

## **Anonymous Referee #1**

**Review on “Glacier dynamics over the last quarter of a century at Jakobshavn Isbrae” by I.S. Muresan et al.**

The paper presents an application of the PISM ice flow model to the region of Jakobshavn Isbrae. The paper discuss the results of a forward run from 1990 to 2014 that gives the best match to the observations. They identify 2 distinct flow accelerations in 1998 and 2003. A long discussion is devoted to the fact that the model does not reproduce the velocity peak in 2012. This is attributed to the exceptional long melt season. The velocity peak in 2012 (and in 2013) has been previously attributed to the retreat of the calving front to an area of deeper bedrock (Joughin et al., 2014). In his interactive comment I. Joughin already replied that there is no such over-deepening in the bedrock topography used in the paper.

*Authors: Unfortunately, the profile shown in the original version of the paper was misplaced. We have updated the bed profile and it is now fully consistent with the profiles shown in e.g Joughin et al., 2014, Nick et al., 2013 (nature).*

**I agree with this comment and the paper fails to demonstrate that there is a causality relationship between the anomalous melt season and the model not reproducing the observations.**

*Authors: We agree and we have adjusted the manuscript accordingly.*

As stated in the paper “the details of the processes triggering and controlling thinning and retreat remain elusive” (p4867; lines 26-27). And “investigate the processes driving the dynamic evolution and the seasonal velocity variation of JI” (p4868, lines 6-7) is an important motivation of the paper, but there is very little discussion on the processes; the model physics and the forcings are too briefly discussed, so that it is difficult to estimate the robustness of the results and how much tuning has been required to match the observations and how sensitive is the model. Looking at Figure 2 the model does a relatively good job in reproducing the retreat of the front from 1994 to 2014, so I would be much more interested by a detailed discussion on the model physics, the forcing required to obtain this result, and the sensitivity to the parameterisations. The observed seasonality of the glacier front position is at the order of few kilometres which is approximately the grid size in the model (2km). There is a strong sub-annual signal in the model that seems also related to the calving events (but the relation between calving (or grounding line movement?) and variations in the velocity is not really discussed in the paper). So I think discussing the seasonality in the model, especially as I understand there is only a seasonality in the surface mass balance model and not in the ocean forcing, is going too far in the interpretation of the results.

*Authors: We appreciate the reviewer’s suggestions and have updated the manuscript and added a new section to SI that targets “parameters, sensitivity and seasonality”. Please see SI, section 1.*

**In conclusion I suggest to rewrite the paper to discuss the robustness of the retreat of the front and of the timing of the retreat to the different parameterisations of the model and to the forcing. A sensitivity to the grid size should also be added, especially if the authors want to discuss the subannual variability.**

The model physics, the forcings and the sensitivity to the different parametrizations used have been included in SI, sect. 1. A sensitivity to grid size was also included (please see SI, sect. 1).

#### **Detailed comments:**

**Sect 2.1. p4869, line 9 :The grid for the enthalpy is not following the ice thickness? I don't understand the “4000m above the bed”.**

*Authors: Yes it does. The “4000m above the bed” is the limit (maximum) of the computational domain extension on the vertical axis. Because the statement creates a lot of confusion it has now been removed from the manuscript.*

**Sect 2.1. p4869, line 3, and p4870 lines 6 and 7: “adjusted to simulate 1990 metrics”, what does that mean?**

*Authors: this sentence is changed to “The terminus position and surface elevation in the Jakobshavn region are based on 1985 aerial photographs and existing satellite altimetry observations (Csatho et al., 2008)”*

*See also sect. 3.2 (12L20).*

**Sect 2.1.2 :“Along the ice shelf front we apply a physically based calving parameterisation and an ice thickness condition”; This is a key point of the simulations presented here and what is really applied should be discussed in more details.**

*Authors: Done. See the new section 2.1.2.*

**Sect 2.1.3. With the appellation “ocean model component” we understand that there is some kind of dynamic ocean model. But it seems that what is applied here is a parameterisation of the heat flux which at the end depend only the the shelf thickness? So there is no external forcing (and no seasonal forcing) from this side? Again this is a crucial point for the simulations and this is too briefly described.**

*Authors: There is no seasonal forcing from the ocean side (See SI, section 1.2.5). We have replaced “the ocean model component” with “Parameterization for ice shelf melting”. See sect. 2.1.3.*

**In Sect 3.1 give the values for the melt rates as I think this is an important point in the forcing of the retreat.**

*Authors: Done. See SI, section. 1.2.8.*

**Sect 3.2 Seasonal variations in velocities. Seasonal variations are not really described (and as already said I think that this would be sur-interpreting the results). There is more discussions here in the inter-annual variations. Again I think that the discussion should focus on these inter-annual variations and on the retreat of the front and the sensitivity of the model to the parameterisations.**

*Authors: Done. Sect 3.2. Seasonal variations in velocities has been renamed “3.1.1. Annual scale variations in velocities, terminus and grounding line positions” (please see SI, section 1.2.5 and 1.4). The retreat of the front and the sensitivity of the model to parametrization is discussed in SI, sect. 1.2.*

**Figure 3 Bottom. Are the observed thicknesses really in absolute value or have they been adjusted to match the model thickness at the first year? I will be very surprised that after a transient spin-up there is a so good match between observed and model thicknesses.**

*Authors: The observed thickness has been adjusted to fit the model thickness at the first year. This is now stated in the figure caption.*

**In general the figures are poor quality. Revise your colour-scales.**

*Authors: Done.*

## Anonymous Referee #2

Review of, “Glacier Dynamics over the last quarter of a century at Jakobshavn Isbrae” by I.S. Muresan et al., submitted for publication to The Cryosphere.

### Summary

In this paper, Muresan et al. use an open-source, higher-order ice sheet model (PISM) in an attempt to (1) reproduce speed, ice-front position, and dynamic thickness changes observed on Greenland’s Jakobshavn glacier during the past ~15 years, and (2) to contribute to our understanding of the causes for those changes by analyzing the model output. In general, the model does a good job of mimicking the observations, and in this sense, the authors succeed in their first goal. I was less convinced with respect to the second goal, however. Despite multiple readings, I came away with a vague understanding that the ongoing acceleration, thinning, and retreat of the calving front is being attributed largely to an ice-dynamic response (which is no surprise, based on lots of previous work), but it was not clear if that was being attributed to some initial perturbation (e.g., breakup of the initial floating ice tongue in the late 90’s), an ongoing response to the applied climate forcing (i.e., from SMB or from the ocean “model”), or possibly just the result of a longer-term model transient that may or may not reflect real life. In terms of using the model to provide some kind of deeper insight into the physical processes responsible for the observed behaviors, I was a bit disappointed. The general feeling one gets is that the analysis of the model output is limited to what one might discern from looking at actual observations. However, with a model, you have much more available than you do from observations (e.g., you have the full 3d stress and velocity fields for every time step). It wasn’t clear that some of the assertions for cause and effect (e.g. loss of resistive stress followed by acceleration retreat) were backed up by actual analysis of the model output, or if this was merely insight / speculation on the part of the authors. I’d like to be convinced it is the former, and a figure or two (even provided in the SI) to back that up would help.

*Authors: We agree and have updated the manuscript accordingly (see SI, sect. 1.) The sensitivity of the model to different parameters and loss of resistive stress related acceleration are now included and discussed in the SI and in the main manuscript (e.g. section 3.2).*

One of the (seemingly) primary conclusions of the paper – that the 2012 acceleration (which the model cannot reproduce) is somehow attributable to missing model physics that would be triggered by an extreme melting event during that same year – seems deeply flawed. There are a whole host of reasons why the model might not be able to reproduce these observations of speed-up. For some reason the focus and discussion is only on this one thing. Even if the missing model physics and melt forcing were added to the model and demonstrated to increase the model skill at matching these particular observations, that could only be used to argue that these missing physics (and by inference the extreme melt event) were a possible explanation. Here however, the authors don’t do that work (admittedly, it would not be easy) but instead just state de facto that the extreme melt year (and missing model physics) must be the cause for the mismatch. Then, this is later misleadingly used to make even sketchier, grander claims in both the abstract and conclusions of the paper.

