation of retreat positions calving activity and driving stresses with time is shown in Fig. S7, Fig. S8 and S9.

The climatic influence is shown in Fig. S12 top vs. bottom. In a simulation with constant climate the 2003 retreat is delayed by approx. 1 year and the retreat of the front is chaotic with no clearly defined seasonal peaks.

Model agreement with observations

The authors claim that their modelled retreat agrees in general well with the observed changes in flow and geometry. Although for mass loss and to some degree velocity changes the trends seem to fit, pretty well, other aspects of the the model results are actually rather different to the observations, or the relevant observations are not really used, or the observations and modelling not shown in a way that they can be compared: âA˘ c The extent of fast flow is not reaching ´ far enough inland and width of fast flowing trough seems far too wide (fig2), âA˘ c Re- ´ lated, the front position seems according to fig 6, not really to match the observations (fig. 6 B) âA˘ c The initial geometry looks very different (grounded thick tongue rather ´ than 10km floating tongue). This needs discussing/explaining (see Csatho 2009) âA˘ c´ There is actually quite a bit of additional velocity data existing (several Joughin etc.. . ..) in particular also earlier from before 2002 (Joughin 2003, Echelmeyer 1994. . .) âA˘ c´

Authors: We disagree with the comment that "other aspects of the model results are actually rather different to the observations". In general our model fits observations very well. A perfect match, e.g. in terms of the flow velocity shape requires inverse methods to infer the basal friction parameter from observed surface velocities. Note that we are doing forward simulation runs from 1990 to 2014, 25 years during which the shape and the speed of the glacier changed dramatically (the shape during the '90s, before the 2000-2003 break-up and the shape today).

In the new version of the manuscript and SI, the observations and modelling are shown in a way that allows for a better comparison. A new Fig. 6 has been included where both geometry and speeds are shown on the same plot. A discussion regarding the initial geometry used (we start our simulations with a grounded terminus) and its implications is now included in discussion (sect. 3.2).

How do modelled and observed surface topo compare? I guess rather poorly initially and near front. âA˘

Authors: The surface elevations and elevation changes compare well with observations, but of course they don't fit perfectly, e.g see Fig 3.

c Front positions with time: how do modelled compare to observed? ´Fig. 2 only shows observed front and modelled grounding line so one can not compare!!! Maybe a plot of front position with time would be useful (along flow speed variations. . . and forcings (temp. . .)). âA˘ c

Authors: Fig. 2 shows both observed and modelled terminus position. For clarity we have added a black line to indicate the terminus position. For front position evolution with time see Fig. 6 and See SI, Fig. S7.

The bed used here (even if from new BAM- ´ BER dataset) appears, according to fig. 6, very different to the earlier CRESIS dataset and may explain some of the discrepancy in geometry and velocity response. Is this an issue of grid resolution that the 1300m trough near the current terminus disappeared. Or is it from some 'adjustment' as mentioned in text? The bed is potentially crucial for the dynamics. Thus, the bed-data should at least be discussed and taken into account in explanation of 2012 speedup âA˘ c The specially treated 2012 speed up seem when ´ looking at the modelling results in fig 3 not a 'special' speed up, there are many similar modelled speedups in earlier years.

Authors: The bed topography and its implication for ice dynamics is now included in the discussion and SI, section 1.3.2. Unfortunately, the bed profile shown in the original version of the paper was misplaces. We have updated the bed profile and it is now fully consistent with the profile shown in e.g Joughin et al., 2014, Nick et al., 2013 (nature). Please refer also to the comments submitted to the other referees regarding this issue.

From a first attempt of a large scale fully dynamic model and of coarse resolution (2km) I think one can not expect a perfect fit and issues with initial geometry etc. are understandable and the modelling study is even if not fully fitting really useful. However, the authors should be more clearly communicate and discuss as uncertainties and issues.

Authors: Changed accordingly.

