

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

Anonymous Referee #3

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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 realistic 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 relatively 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

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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 loses 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).

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). Currently the discussion loses 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 retreat 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 can not really be linked to the presented modelling results.

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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. Could it be you get the 'right' behaviour for a very unrealistic forcing? Also the forcing (with time) is not well illustrated. 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? Would the terminus/glacier be completely stable without any changing forcing? It is not trivial to get the initial geometry to the right place and I am really interested how the authors managed to do so. 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.

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? Has oceanic forcing actually been varied? 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. Oceanic forcing:

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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 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.

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. 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. 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).

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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: "The extent of fast flow is not reaching far enough inland and width of fast flowing trough seems far too wide (fig2), " Related, the front position seems according to fig 6, not really to match the observations (fig. 6 B) " The initial geometry looks very different (grounded thick tongue rather than 10km floating tongue). This needs discussing/explaining (see Csatho 2009) " 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. . .) " How do modelled and observed surface topo compare? I guess rather poorly initially and near front. " 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. . .)). " The bed used here (even if from new BAMBER 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 " 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.

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

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issues.

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 - . .

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

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analysed and not that convincing.

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.

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.

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 it 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 back 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)?

p. 4870 line 4: the information on the bed data needs to be extended, the bed in Fig. 6 looks pretty different to the Cressid 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 known). And anyway the initial surface seems pretty off. The rapid deepening into the trough around the front in recent years appears in the Cressid 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.

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.

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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?

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.

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.

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

p. 4872 line 7 section 3.1: this whole section is very limited on the fit of general retreat dynamic behaviour (how and why 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.

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.

p. 4872 line 13-14: I would be interested how these melt rates actually vary along flow (below floating part). Am I right that the melt rate is only applied below floating

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ice (meaning ice removed vertically)? This means the ocean forcing influence is gone when front fully grounded.

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.

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. Line 5-6: is this hydrofracturing in model? Does it impact on your modelled calving? How? 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. Line 15 onwards: It seems years of research on the effect of melt water and ice flow of outlet glaciers has not been well integrated/absorbed. the whole line of argument and explanation seems to me not convincing and rather confused.

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.

p. 4874/4875 section 3.2: this section is going through the different stages of retreat and discusses the behaviour of retreat/dynamic change (modelled and observed). But

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the title of the section only refers to seasonal variations. Further while there are some good observations made here and relevant points discussed, the discussion struggles to get to explain what is really going on here. The authors 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. 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. Line 14: the lack of seasonal acceleration actually agrees with observations of Echelmeyer et al (J- Glaciol 1994). 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? Line 10-11: you probably mean 'reduction in buttressing' due to a reduction in lateral resistance (van der Veen 2010).

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.

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

Fig2: I did 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).

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

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joughin 2003

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!!

Interactive comment on The Cryosphere Discuss., 9, 4865, 2015.

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