*Authors: We agree and have changed the discussion (sect. 3.2) and SI, see sect 1. We have added a discussion on bed geometry, oceanic and climatic forcing influence and their implications for the model results.*

### **General comments / questions**

**The last part of the abstract (4866 lines 14-20) is very misleading. As discussed further below, I don't think you've done enough work or analysis to justify the conclusion about the importance of extreme melt events. And the jump from this unsupported conclusion to the suggestion that future sea-level rise may be larger than predicted by current models (because of extreme melt events) is really over reaching.**

*Authors: We agree. This statement is not included in the new version of the manuscript.*

**As discussed further below, the description of the spin-up procedure and the transition from that to the "forced" model run is confusing to me. It is discussed in more than one place (e.g., 4869, 5-25 and 4870, 21-25) and it would be nice to have a clear description of that in one place only.**

*Authors: We agree. Therefore, we have moved 4869, 5-25 to 4870 (sect. Boundary conditions, calving and ground line parametrization).*

**I'm not sure why the authors are using the 1 km Bamber DEM rather than the relatively improved Morlighem et al. DEM, which is the same as Bamber in many areas, but clearly better in others. It could be that in this region, the actual data resolution is such that the two are very similar, but it seems like this should be discussed / shown at some point by the authors (even if only in the SI). The Morlighem et al. work clearly demonstrates some negative aspects of the Bamber DEM in specific regions.**

*Authors: The bed topography and its implication for ice dynamics is now included in the SI. However, the difference between the two datasets in the JI region is minor (see the Fig. 1 below). While we do agree that there are major differences between the two beds for other regions of the GrIS, this is not the case for Jakobshavn. See also SI, sect. 1.3.2 and Fig. S7 .*

*Unfortunately, the profile shown in the original version of the paper was misplaced. 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).*

*Furthermore, we have performed a simulation using the bed from Morlighem et al. and we did not obtain any significant differences in terms of timing, magnitude and shape of the modelled velocities.*

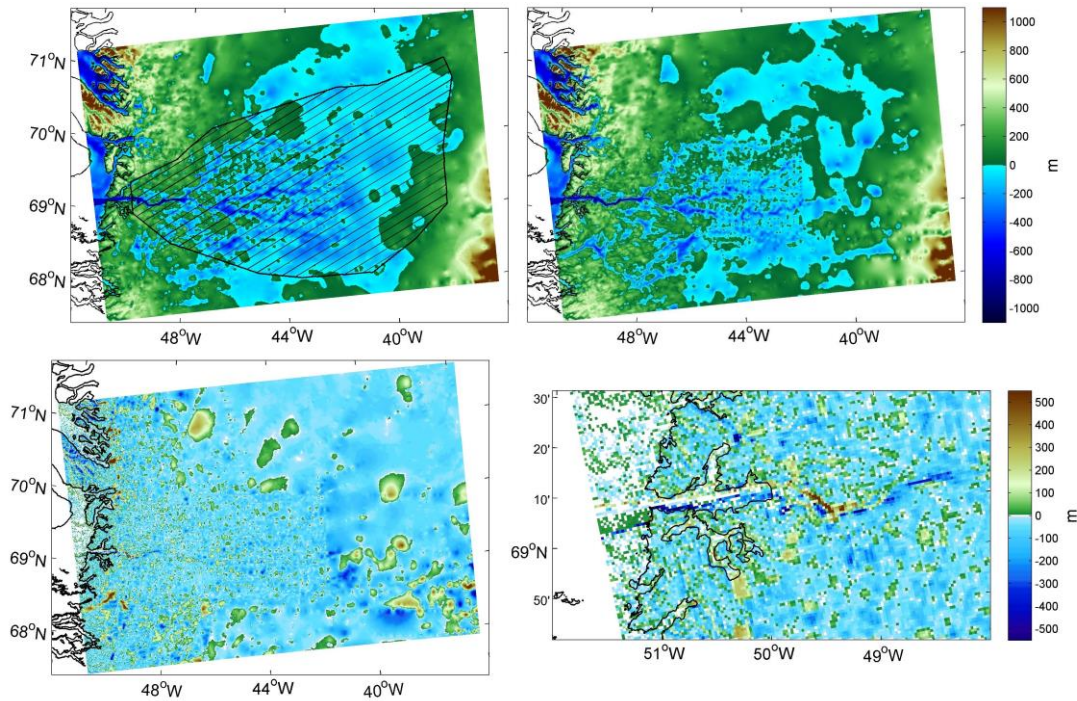


Figure 1. Bed topography from Bamber et al. (2013) (top-left) and from Morlighem et al. (2014) (top-right). The difference (M-B) is shown in the two bottom figures.

These aspects may not affect the current study, but an effort should be made to assure the reader of that as the bedrock topography will profoundly impact the ice dynamics response in this simulation. Similarly, the ice dynamics will be very sensitive to the grid resolution, and in particular, the dynamics in the region of the grounding line, which is more-or-less where all the interesting and important dynamical behaviors are going to originate from in these simulations. There is some mention of the grounding line parameterization used and the “reversible grounding line dynamics” demonstrated by Feldmann et al.

*Authors: The grid resolution, the bed topography and its implication for ice dynamics are now included and made clear in the SI, sect. 1.3 and the main manuscript sect. 3.2. The calving law and grounding line parametrization are now better described in the main manuscript, sect 2.1.2.*

Still, it would be good to know if the authors have conducted the MISMIP3d simulations on their own (with these model settings) and compared them to the benchmarks, in order to convince themselves that they are modeling grounding line behavior accurately. I know that many people remain unconvinced that grounding line dynamics can be modeled accurately at the coarse grid resolutions discussed in the Feldmann et al. paper (and are thus similarly skeptical of papers that use Feldmann et al. as a basis for applying coarse-resolution PISM to problems with grounding line dynamics).

*Authors: No. The authors have not conducted the MISMIP3d simulations themselves. We have included the following statement in the main manuscript, section 2.1.2:*

*“In the Mismip3d experiments, PISM was used to model reversible grounding line dynamics with results consistent with full-Stokes models (Feldmann et al., 2014). However, we have not performed the Mismip3d*

experiments for our particular parameter settings and therefore, the accuracy of the modelled grounding line migration is solely based on the results presented in Feldmann et al. (2014). ”

**An obvious question that arises is, could some combination of inaccurate bed topography and / or low grid resolution be the cause for the lack of the 2012 speed-up (which the authors go on to attribute to the lack of physics associated with extreme melting events)?**

*Authors: Yes. This has been now better discussed in the main manuscript (sect. 3.2) and in the SI.*

**p.4872, 1-6: At what point were the 50 simulations with different sets of parameters conducted? Were there 50 different simulations started from the spin-up stage, all with different parameter settings, or were the different parameter combinations spawned off from the same modern-day initial condition? If a model is run to quasi-equilibrium with a specific set of parameter settings, and then those parameter values are changed (e.g., starting in 1990), the model will very likely experience a significant transient as it adjusts to the new parameter settings; a step function change in parameter settings will look very much like a step function change in the model forcing. The authors should clarify which of the above was done here. If the latter was done, they should also describe if / how they’ve sorted out what fraction of the models transient behavior is due to the climate forcing applied as opposed to being due to a change in model parameters that initiate some (possibly unrealistic) model transient.**

*Authors: We have added:*

*“We perform fifty simulations with different sets of parameters. The parameters are altered only during the regional JI runs.”*

*A sensitivity of the model to different parameter settings has been included in SI, sect. 1. See also Fig. S5, S6 and S12(A vs B).*

**The discussion of the “ocean model component” is not very informative. In particular, it’s not clear to me that there is even a “model” being used here in the sense that most readers would be familiar with. There are repeated references to the model of Martin et al. (2011) in terms of additional details. I think it would not hurt to repeat some of these details here in the SI. Without knowing a bit more about this model, it’s very hard to understand how important, or unimportant, the ocean forcing is in terms of playing a role in the model’s behavior.**

*Authors: Done. See the new sect 2.1.3 and SI, sect. 1.2.5. The “ocean model component” has been replaced with “Parametrization for ice shelf melting” .*

**Everything on p.4873, lines 1-29, and 4874, lines 1-5, seems like speculation to me. Worse yet, this speculation builds on itself in order to support much broader claims in the conclusions (and abstract) of the paper. If the model does not match the observations (the acceleration in 2012) it could be for many reasons, or many combinations of reasons (inadequate model resolution, missing model physics, incorrect or incomplete input / forcing data, etc.).**

*Authors: All of the statements are now fully supported by model or observations. However, we do agree that the lack of the 2012 acceleration may be due to various reasons, which are now included and discussed in the manuscript and in the SI.*

The authors have chosen one of many possible explanations (missing model physics – specifically a wide range of missing physics with possible links to rapid surface melting) as the reason for the mismatch. As a hypothesis to be tested this is fine, but that would require some additional follow up, like implementing new physics in the model, testing them, and showing they improve the model's ability to match the observations. The authors don't do that here. In fact it doesn't seem like they tested anything else to try and understand the mismatch to the 2012 observations. Thus, it seems very premature to conclude (and strongly at that), "the influence of enhanced surface melting on JIs dynamics has been proven." Additional sweeping conclusions should not also then be built upon this already tenuous conclusion (e.g., that simulations of sea-level rise from prognostic ice sheet models may be underestimated because of the particular missing model physics speculated on here).