Discussion of modelling results

I think this is really the major weakness of the manuscript, here it really suffers of mis- and/or over-interpretation of the modelling. The discussion is further unbalanced regarding focus and relevance and lacks a better integration of existing understanding/literature. Maybe from the structural side I would also suggest to not mix up the modelling results and general discussion and separate them. More crucially, the paper results and discussion lacks a clear focus on the points that can really be convincingly addressed with the given modelling framework. Currently, the discussion and conclusions strongly focussed on short-term variations in flow speed (2012 speed-up, seasonal variations) and the forcing by surface melt, but the discussed mechanisms and feedbacks are not really in the model (link of surface melt to flow speed) and the more likely mechanisms not discussed (ice melabge and calving retreat feedback,. . .etc.). Existing literature and knowledge Within the discussion, the current version of the manuscript rather poorly integrates exiting understanding/ideas from literature in for interpretation.

This is not to say that the existing literature is always right, but the authors have not really argued their case convincingly with or against it, and in several instances rather ignored it. For example, -there is a wealth of literature arguing the role of seasonal front variations of the floating tongue explaining short term (seasonal variations) in flow speed but this has not really been taken into account. -short-term speed-up and surface melt relation has been reseach intensively and seems for large and fast flowing outlets not a very big fraction and not sustained over time. -ocean forcing has not been analysed or discussed in this paper, which has been suggested as a major trigger for retreat in literature. -the difference in bed topography used here (compared to other studies) and its effect on the results is not really discussed but crucial -. . .

Authors: All the issues raised above regarding ocean forcing, seasonal variation in flow due to retreat and advance of the front are now included either in SI or in the main manuscript. The discussion has been significantly improved and now includes the suggestions above.

Specific comments:

Title: Regarding the title I think it would be more honest to include in some way 'modelling' as well, in particular as the 'dynamics' part is currently not well analysed and not that convincing.

Authors: We agree. The title is changed to:

"Modelled glacier dynamics over the last quarter of a century at Jakobshavn Isbræ"

p. 4867: line 23-24: here and in the discussion of dynamics the modelling investigations of Jako from Vieli and Nick (2011, Surveys in Geophysics) maybe useful.

Authors: We found the paper very helpful.

p. 4868 line 18: I would add '. . .and forcing' or maybe make a subsection 2.2 of atmospheric forcing. Certainly in this section the forcing needs to be explained better.

Authors: Changed accordingly.

4869 line 13 onwards: is this really a stable state? It is not clear to me how this stable state has been found (is not trivial), in particular whether the terminus is allowed to evolve freely when finding this stable state or whether is has been fixed. Is it also stable after switching to 2km resolution? Needs more explanation. The discrepancy to the observed geometry needs also be discussed (here or in discussion). Csatho et al 2008 shows a very different geometry beck to the 1950s which is clearly floating and far from modelled thick and grounded tongue (for 1944 Csatho indicates some grounding though). (even when terminus is allowed to evolve freely)?

Authors: Stable in the sense that after our standards the equilibrium has been established as the ice volume in the regional domain changed by less than 1 %. Check also the thickness changes (Fig. 3, bottom) at the beginning of the forward simulation. For the initial state (i.e. the paleo-spinup – whole GrIS simulation) the *front position is held fixed to the 1990 observed position (the paleo-spinup procedure is similar with Aschwanden et al. (2013)).*

We have added text in bold (sect. 2.1.2):

"In our model, the three-dimensional ice enthalpy field, basal melt, modelled amount of till-pore water, and lithospheric temperature are given as simulated in a whole GrIS paleo-climatic spin-up. The paleo-climatic spin-up follows closely the initialization procedure described in detail by Bindschadler et al. (2013) and Aschwanden et al. (2013). ***It is important to note that during the paleo-climatic initialization the front positions are held fixed to the 1990 observed position in the JI region, and to the present day position elsewhere.*** *"*

The front is only allowed to freely evolve during the regional JI runs, i.e. the equilibrium simulation and during the forwards runs. The text reads:

"The calving fronts and grounding lines are free to evolve in time both during the equilibrium and the forward simulation."