*Authors: We agree. Therefore, both statements were not included in the new version of the manuscript. Furthermore, additional tests have been performed to try and explain the mismatch (e.g. see sect. 3.2).*

p.4875, 4-12: It sounds here like the first period of acceleration and retreat is some kind of transient as the model adjusts from its initial conditions. How confident are the authors that the longer-term dynamic response throughout the entire simulation is not some further manifestation of this same transient? If the climate forcing is held steady at its initial (1990s) values, is the overall model behavior for the remainder of the simulation very similar or very different? A control run where forcing is held constant would be very useful for convincing us that we're not just looking at a model transient here. If the bulk of the dynamic response is a transient following the spin-up, then that is useful information (but we would then also need to be convinced that it is a realistic transient rather than some artifact from a change in model parameters (as mentioned above)).

*Authors: The climatic forcing influence in JI's retreat is insignificant, and our results show that the dynamic changes observed at JI are triggered at the terminus. Therefore, keeping the climate forcing steady will not have a significant impact on the model results. Furthermore, the input ocean related temperature ( $T_o$ ) is already constant. A sensitivity of the model to different parameter settings has been included in SI, sect. 1. Please see also Fig. S5 which shows the cumulative mass change evolution in time under different forcing scenarios (e.g. constant climate, no oceanic forcing, fixed terminus etc.) and Fig. S12.*

Section 3.2 in general – A goal for this section is to "investigate" processes driving the dynamic evolution of JI. This investigation seems limited to treating the model output as observations as opposed to diving deeper into the level of insight the model could be providing, e.g. like looking at the results for force balance analyses. When you say "we attribute x to y", do you mean that you are guessing that is what is happening based on looking at the model output, or have you actually diagnosed cause and effect by making calculations with the model output? If the latter, it would bolster the readers confidence to see some support of this (e.g., in the SI). You could, for instance, show how "[you] attribute most of the observed 1998 acceleration to a reduction in lateral stress, retreat of the grounding line . . .", by showing us a plot of resistive stresses in time along with a plot of velocities and grounding line positions. As currently written, it sounds like there is a lot of speculation about what the model is doing. But you have the information on hand to actually know for certain what the model is doing.

*Authors: Please see SI (section 1.2.7) Fig. S7 and S8, and the main manuscript, sect. 3.2 Feedback mechanisms, forcings and limitations.*



**p.4878, 1-14:** One of the main “conclusions”, about the 2012 acceleration and its cause, does not seem supported at all by the simulations and / or analysis presented in the paper. Either additional experiments need to be done to support these conclusions, or this section (and other prev. sections) need to be heavily re-written to provide a more inclusive list of possible causes. The model does not match the observations well over a particular time period but there is no logical way that information alone supports the importance of the particular process that is focused on here (to the exclusion of a whole host of other possible missing processes, model simplifications, etc.).

*Authors: p.4878, 1-14 has been rewritten.*

#### **Detailed comments / questions**

**4867, 10:** I think calling out JI’s contribution to global SLR might be a bit much here. Why not just state that it is important to the Greenland ice sheet’s mass balance (and hence indirectly to SLR)?

*Authors: Done.*

**4867, 11-13:** Clarify – you mean that demonstrating a model’s predictive skill is a prerequisite to having confidence in its future projections?

*Authors: Yes. In the new version of the manuscript the statement is no longer included.*

**4869, 1:** “it is known to reasonably capture the fast moving grounded ice” -> “has been shown to reasonably simulate the flow of both grounded and floating ice.”

*Authors: Done.*

**4869, 2-4:** The temperature / enthalpy balance in PISM also accounts for conservation of energy (through the standard heat equation) in cold ice!

*We have replaced:*

*“For conservation of energy, PISM uses an enthalpy scheme that accounts for the latent heat content within temperate ice (i.e., ice at the pressure melting point) (Aschwanden et al., 2012).”*

*with*

*“For conservation of energy, PISM uses an enthalpy scheme (Aschwanden et al., 2012) that accounts for changes in temperature in cold ice (i.e., ice below the pressure melting point) and for changes in water content in temperate ice (i.e., ice at the pressure melting point).”*

**4869, 8-9:** Unclear what you mean by here by “computational domain that does not extend farther than 4000m above the bed”. Is it relevant?

*Authors: No, is not relevant. Therefore, is not included in the new version of the manuscript.*

**4870, 2:** Note that the Bamber DEM has information at 1 km, but that is not the same as 1 km resolution. Over most of the ice sheet it is based on data that is far sparser than 1 km.

*We agree. Therefore, L4870,2 has been changed too:*

*Authors: The 1 km bed elevation dataset for all of Greenland was derived from a combination of multiple airborne ice thickness surveys and satellite-derived elevations undertaken between 1970–2012 (see SI, sect. 1.3.2).*

**4870, 20: Why “interpreted as”? It seems to me these are Dirichlet boundary conditions.**

*Done. “interpreted” has been changed to “applied”.*

**4870, 27: “. . . and apply an ice thickness condition.” What does this mean? Is this a boundary condition?**

*Authors: The so called “ice thickness condition” is now discussed in more detailed in the manuscript.*

*“Along the ice shelf calving front, we apply a physically based calving (eigencalving) parametrization (Winkelmann et al., 2011; Levermann et al., 2012) and an ice thickness condition (Albrecht et al., 2011) that removes at a rate of at most one grid cell per time step any floating ice at the calving front thinner than a given threshold (see SI sect. 1 for its specific value).”*

**4871, 5-8: It would be good to demonstrate that, with the model settings and grid resolutions used here, that this particular model configuration does a reasonable job of accurate grounding line migration (e.g., by comparing to MISMIP), as opposed to just pointing to the previous work of others who’ve used the model.**

*Authors: While we do agree that it will be interesting to recreate the MISMIP experiments, we find this outside of the purpose of the current manuscript. Furthermore, we assume the MISMIP and the role of the Feldmann paper is to represent a benchmark and a starting point for those using the model. Considering the parametrization of the grounding line used here and general in PISM or PISM-PIK we find this highly unnecessary. Compared with the resolutions used in the Feldmann paper and other similar papers that are using PISM (e.g. 10-20 km) and the same grounding line parametrization, the 2 km resolution used here is far from being too coarse.*

*We have added in the main manuscript (sect. 2.1.2, 6L30-31 and 7L1-3):*

*“In the Mismip3d experiments, PISM was used to model reversible grounding line dynamics with results consistent with full-Stokes models (Feldmann et al., 2014). However, we have not performed the Mismip3d experiments for our particular parameter settings and therefore, the accuracy of the modelled grounding line migration is solely based on the results presented in Feldmann et al. (2014).”*

**4871, 10: Ocean model “component” is misleading here. Is there actually an ocean “model” being used here? If so, it should be described with at least a little more detail in the SI. It’s very hard to tell what exactly is going on here in terms of ocean forcing. It sounds like some kind of constant temperature / salinity profile is being applied and the only “forcing” is how deep the ice draft reaches.**

*Authors: Done. See section 2.1.3 and SI, section. 1.2.5.*

**4872, 2-6: Do these fifty simulations include the spin-up phase, or are the parameters only altered in the portion of the simulation that seeks to mimic the past few decades?**

*Authors: The parameters are altered only in the portion of the simulation that seeks to mimic the past few decades. We have added:*

*“We perform fifty simulations with different sets of parameters. The parameters are altered only during the regional JI runs.”*

**4872, 11-14:** Again, very hard to understand what is going on here with forcing from the ocean. How does this information relate to the description of the ocean “model” given above? What is  $F_{\text{melt}}$  and what is the value of 0.198 m/s supposed to tell us? These have no wider context as far as I can tell and thus are just confusing when presented here.

*Authors: The “ocean model component” has been replaced with “parametrization for ice shelf melting” (to avoid any confusion) and is explained in more detailed in the new version of the manuscript. Please refer to sect 2.1.3 and SI, sect. 1.2.5.*

**4874, 17-20:** Some additional information on the elastic “model” being used here should be added to the SI.

*Authors: To predict elastic displacements, we convolve mass loss estimates from airborne radar and laser altimetry with the Green’s function for vertical displacements derived by Jean-Paul Boy (2004) for the Preliminary Reference Earth Model (Dziewonski, 1981).*

*Petrov, L., and J.-P. Boy, Study of the atmospheric pressure loading signal in VLBI observations, J. Geophys. Res., **109**, B03405, doi:10.1029/2003JB002500 (2004).*

*Dziewonski, A., and D. Anderson, Preliminary reference Earth model, Phys. Earth Planet. In., **25**(4), 297-356 (1981).*

**4875, 16:** In general, it’s unclear where the seasonal cycle in velocity in the model is coming from. Is it from the ocean forcing? From the SMB forcing?

*Authors: A new sect. discussing the seasonal cycle in velocity is added to the SI. See sect. 1.2.5 and sect. 1.4.*

**4876, 10-12, 27-29:** You discuss the attribution of certain model behaviors to things like a reduction in lateral drag or a reduction in resistive stress, but there is no indication as to whether this is coming from some quantitative analysis of the model output (e.g., stress balance calculations) or if it is something less rigorous, like your intuition from looking at the model data.

*Authors: It is now clarified. See SI (the new section 1.2.7).*

**Figures Figure 1:** I don’t see any “grey filled contour map”. All I see is the shape of Greenland but it is all “grey” inside (no contours). In the print version, it is also nearly impossible to see the “blue box”.

*Authors: “contour” has been removed. We have made the blue box, light blue – for more contrast.*

**Figure 3:** Especially in print, it is very hard to see the “filled circles”. They just look like black circles on top of black lines, making it hard to compare the modeled and observed profiles at different locations. Would suggest making the colored circles for the observations quite a bit bigger so they can be seen.

*Authors: Done. See the new Fig. 3.*

**Figure 4: Is it “SMB variability” or just “SMB”?**

*Authors: Just SMB.*

### **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 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 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).

*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 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 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  $E_{SIA}$ ,  $q$ ,  $\delta$ ,  $F_{melt}$ ,  $H_{cr}$  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 ( $E_{SIA}$ ,  $q$ ,  $\delta$ ).*

*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  $\pm 30$  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  $\sim 2236 \text{ m a}^{-1}$ ; 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,  $T_f$ ) 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  $T_0 - T_f$  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 as 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.

*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 ( $\dot{\epsilon}_{\pm}$ ), 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 analysed*

*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: • 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. . .) •**

*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. ~**

*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. . .)). ~ 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 ~ 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 misplaced. 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 researched 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 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)?

*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 GRIIS 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 Cressida 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 Cressida 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 misplaced.*

**p. 4870 line 13-14: so how is RACMO downscaled for the 2km surface grid of Jakob, 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  $\sim 11$  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  $J_I$ ’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  $J_I$ ’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 “ ( $T_o - T_f$ ) 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  $F_{melt}$ ,  $H_{cr}$  and  $T_0$  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 ( $ESIA$ ,  $q$ ,  $\delta$ ).”*

*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 integrated/absorbed. 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 retreat 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 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.**

*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 do 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 Jakob 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 Jakob trough, see comments above.*

## Relevant changes

Significant changes have been made throughout the manuscript to accommodate the three referees comments:

1. The bed profile shown in the original version of the paper was misplaced. 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). See Fig. 6 in the new version of the manuscript.
2. The discussion has been completely rewritten and now includes all the suggestions made by the reviewers. See the new sect. 3.2 Feedback mechanisms, forcings and limitations.
3. The forcings and the sensitivity of the model to different parameters have been considered and are now included in SI, sect 1;
4. The climatic and oceanic forcings are better discussed in the SI and in the main manuscript;
5. Figures that show deviatoric stresses have been included in SI;
6. All the figures have been re-made.

# **Modelled gGlacier dynamics over the last quarter of a century at Jakobshavn Isbræ**

**Ioana S. Muresan<sup>1</sup>, Shfaqat A. Khan<sup>1</sup>, Andy Aschwanden<sup>2</sup>, Constantine Khroulev<sup>2</sup>, Tonie Van Dam<sup>3</sup>, Jonathan Bamber<sup>4</sup>, Michiel R. van den Broeke<sup>5</sup>, Bert Wouters<sup>4</sup>, Peter Kuipers Munneke<sup>5</sup>, and Kurt H. Kjær<sup>6</sup>**

[1]{Department of Geodesy, DTU Space, Technical University of Denmark, Kgs. Lyngby, Denmark}

[2]{Geophysical Institute, University of Alaska Fairbanks, Fairbanks, Alaska, USA}

[3]{University of Luxembourg, Faculty of Science, Technology and Communication (FSTC), Engineering Research Unit, Luxembourg}

[4]{University of Bristol, School of Geographical Sciences, Bristol, England}

[5]{Institute for Marine and Atmospheric research Utrecht (IMAU), Utrecht University, The Netherlands}

[6]{Centre for GeoGenetics, Natural History Museum of Denmark, University of Copenhagen, Copenhagen, Denmark}

Correspondence to: I. S. Muresan (iomur@space.dtu.dk)

## **Abstract**

Observations over the past two decades show substantial ice loss associated with the speedup of marine terminating glaciers in Greenland. Here we use a regional 3-D outlet glacier model to simulate the behaviour of Jakobshavn Isbræ (JI) located in west Greenland. Our approach represents an attempt to model and understand the recent behaviour of JI with a physical process-based model. Using atmospheric and oceanic forcing we tune our model to reproduce the observed frontal changes of JI during 1990–2014. – We find that most of the JI retreat during 1990-2014 is driven by ocean and bed geometry. Our results suggest that the overall

variability in modelled horizontal velocities is a response to variations in terminus position. We identify two major accelerations which are consistent with observations of changes in the glacier terminus. The first ~~event occurs-occured~~ in 1998, and ~~is-was~~ triggered ~~-by a retreat of the front and~~ moderate thinning ~~of JI~~ prior to 1998. The second ~~accelerationevent~~, which ~~starts-started~~ in 2003 and ~~peaks-peaked~~ in ~~the~~ summer 2004, ~~is-was~~ triggered by the final breakup of the floating tongue~~-. This breakup which generates a reductionreduced in-the buttressing at the JI terminus. This results in further that resulted in further thinning-. A~~and as the slope ~~steepens-steepened~~ inland ~~over the last decade~~, sustained high velocities have been observed at JI~~-over the last decade~~. Our model provides evidence that the 1998 and 2003 flow accelerations are most likely initiated by the bed geometry. As opposed to other regions on the Greenland Ice Sheet (GrIS), where dynamically induced mass loss has slowed down over recent years, both modelled and observed results for JI suggest a continuation of the acceleration in mass loss. However, our model is not able to capture the observed 2010-2012 terminus retreat. We attribute this slight failing to either inaccuracies in basal topography, or to misrepresentations of the climatic and oceanic forcings that were used. Both modelled and observed results suggest that JI has been losing mass at an accelerated rate, and it continued to accelerate throughout 2014.

~~Further, we find that our model is not able to capture the 2012 peak in the observed velocities. Our analysis suggests that the 2012 acceleration of JI is likely the result of an exceptionally long melt season dominated by extreme melt events. Considering that such extreme surface melt events are expected to intensify in the future, our findings suggest that the 21st century projections of the GrIS mass loss and the future sea level rise may be larger than predicted by existing modelling results.~~

## 1 Introduction

The rate of ice mass loss from Greenlandic marine terminating glaciers has more than doubled over the past two decades (Rignot et al., 2008; Moon et al., 2012; Shepherd et al., 2012). Jakobshavn Isbræ, located mid-way up on the west side of Greenland, is one of the largest outlet glaciers in terms of drainage area as it drains ~ 6 % of the GrIS (Krabill et al., 2000). Due to its consistently high flow rate and seasonally varying rate of flow and front position, the glacier has received much attention in the last two decades (Thomas et al., 2003; Luckman and Murray, 2005; Holland et al., 2008; Amundson et al., 2010; Khan et al., 2010; Motyka et al., 2011; Joughin et al., 2012; Gladish et al., 2015b,a). Measurements from synthetic aperture

radar suggest that the speed of JI doubled between 1992 and 2003 (Joughin et al., 2004). More recent measurements show a steady increase in the flow rate over the glacier's faster-moving region of  $\sim 5\%$  per year (Joughin et al., 2008). The speedup coincides with thinning of up to  $15 \text{ m a}^{-1}$  between ~~2006-2003 and~~ 2012 near the glacier front ([Krabill et al., 2004](#); [Joughin et al., 2008](#); Nielsen et al., 2013) as observed from airborne laser altimeter surveys. The steady increase in the flow rate and glacier thinning suggest a continuous dynamic drawdown of mass, and highlights JIs importance for the GrIS mass balance~~for predicting present and future sea level rise~~.

~~In general, reproducing past and present day observations of the dynamical behaviour of Greenland's outlet glaciers is the key for developing realistic projections of future changes in the GrIS (IPCC, 2013). Over the past decade, we have seen The past decade has shown~~ significant improvements in the numerical modelling of glaciers and ice sheets (e.g. Price et al., 2011; Vieli and Nick, 2011; Winkelmann et al., 2011; Larour et al., 2012; Pattyn et al., 2012; Seroussi et al., 2012; Aschwanden et al., 2013; Nick et al., 2013; Mengel and Levermann, 2014). Some regional scale glacier models are based on a flow-line approach (Nick et al., 2009; Parizek and Walker, 2010), which models the one- or two-dimensional dynamic behaviour of the glacier ~~under consideration~~considered. Flow-line models are computationally efficient and valuable for understanding basic processes. However, three-dimensional models are more appropriate in areas of flow divergence/convergence and/or where lateral stresses are important.