A discussion regarding the initial geometry used (we start our simulations with a grounded terminus) and its implications is included in the new version of the manuscript (please see the new discussion section).

p. 4870 line 4: the information on the bed data needs to be extended, the bed in Fig. 6 looks pretty different to the Cresis data I have seen before. Where has the trough gone, is this just a matter of limited grid resolution and where the profile is located for visualization. Or is it due to the additional adjustment of the bed mentioned on line 5-8? It seems rather odd to adjust the bed to get the right surface, I would rather expect to change some model parameters to get the right surface (unless the bed is not know). And anyway the initial surface seems seems pretty off. The rapid deepening into the trough around the front in recent years appears in the cresis data pretty well (see their data or for example joughin 2014) and has a surface expression in a steep slope of the surface before the floating tongue (see Csatho 2008) but again I struggle to see it in the profile data in Fig 6. Maybe a clear map of bed topography would help.

Authors: No. It is not due to any adjustment. The adjustment has been made to the input geometry, but only to the surface elevation, not to the bed. This has been clarified in the new version of the manuscript.

Regarding the trough, it is still there. Unfortunately, the profile shown in the original version of the paper was misplaces.

p. 4870 line 13-14: so how is RACMO downscaled for the 2km surface grid of Jako, in particular on the tongue? This is crucial as this seems the important forcing driving all the changes.

Authors: We have added:

“The version used in this study is produced at a spatial resolution of ∼ 11 km and is extended to the end of 2014, to cover the period 1958–2014. The original dataset of 11 km grid is interpolated to 2x2 km grids using bilinear interpolation.”

C2048 p. 4870 line 19: I am not sure the ‘till pore water’ makes here any physical meaning here, I guess it is roughly representing a sliding coefficient that has some value. What would be useful, is however, to know whether it is in any way affected by external forcing (e.g. surface melt etc. . .), or a sliding conditions constant with time?

Authors: Please see SI, sect. 1.2.3, equations 3 and 4. The “till pore water” is not affected by the external forcing (i.e. climatic forcing). The effective thickness of the water in the till (SI, eq. 4) is computed by time integrating the basal melt rate, which in our model is not connected with surface melt. The only way the dynamic part is influenced by the climate is through elevation changes via SMB.

We have added in the SI, sect 1.2.5:

“In our model, the climatic forcing applied can influence JI’s dynamics only through changes in surface mass balance (SMB) (i.e. accumulation and ablation). Changes in surface elevation (i.e. ice thickness) due to changes in SMB affect both SIA and SSA. In the SIA, this effect is weak as SMB related changes in elevation will not have a significant effect on the driving stress. In the SSA, the coupling is done through the effective pressure term in the yield stress (see Eq. 3). Because the effective pressure is related to overburden pressure (i.e. ice thickness; see Eq. 4), we expect this effect to be much stronger.”

And in the main manuscript, (14L13-15):

“In our model, the climatic forcing applied can influence JI’s dynamics only through changes in surface mass balance (SMB) (i.e. accumulation and ablation) (see SI, Sect. 1.2.5).”

p. 4870 bottom/ p. 4871 top: this model has been developed and tested for relatively large floating ice tongues (shelves). so not it is not obvious that it works for the narrow and towards the end mostly grounded jako-front. Also can external forcing directly impact on calving (if so how?). see general comments.

Authors: The calving law is better described in the new version of the manuscript and briefly discussed in the discussion section. However, the eigen calving performance should not be analyse alone but rather together with the shelf parametrization. Without the shelf parametrization the eigen calving rate will be close to 0 for a grounded terminus.

p. 4872 line 17: -1.7 degrees seems very cold for Greenland and at what depth is this. This is way below the observed water temperature at depth in Disco bugt (min +1.5 degrees Celcius). Anyway needs more explanation on ocean forcing, is it varied with time, how much, forced by what, and show it.