In the last decade, several processes have been identified ~~to as~~ controlling the observed speedup ~~at of~~ JI (Nick et al., 2009; Van der Veen et al., 2011; Joughin et al., 2012). One is a reduction in resistance (buttressing) at the marine front through thinning or retreat of the floating tongue of the glacier, but the details of the processes triggering and controlling thinning and retreat remain elusive. Accurately modelling complex interactions between thinning, retreat, and acceleration as observed at JI, is challenging. Our knowledge of the mechanisms triggering these events is usually constrained to the period covered by observations. The initial speedup of JI occurred during the time when the satellite and airborne observations were infrequent and therefore insufficient to monitor the seasonal to monthly evolution of glacier geometry and speed.

~~In this paper~~Here, we use a high-resolution, three dimensional and time-dependent regional outlet glacier model, developed as part of the Parallel Ice Sheet Model (PISM; please refer to

sec. 2.1 The ice sheet model) (The PISM Authors, 2014), ~~is used to investigate the processes driving investigate the dynamic evolution and the seasonal velocity variation~~ of JI between 1990–2014. ~~While previous 3-D modelling studies concentrate mostly on modelling individual processes using stress perturbations (e.g. Van der Veen et al., 2011, Joughin et al. 2012), the present paper aims to model and understand the recent behaviour of JI with a process-based model.~~

~~The period prior to 2004 is characterized by sparsely sampled data with observations available only during 1992, 1995 and between 2000 and 2003. The second part of 2003 and the first part of 2004 are missing entirely from the available observational record (Joughin et al., 2008; Joughin et al., 2012). Therefore, we concentrate our efforts on resolving the degree of seasonal velocity variation before, during, and after the final breakup of the ice tongue in the summer of 2003 (Luckman and Murray, 2005; Joughin et al., 2008).~~ Our modelling approach is based on a regional equilibrium run and a time-integration ~~from over the period~~ 1990 to 2014, where the grounding lines and the calving fronts are free to evolve under monthly climatic forcing and oceanic boundary conditions.

## 2 Methods and forcing

### 2.1 The ice sheet model

The ice sheet model used in this study is PISM (stable version 0.6). PISM is an open source, parallel, three-dimensional, thermodynamically coupled and time dependent ice sheet model (Bueler and Brown, 2009; Winkelmann et al., 2011; The PISM Authors, 2014). The ice dynamic model is the “SIA+SSA hybrid”, with the non-sliding shallow ice approximation (SIA) for simulating slowly moving grounded ice in the interior part of the ice sheet and the shallow shelf approximation (SSA) for simulating fast-flowing outlet glacier and ice shelf systems (Bueler and Brown, 2009). The superposition of SIA and SSA sustains a smooth transition between non-sliding, bedrock frozen ice and sliding, fast-flowing ice, and ~~it is knownhas been shown~~ to reasonably ~~capture-simulate~~ the ~~fast-moving-grounded-ice- flow of both grounded and floating ice~~ (Winkelmann et al., 2011). ~~For conservation of energy, PISM uses an enthalpy scheme that accounts for the latent heat content within temperate ice (i.e., ice at the pressure melting point) (Aschwanden et al., 2012).~~ For conservation of energy, PISM uses an enthalpy scheme (Aschwanden et al., 2012) that accounts for changes in temperature

1 in cold ice (i.e., ice below the pressure melting point) and for changes in water content in  
2 temperate ice (i.e., ice at the pressure melting point).

3 ~~We start our regional JI runs with an equilibrium simulation on a 125×86 horizontal grid with~~  
4 ~~5 km spacing and a vertical resolution of 20 m. The enthalpy formulation models the mass~~  
5 ~~and energy balance for the three dimensional ice fluid field based on 200 regularly spaced~~  
6 ~~layers within the ice in a computational domain that does not extend farther than 4000 m~~  
7 ~~above the bed. The temperature in the bedrock thermal layer is computed up to a depth of~~  
8 ~~1000 m with 50 regularly spaced layers. The first step is to obtain a 5 km regional equilibrium~~  
9 ~~model for JI using constant mean climate (i.e. repeating the 1960–1990 mean air temperature~~  
10 ~~and surface mass balance; see 2.1.1 Input data). We consider that equilibrium has been~~  
11 ~~established when the ice volume in the regional domain changes by less than 1% in the final~~  
12 ~~100 model years. Grid refinements are made from 5 km (125×86) to 2 km (310×213) after~~  
13 ~~3000 years. The length of the simulation with the 2 km grid is 200 years. The model reaches~~  
14 ~~equilibrium with an ice volume of  $0.25 [10^6 \text{ km}^3]$  (or a 3.6 % increase relative to the input~~  
15 ~~dataset from Bamber et al. (2013) adjusted to simulate 1990's metrics) and horizontal~~  
16 ~~velocities that do not exceed  $5500 \text{ m a}^{-1}$  near the terminus. By the end of the equilibrium~~  
17 ~~simulation, thin ice ( $< 400 \text{ m}$ ) fills the ice fjord up to 2 to 4 km away from the 1990 observed~~  
18 ~~frontal position. This excess thin ice is calved within the first two years of the forward~~  
19 ~~simulation. Using our equilibrium simulations with 2 km and with a 10 m grid in vertical, we~~  
20 ~~integrate forward in time (hindcast) from 1990 to 2014 by imposing monthly fields of SMB~~  
21 ~~and 2 m air temperatures through a one-way coupling scheme. The calving fronts and~~  
22 ~~grounding lines are free to evolve in time both during the equilibrium and the forward~~  
23 ~~simulation (see 2.1.2 Boundary conditions, calving and ground line parametrization).~~

## 24 **2.1.1 Input data**

25 ~~We use the bed topography from Bamber et al. (2013). The bed topography used in this study~~  
26 ~~is from Bamber et al. (2013). The 1 km bed elevation dataset for all of Greenland ~~has a 1 km~~~~  
27 ~~spatial resolution and~~ was derived from a combination of multiple airborne ice thickness  
28 surveys and satellite-derived elevations undertaken between 1970–2012 (~~Bamber et al.,~~  
29 2013see Supplementary information (SI), sect. 1.3.2). The terminus position and surface  
30 elevation in the Jakobshavn region is further adjusted to simulate 1990's metrics based on  
31 1985 aerial photographs and existing satellite altimetry observations (Csatho et al., 2008). Ice  
32 thickness in the JI basin is computed as the difference between surface and bedrock elevation,



which implies that at the beginning of our equilibrium simulation JI's terminus is considered grounded. The model of the geothermal flux is from Shapiro and Ritzwoller (2004). The input fields of near-surface air temperature and surface mass balance are from the regional climate model RACMO2.3 (Noël et al., 2015). The version used in this study is produced at a spatial resolution of  $\sim 11$  km and ~~is extended to the end of 2014, to covers~~ the period from 1958 to 2014. The original dataset of 11 km grid is interpolated to 2x2 km grids using bilinear interpolation.

### **2.1.2 Boundary conditions, calving and ground line parametrization**

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 start the spin-up on a 10 km grid, and then we refine to 5 km at -5ka. It is important to note that during the paleo-climatic initialization the terminus is held fixed to the 1990 observed position in the JI region, and to the present-day position elsewhere.

In the regional outlet glacier model of PISM, the boundary conditions are handled in a 10 km strip positioned outside of the JI's drainage basin and around the edge of the computational domain. In this strip, the input values of the basal melt, the amount of till-pore water, ice enthalpy, and lithospheric temperature (Aschwanden et al., 2013) are held fixed and ~~interpreted as~~ applied as Dirichlet boundary conditions ~~by in~~ the conservation of energy model (The PISM Authors, 2014). ~~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 start our regional JI runs with an equilibrium simulation on a 125×86 horizontal grid with 5 km spacing and a vertical resolution of 20 m. The enthalpy formulation models the mass and energy balance for the three dimensional ice fluid field based on 200 regularly spaced layers within the ice. The temperature in the bedrock thermal layer is computed up to a depth of 1000 m with 50 regularly spaced layers. The first step is to obtain a 5 km regional equilibrium model for JI using constant mean climate (i.e. repeating the 1960-1990 mean air temperature and surface mass balance; see 2.1.1 Input data). We consider that equilibrium has been established when the ice volume in the regional domain changes by less

than 1% in the final 100 model years. Grid refinements are made from 5 km (125×86) to 2 km (310×213) after 3000 years. The length of the simulation with the 2 km grid is 200 years. The model reaches equilibrium with an ice volume of 0.25·10<sup>6</sup> km<sup>3</sup> (or a 3.6 % increase relative to the input dataset from Bamber et al. (2013) adjusted to simulate, 1990's metrics; see Sect. 2.1.1) and horizontal velocities that do not exceed 5500 m a<sup>-1</sup> near the terminus. By the end of the equilibrium simulation, thin ice (< 400 m) fills the ice fjord up to 2 to 4 km away from the 1990 observed frontal position. This excess thin ice is calved within the first two years of the forward simulation. Using our equilibrium simulations with 2 km and with a 10 m grid in vertical, we integrate forward in time (hindcast) from 1990 to 2014 by imposing monthly fields of SMB and 2 m air temperatures through a one-way coupling scheme. The calving fronts and grounding lines are free to evolve in time both during the equilibrium and the forward simulation.

Along the ice shelf calving front, we apply a physically based calving (eigencalving) parametrization (Winkelmann et al., 2011; Levermann et al., 2012) and an ice thickness condition (Albrecht et al., 2011) that removes at a rate of at most one grid cell per time step any floating ice at the calving front thinner than a given threshold (see SI sect. 1 for its specific value). ~~The calving law is known to yield realistic calving front positions for various types of ice shelves being successfully used for modelling calving front positions in whole Antarctica simulations (Martin et al., 2011) and regional east Antarctica simulations (Mengel and Levermann, 2014).~~ The average calving rate ( $c$ ) is calculated as the product of the principal components of the horizontal strain rates ( $\dot{\epsilon}_{\pm}$ ), derived from SSA velocities, and a proportionality constant parameter ( $k$ ) that captures the material properties relevant for calving :

$$c = k \dot{\epsilon}_{+} \dot{\epsilon}_{-} \quad \text{for } \dot{\epsilon}_{\pm} > 0. \quad (1)$$

The strain rate pattern is strongly influenced by the geometry and the boundary conditions at the ice shelf front. A partially-filled grid cell formulation (Albrecht et al., 2011) which allows for sub-grid scale retreat and advance of the front is used to connect the calving rate computed by the eigen calving parametrization with the mass transport scheme at the ice shelf terminus. ~~The calving law is known to yield realistic calving front positions for various types of ice shelves being successfully used for modelling calving front positions in whole Antarctica simulations (Martin et al., 2011) and regional east Antarctica simulations (Mengel and Levermann, 2014).~~

~~The calving fronts and grounding lines~~—The parameterization of the grounding line position is based on the “LI” parameterization (Gladstone et al., 2010) and is not subject to any boundary conditions. At each time step the grounding line position is determined by a mask which distinguishes between grounded and floating ice using a flotation criterion based on the modelled ice thickness (Winkelmann et al., 2011):

$$b(x, y) = -\frac{\rho_i}{\rho_o} H(x, y) \quad (2)$$

where (x,y) give the horizontal dimension,  $\rho_i$  is the density of the ice,  $\rho_o$  is the density of the ocean water and  $H$  represents the ice thickness.

~~—Therefore, the grounding line migration is influenced by the ice thickness evolution, which further depends on the velocities computed from the stress balance. The superposition of SIA and SSA, which implies that the SSA velocities are computed simultaneously for the shelf and for the sheet, ensures that the stress transmission across the grounding line is continuous and that buttressing effects are included.~~ In the Mismip3d experiments, PISM was used to model reversible grounding line dynamics with results consistent with full-Stokes models (Feldmann et al., 2014). However, we have not performed the Mismip3d experiments for our particular parameter settings and therefore, the accuracy of the modelled grounding line migration is solely based on the results presented in Feldmann et al. (2014).

### 2.1.3 Ocean-model-component Parameterization for ice shelf

#### 2.1.2 melting

We use ~~an ocean-model-component~~ a simple parametrization for ice shelf melting where the melting effect of the ocean is based on both sub-shelf ocean temperature and salinity (Martin et al., 2011). At the base of the ice shelf, the sub-shelf ice temperature ( $T_{pm}$ ) is set to the pressure-melting temperature and is applied as a Dirichlet boundary condition in the conservation of energy equation. The sub-shelf ice temperature holds the following form:

$$T_{pm} = 273.15 + \beta_{cc} z_b \quad (3)$$

where  $\beta_{cc} = 8.66 \times 10^{-4} \text{ K m}^{-1}$  represents the Clausius-Clapeyron gradient and  $z_b$  represents the elevation of the base of the ice shelf.

~~The sub-shelf ice temperature is set to the pressure melting temperature being applied as a Dirichlet boundary condition in the conservation of energy equation, while t~~ The mass flux from shelf to ocean (S) ~~applied~~ follows Beckmann and Goosse (2003). ~~This mass flux and~~ is computed as a heat flux ( $Q_{heat}$ ) between the ocean and ice that represents the melting effect of the ocean through both temperature and salinity (Martin et al., 2011):

$$S = \frac{Q_{heat}}{L_i \rho_i} \quad (4)$$

$$Q_{heat} = \rho_o c_{p_o} \gamma_T F_{melt} (T_o - T_f) \quad (5)$$

where  $L_i = 3.35 \times 10^5 \text{ J kg}^{-1}$  is the latent heat capacity of ice,  $c_{p_o} = 3974 \text{ J (kg K)}^{-1}$  is the specific heat capacity of the ocean mixed layer,  $\gamma_T = 10^{-4} \text{ m s}^{-1}$  is the thermal exchange velocity,  $F_{melt}$  is a model parameter,  $T_o$  is the ocean water temperature and  $T_f$  is the virtual temperature. The virtual temperature represents the freezing temperature of ocean water at the depth  $z_b$  below the ice shelf and has the form:

$$T_f = 273.15 + 0.0939 - 0.057S_o + 7.64 \times 10^{-4} z_b \quad (6)$$

where  $S_o$  is the salinity of the ocean.

We start our simulations with a constant ocean water temperature ( $T_o$ ) of  $-1.7^\circ \text{C}$  which is further scaled ~~in the ocean-ice boundary layer~~ by the ice shelf melting parametrization spatially and temporally based ~~on the ocean water salinity and~~ on the depth below the ice shelf and the ocean water salinity (see also SI, Fig. S4)(see eq. 4 and 5 from Martin et al. (2011) and Supplementary information (SI), Table 1 for their respective values). Therefore, the sub-shelf melt rates are dependent on the ice shelf thickness and indirectly to the bed topography depth. We choose to keep the ocean water salinity ( $S_o = 35 \text{ psu}$ ) constant in time and space (see also SI, Table 1) as the model does not capture the salinity gradient from the base of the ice shelf through layers of low and high salinity. However, a previous study conducted by Mengel and Levermann (2014) using the same model established that the sensitivity of the melt rate to salinity is negligible.

### 3 Results and discussion

~~Fifty simulations are performed with different sets of parameters. We perform fifty simulations with different sets of parameters. The parameters are altered only during the regional JI runs.~~ From these results, we present here the parameterization that best captures the full evolution of JI during the period 1990–2014 (see SI, sect. 1.1 for more details and for

the values of the ice sheet model parameters ~~and sec. 5 for the evolution of the main driver variables for the atmosphere and the ocean~~). The parameters are calibrated such that the model reproduces the frontal positions (Fig. 2) and the ice mass change observations (Fig. 4, please refer to sec. 3.1.1 Mass change) at JI during the period 1990-2014 and 1997-2014, respectively.

The sensitivity to parameters controlling ice dynamics, basal processes and ice shelf melt are discussed in SI, sect. 1.2. The evolution of the main driver variables for the atmosphere and the ocean are further detailed in SI, sect. 1.2.5. Seasonal scale variations in terminus positions and horizontal velocities are discussed in SI, sect. 1.4.