Authors: Although -1.7 °C is the input ocean temperature, this temperature is scaled based on a virtual temperature T_f which depends on the ice shelf geometry. Please see sect. 2.1.3, equations 3 to 6. The so called “scaled temperature “ (T_0-T_f) is showed for different ice shelf thicknesses in SI, Fig. S4 and it can be e.g. around 0.3 °C for a shelf with an elevation at the base of - 100 m and around 1 °C for a shelf with an elevation at the base of 1000 m.

The min.+ 1.5 °C temperature is in Disco bugt and not inside the fjord, near the terminus or at the base of the shelf.

p. 4872 line 1: I would suggest to keep results and discussion separate and structure clearer (general longer-term behaviour, seasonal„ events. . ..).

Authors: We agree. Therefore some changes and adjustments have been made to the new version of the manuscript (See the new sect. 3). We think that the structure has become clear now.

p. 4872 line 7 section 3.1: this whole section is very limited on the fit of general retreat dynamic behaviour (how and whay it matches) and in particular the link to forcing is not discussed/shown. The bulk of this section is on the 2012 speed-up event which is highly speculative and the argumentation not convincing and pretty ignorant regarding previous research/understanding. So the title of this section is currently not appropriate.

Authors: The title has been changed.

p. 4872 line 8: Which parameters were calibrated to match retreat trend, are these mostly the ones related to the forcing? This really needs better explaining, how has this calibration been done, which knobs turned, in particular also for getting the right initial state.

Authors: We have added (see SI, section 1.1 page 2):

"In order to match the overall retreat trend the parameters Fmelt, Hcr and T0 were first altered. However, a finer tuning was required to match the observed front positions and to capture the two accelerations (i.e. 1998 and 2003) within the observed time frame. This fine tuning was done by altering some of the parameters that control the ice dynamics (ESIA, q, δ)."

Please refer also to SI, sect. 1.2 Sensitivity experiments for parameters controlling ice dynamics, basal processes and ice shelf melt.

p. 4872 line 13-14: I would be interested how these melt rates actually vary along flow (below gfloating part). Am I right that the melt rate is only applied below floating ice (meaning ice removed vertically)? This means the ocean forcing influence is gone when front fully grounded.

Authors: Yes. But note that the calving front is never fully grounded (please see Fig. 2 and Fig. 6) (i.e. it does not have a complete vertical grounded face).

For melt rates please refer to SI, Fig. S9 and Table S3.

p. 4872 line 19: looking at figure 3 I think the model actually captures some of the speed up pretty well, but importantly also shows similar such speed ups in other years. So the authors really should try and understand what these short-term speed variations are, rather than trying in length to explain what the model can not show anyway (surface melt 2012). These speed variations seem most likely related to rapid calving or events and related retreat and loss of buttressing but without showing any of such variables one can only speculate.

Authors: We agree. These short-term speed variations are discussed in SI, sect. 1.4. The deviatoric stresses are shown in point S1 and along flowline in Figs. S7 and S8.

p. 4873: see related comments before, this whole discussion is speculative and not supported by the modelling results presented here and in several places demonstrates rather poor understanding of how outlet glaciers dynamically work. Other potential mechanisms than surface melt (ice melange, front variations and calving,. . .Amundson 2010, Joughin 2008, . . .) should be discussed here as well.

Authors: Done. See the new discussion.

Line 5-6: is this hydrofracturing in model? Does it impact on your modelled calving? How?

Authors: There is no hydrofracturing in the model.

Line 13: a warm summer may cause enhanced surface melt but the resulting difference in surface melt does not produce significant changes in slope (it melts more everywhere, at best the slope changes by a meter over several kilometres). The steepening may happen in the model but due to dynamic effects (rapid retreat of front, bed topo,. . .) and not surface melt in one summer.

Authors: Changed accordingly.

Line 15 onwards: It seems years of research on the effect of melt water and ice flow of outlet glaciers has not been well been integrated/absobed. the whole line of argument and explanation seems to me not convincing and rather confused.