### **3.1—Observations vs. modelling results**

~~We calibrated the parameters such that the model reproduces the frontal positions (Fig. 2) and the ice mass change observations (Fig. 4, please refer to sec. 3.1.1 Mass change) at JI during the period 1990-2014 and 1997-2014, respectively. The procedure for deriving the observed ice mass change is described in SI, sec. 2. In order to match the observed front positions a sub-shelf melting parameter ( $F_{\text{melt}}$ ) with a value of  $0.198 \text{ m s}^{-1}$  (see eq. 5 from Martin et al. (2011)) is used in our simulation and results in basal melt rates slightly larger than those obtained by Motyka et al. (2011).~~

When the modelled velocities in the points S1 to S7 (Fig. 1) are compared with available observations from Smith et al. (2010) the model is able to reproduce the overall pattern of the observed velocities. However, the modelled velocity at point S4 is underestimated and at point S7 is overestimated. Further for all the points and all the simulations the model does not capture the anomalous 2012 observed velocities (see Fig. 3). A previous analysis by Joughin et al. (2014) attributes the acceleration and the summer peak of 2012 to the retreat of the JI terminus to the bottom of an over deepened basin (see Fig. 3 from Joughin et al. 2014). This retreat, which started in 2009 (Joughin et al., 2014), should have triggered an acceleration of JI as soon as the terminus started to retreat in 2009 over the slope of the over deepened basin. However, there is no evidence of such acceleration either in the observational record (see Fig. 1 from Joughin et al., (2014)) , nor in our simulations (see Fig. 6, 7 and sec. 3.2, 2003-2014 for more details). Furthermore, the summer of 2012 is characterized by exceptional surface melt, covering 98% of the entire ice sheet surface, including the high altitude Summit region (Nghiem et al., 2012; Hanna et al., 2014). The three extreme melt events that occurred

1 between 10 July and 10 August 2012 (see Fig. 1a from Hanna et al. (2014) and Fig. 3 from  
2 NSIDC (2015)) might have enhanced hydrofracturing of the calving front due to melt  
3 draining into surface crevasses (MacAyeal et al., 2003; Joughin et al., 2013; Pollard et al.,  
4 2015) resulting in greater and/or faster seasonal retreat and increase the submarine melt at the  
5 terminus and the sub-shelf cavity (Schoof, 2007; Stanley et al., 2011; Kimura et al., 2014;  
6 Slater et al., 2015), which likely triggered the rapid acceleration in the observed ice surface  
7 speed in the summer of 2012. Overall, the 2012 melt season was two months longer than the  
8 1979–2011 mean and the longest recorded in the satellite era (Tedesco et al., 2013). An  
9 intense and long melt year leads to strong thinning of the ice, steepening surface slopes, and  
10 has the potential to further sustain the initial acceleration of JI through enhanced longitudinal  
11 stretching. Furthermore, a study by Doyle et al. (2015) found that during an amplified melt  
12 event (caused by late-summer cyclonic rainfall that occurred between 23 August and 3  
13 September 2011) JIs speed increased by 10% (see Fig. S3 in Doyle et al. (2015)). Therefore,  
14 the influence of enhanced surface melting in JIs dynamics has been proven. Under normal  
15 circumstances (i.e. average melt years), the winter slowdown is usually able to compensate  
16 for the summer acceleration events. The summer of 2012 was however preceded by a series of  
17 warm summers (2007, 2008, 2010 and 2011) (Hanna et al., 2014), which may have created  
18 the conditions under which the winter slowdowns can no longer compensate for the summer  
19 accelerations leading to an increase in the mean annual flow. Bougamont et al. (2014) showed  
20 that a sustained increase in surface runoff by 50% may cause some small runoff events to  
21 increase ice flow magnitudes to those observed in individual supraglacial lake drainage  
22 events. To account for the influence of meltwater runoff and its role in the subglacial system  
23 during such extreme melt events in modelling simulations only by means of inputs from  
24 climate models, is very challenging. There have been very few observational studies of  
25 meltwater runoff through supraglacial water storage, drainage, and discharge patterns and  
26 therefore these events cannot be represented nor captured accurately by current ice sheet  
27 models without any additional coupling. Failing to accurately represent these processes may  
28 lead to an underestimation of the flow speed during intense melt events as observed in our  
29 simulations (Fig. 3, year 2012).

### 30 **3.1.1 Mass change**

31 Figure 4 shows observed and modelled mass change for the period 1997 to 2014. We estimate  
32 the rate of ice volume change from airborne and satellite altimetry over the same period and

convert to mass change rate (see SI, sec. 2). Overall there is good agreement between modelled and observed mass change over this period (see Fig. 4), and our results are in agreement with other similar studies (Howat et al., 2011; Nick et al., 2013). Dynamically driven discharge is known to control Jakobshavn's mass loss (Nick et al., 2013). The modelled cumulative mass loss is 269 Gt, of which 93% (~251 Gt) is determined to be dynamic in origin while the remaining 7% (~18 Gt) is attributed to a decrease in SMB (see Fig. 4). Further, the present-day unloading of ice causes the Earth to respond elastically. Thus, we can use modelled mass changes to predict elastic uplift. We compare modelled changes of the Earth's elastic response to changes in ice mass to uplift observed at four GPS sites (see Fig. 5 and SI, sec. 3). Both model and observations consistently suggest large uplift near the JI front and somewhat minor uplift rates of few mm/yr at distances of >100km from the ice margin.

### 3.2 Seasonal variations in velocities



Next, we investigate the processes driving the dynamic evolution of JI and its seasonal variation in velocity between 1990–2014 with a focus on the initial speedup of JI and the 2003 breakup of the ice tongue. The pattern observed in our simulations suggests a gradual increase in velocities that agrees well with observations from (Joughin et al., 2008; Smith et al., 2010; Joughin et al., 2014) (Fig. 3 and 7). Three distinct stages of acceleration are identified in Fig. 7 (see also Movie 1) and discussed in detail below.

#### 1990–1997

The first period of acceleration is caused by a retreat of the front position by approximately 2 to 4 km between 1990 and 1991. There is no observational evidence that this retreat actually occurred. It is probably a modelling artefact as the geometry obtained during the regional equilibrium simulation is forced with new oceanic and atmospheric conditions. During the first two years, 1990–1991, the model is still calving the excess thin ice (<400 m) generated during the equilibrium simulation. This acceleration results from a reduction in resistive stress near the grounding line, generated by the gradual retreat of the front, triggering a dynamic response in the upstream region of JI.

Starting in 1992 we obtain a good fit of our model to the observed frontal positions. Disregarding the acceleration observed in 1991–1992, no significant seasonal fluctuation in flow rate is observed during this period. Beginning in 1993, a stronger seasonal velocity signal begins to emerge in our simulation that continues and intensifies in magnitude during

1994 and 1995. In 1996 and 1997, the frontal extent and the grounding line position remain relatively stable (Fig. 2 and Fig. 6), and no significant seasonal fluctuation in flow rate is observed. These model results agree well with observations, which indicate that the glacier speed was relatively constant during this period (Joughin et al., 2004; Luckman and Murray, 2005).

#### 1998–2002

According to observations (Joughin et al., 2004; Motyka et al., 2011; Bevan et al., 2012), the initial acceleration of JI occurred in May–August 1998, which coincides with our modelling results. In our simulation, the 1998 acceleration is generated by an increase in surface slopes due to thinning both near the terminus and inland (up to 10 km away from the 1990 front position). These findings are corroborated both by observations (SI, Fig. 1) and modelling results (Fig. 6). The thinning starts in our model in the summer of 1995 and continues to accelerate after 1998 (Fig. 6 and Fig. 7). In addition, a small retreat can be observed on the north side of the fjord, which may be responsible for a reduction in lateral shear (see Movie 1). We observe a retreat of the grounding line position starting in 1998 that accelerates thereafter (Fig. 2 and Fig. 6). Although thinning appears to have increased in our model during three continuous years we find little additional speedup during the period prior to 1998 (Fig. 2 and Fig. 6). According to our simulation, JI's speed increased in the summer of 1998 by  $\sim 80\%$  relative to the summer of 1992 (Fig. 7). We attribute most of the observed 1998 acceleration to a reduction in lateral stress, retreat of the grounding line, and a thinning of the southern tributary that characterized JI from 1995 to 1998. The period between 1999 and 2002 is in our simulation characterized by a temporally uniform flow, with summer retreats and winter advances in terminus position that drive the seasonal velocity fluctuations at JI during this period. Overall we observe an advance of the terminus between 1999 and 2000 and a retreat of the southern tributary between 2000 and 2002 by  $\sim 2$  km, which correlates with existing observations (Thomas, 2004). Concurrent with the 1998–2001 terminus retreat, the grounding line retreats in our model by  $\sim 4$  km. The grounding line retreat sustained by the bed geometry (Fig. 2 and Fig. 6) is further combined with calving and thinning near the front and results in a reduction of resistive stresses at the terminus.

#### 2003–2014

In the late summer of 2003, a steep increase in flow velocity can be observed in Fig. 7, which is driven in our simulations by the final breakup of the ice tongue (see Fig. 2 and Fig. 6). The



period 2002–2003 is characterized by substantial retreat (~4 km), which starts in June 2002 and continues throughout 2003. By December 2003 the terminus has retreated back to the position of the grounding line (see Fig. 2 and Fig. 6). In our simulation, the retreat that occurred in 2003 and the loss of much of the floating tongue causes a major decrease in resistive stresses near the terminus. By 2004 the glacier has thinned significantly both near the front and further inland in response to a change in the near-terminus stress field (Fig. 6 and Fig. 7). During the ice tongue final breakup, JI reached unprecedented flow rates, which in our simulation are as high as 20 km/yr (~120% increase relative to 1998). The velocities decrease to 16 km/yr (~80% increase relative to 1998) in the subsequent months, and JI remains relatively stable with high seasonal fluctuations. The high velocities observed at JI after the loss of its floating tongue are further sustained in our simulation by the thinning that occurred in 2004 onward (see Fig. 7) and which continues to steepen the slopes near the terminus (see Fig. 6). This thinning is combined in the following years with a reduction in surface mass balance due to increased melting and runoff (van den Broeke et al., 2009; Enderlin et al., 2014; Khan et al., 2014). The period 2004–2014 is characterized in our simulation by relatively uniform velocities with strong seasonal variations caused by summer retreat and winter advance of the floating tongue (Fig. 7). During this period, the terminus remains close to the grounding line position with no episodes of significant retreat. Our results indicate that after 2005 the seasonal variation in the horizontal velocities close to the terminus is a response of seasonally induced variations in the terminus position (Fig. 8, S1); while further inland (Fig. 8, S2–S7) the seasonal variation in velocities is mostly driven by a seasonal thinning induced variation.