Authors: Not included in the new version of the manuscript.

p. 5874 line 7: not clear if this is data produced/compiled by this study. If so the method should be explained in the methods.

Authors: The observed data set is produced (compiled) by this study. The method was explained in SI, sect. 2. We have added in the main text:

*"*We estimate the rate of ice volume change from airborne and satellite altimetry over the same period and convert to mass change rate (**see SI, Sect. 2 for more details**)*."*

p. 4874/4875 section 3.2: this section is going through the different stages of erterat and discusses the behaviour of retreat/dynamic change (modelled and observed). But the title of the section only refers to seasonal variations. Further while there are some good obsevrations made here and relevant points discussed, the discussion struggles to get to explain what is really going on here. The autors in my view fail to make meaning out of their modelling and actually using their modelling results to try and understand why things change. Thus the potential mechanisms are not really linked with the model results, and the link to forcing is mostly missing as not really shown and cause and effect are often confused not kept apart.

Authors: We disagree. However, the structure of section 3 and the different sub sections has been completely changed in the new version of the manuscript (the same goes with their name).

3. Results and discussion

3.1. Observations vs. modelling

3.1.1. Annual scale variations in velocities, terminus and grounding line positions (in the previous version of the manuscript was sect. Observations vs. modelling)

3.1.2 Ice mass change

3.2. Feedback mechanisms, forcings and limitations (in the previous version of the manuscript was sect. Observations vs. modelling)

p. 4875 line 4-7: so why does it retreat? What exactly is the forcing here and how does it exactly lead to retreat, this needs to be understood.

Authors: The retreat of the front in our simulations is oceanic driven. In the light of the new manuscript, this should have become clear. See also SI, Figs. S5 and S12.

Line 14: the lack of seasonal acceleration actually agrees with observations of Echelmeyer at al (J- Glaciol 1994).

Authors: The reference has been added to the main manuscript.

Line 26: so why does the surface slope increase, why is there thinning in the first place, needs to be explained, probably a result of enhanced retreat/reduced buttressing. . .. P 4876 line 1: why do you get thinning in first place, what is trigger/forcing?

Authors: This section has been partly rewritten. We have added:

"In our simulation, the 1998 acceleration is generated by a retreat of the terminus in 1997-1998, which may be responsible for reduction in buttressing (see Movie 1 and SI, Fig. S7)."

Line 10- 11: you probably mean 'reduction in buttressing' due to a reduction in lateral resistance (van der veen 2010).

Authors: Changed to "reduction in buttressing".

p. 4877/4878: conclusions: overall there are some valid points but the interpretation of the 2012 speedup is overrated and misleading and generalization and wider implications for future behaviour of greenland outlet glaciers derived in my view tentative and even dangerous. The buttressing argument is an important one and in my view a valid point but it has unfortunately not really been elaborated in the discussion and needs to be better illustrated with the model results.

Authors: The discussion and conclusion have been largely adjusted to include the comments above. New figures have been added in SI to sustain our results.

Fig1: I can not see any contours that are mentined in the caption.

Authors: "contours" removed.

Fi2: I dio not understand why modelled grounding lines and observed fronts are shown. This does not allow any comparison between modelled and observed! Further the region of fast flow seems rather wide but not extending enough upstream (fast flowing channel is not really visible).

Authors: We have added the modelled terminus positions. See the new Fig. 2.

Fig 3: some of earlier velocity data (pre-acceleration in 1998) would be useful, see joughin 2003 F

Authors: We now state that "The modelled velocities for 1992 and 1995 are consistent with observed velocities for the same period (Vieli et al., 2011)."

Fig. 6: initial surface profile is odd (see main comments). Also the terminus extent in the velocity plot seems not to agree with the observed. Further, where is the jako trough!!

Authors: A discussion regarding the geometry of the terminus at the beginning of our simulations is included now in the main manuscript (discussion, see sect. 3.2). Regarding the jako trough, see comments above.