In contrast to other drainage basins of the GrIS where the mass loss and flow speed slowed down in recent years (Khan et al. 2010; Bevan et al. 2012; Enderlin et al., 2014), in the Jakobshavn basin both modelled and observed data suggest that the JI continued to lose mass at an accelerated rate between 2013–2014.

### **3.1 Observations vs. modelling results**

#### **3.1.1. Annual scale variations in velocities, terminus and grounding line positions**



1 We investigate the processes driving the dynamic evolution of JI and its variation in velocity  
2 between 1990–2014 with a focus on the initial speedup of JI and the 2003 breakup of the ice  
3 tongue. The overall pattern observed in our simulations suggests a gradual increase in  
4 velocities that agrees well with observations (Joughin et al., 2014) (Fig. 3). Three distinct  
5 stages of acceleration are identified in Fig. 3 (see also Movie 1) and discussed in detail below.

6 **• 1990–1997**

7 The first speedup is caused by a retreat of the front position by approximately 2 to 4  
8 km between 1990 and 1991. There is no observational evidence that this retreat  
9 actually occurred. It is probably a modelling artefact as the geometry obtained  
10 during the regional equilibrium simulation is forced with new oceanic and  
11 atmospheric conditions. This acceleration (Fig. 3) is caused by a reduction in  
12 buttressing due to a reduction in lateral resistance (Van der Veen et al., 2011),  
13 which is generated by the gradual retreat of the front, and which triggers a dynamic  
14 response in the upstream region of JI.

15 Starting in 1992 we obtain a good fit between modelled and observed frontal  
16 positions. Disregarding the acceleration in 1991–1992, no significant seasonal  
17 fluctuation in flow rate is modelled during this period. These results are consistent  
18 with observations (Echelmeyer et al., 1994). From 1993 a stronger seasonal  
19 velocity signal begins to emerge in our simulation that continues and intensifies in  
20 magnitude during 1994 and 1995. The departure in 1995 from the normal seasonal  
21 invariance in velocity seems to be in our model influenced by the climate forcing  
22 (see SI, Figs. S2, S9, and S12(A, B)). This indicates that, as suggested by Luckman  
23 and Murray (2005), the 1995 anomalously high melt year (see Figs. S2 and S3)  
24 may have potentially contributed to JIs retreat and flow acceleration during this  
25 period. The modelled velocities for 1992 and 1995 are consistent with observed  
26 velocities for the same period (Joughin et al., 2008; Vieli et al., 2011). In 1996 and  
27 1997, the frontal extent and the grounding line position remain relatively stable  
28 (Figs. 2 and 6), and no significant seasonal fluctuation in ice flow rate is observed.  
29 These model results agree well with observations, which indicate that the glacier  
30 speed was relatively constant during this period (Luckman and Murray, 2005).

31 **• 1998–2002**

According to observations (Joughin et al., 2004; Luckman and Murray, 2005; Motyka et al., 2011; Bevan et al., 2012), the initial acceleration of JI occurred in May-August 1998, which coincides with our modelled results. 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). Thinning, both near the terminus and inland (up to 10 km away from the 1990 front position), starts in our model in the summer of 1995 and continues to accelerate after 1998 (Figs. 3 and 6). These findings are corroborated both by observations (SI, Fig. S15) and modelling results (Fig. 3). Although thinning appears to have increased in our model during three continuous years we find little additional speedup during the period prior to 1998 (Figs. 2, 6, and S7). According to our simulation, JI's speed increased in the summer of 1998 by ~ 80% relative to the summer of 1992 (Fig. 3). Our model suggests a retreat of the grounding line position starting in 1998 that accelerates thereafter (Figs. 2, 6, and S7). The period between 1999 and 2002 is in our simulation characterized by a temporal uniform flow, with no episodes of significant terminus retreat. Overall modelling results suggest an advance of the terminus between 1999 and 2000 and a retreat of the southern tributary between 2000 and 2002 by ~4 km, which correlates with existing observations (Thomas, 2004). Concurrent with the 1998-2001 terminus retreat, the grounding line retreats in our model by ~6 km (Figs. 2 and 6). Calving and thinning near the front continuous in 2002 and results in decrease in resistive stresses at the terminus (see SI, Figs. S7 and S8).

#### • **2003–2014**

In the late summer of 2003, an increase in flow velocity is observed (Fig. 3), which is driven in our simulations by the final breakup of the ice tongue (see Figs. 2 and 6). The period, 2002-2003, is characterized by substantial retreat of the front (~4-6 km) and the grounding line (~4 km), which starts in June 2002 and continues throughout 2003. By December 2003 the terminus has retreated back to the position of the grounding line (see Figs. 2 and 6). The retreat that occurred in 2003 and the loss of the floating tongue caused a major decrease in resistive stresses near the terminus (see SI, Figs. S7 and S8). By 2004 the glacier has thinned significantly, both near the front, and further inland in response to a change in the near-terminus stress field

(Figs. 3 and 6). During the final breakup of the ice tongue, JI reached unprecedented flow rates, which in our simulation are as high as  $20 \text{ km a}^{-1}$  (~ 120% increase relative to 1998). The velocities decreased to  $16 \text{ km a}^{-1}$  (~ 80% increase relative to 1998) in the subsequent months, and JI remains relatively stable with high seasonal fluctuations. The high velocities observed at JI after the loss of its floating tongue are further sustained in our simulation by the thinning that occurred in 2004 onward (see Fig. 3), which continues to steepen the slopes near the terminus (see Fig. 6), and by a seasonal driven (sub-annual scale) retreat and advance of the front. This thinning is combined in the following years with a reduction in surface mass balance due to increased melting and runoff (van den Broeke et al., 2009; Enderlin et al., 2014, Khan et al., 2014). The period 2004-2014 is characterized in our simulation by relatively uniform velocity peaks with strong seasonal variations (Fig. 3). During this period, the terminus remained close to the grounding line with no episodes of significant retreat.

In agreement with previous studies (e.g. Joughin et al. 2012), our results suggest that the overall variability in the modelled horizontal velocities is a response to variations in terminus position (see SI, Fig. S7 and Sect. 1.4 for more details). The terminus retreat is mostly driven in our model by the sub-shelf melting parametrization applied (see SI, Sect. 1.2.5 and Figs. S5, S12). In terms of seasonality, our results suggest that most of the sub-annual signal in the model is climate driven (see SI, Sect. 1.4 and Fig. S12).

### **3.1.2 Ice mass change**

Figure 4 shows observed and modelled mass change for the period 1997 to 2014. 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). Overall there is good agreement between modelled and observed mass change (see Fig. 4), and our results are in agreement with other similar studies (Howat et al., 2011; Nick et al., 2013). Dynamically driven discharge is known to control Jakobshavn's mass loss (Nick et al., 2013). The modelled cumulative mass loss is 269 Gt, of which 93% (~251 Gt) is determined to be dynamic in origin while the remaining 7% (~18 Gt) is attributed to a decrease in SMB (see Fig. 4). Further, the present-day unloading of ice causes the Earth to respond elastically. Thus, we can use modelled mass changes to predict elastic uplift. We compare modelled changes of the Earth's elastic response to changes in ice mass to uplift observed at four GPS sites (see Fig. 5

1 and SI, Sect. 3). Both model and observations consistently suggest large uplift near the JI  
2 front and somewhat minor uplift rates of few mm a<sup>-1</sup> at distances of >100km from the ice  
3 margin.

4 Although the terminus has ceased to retreat in our simulations after 2009 (see Fig. 6 and SI,  
5 Fig. S7), the mass loss, and most important the dynamic mass loss, has continued to  
6 accelerate (see Fig. 4). Our results show (SI, Figs. S7 and S8) that during this period the mass  
7 change is mostly driven by the sub-annual terminus retreat and advance, which continues to  
8 generate dynamic changes at JI through seasonal (sub-annual scale) reductions in resistive  
9 stresses.

### 10 **3.2 Feedback mechanisms, forcings and limitations**

11 Representing the processes that act at the marine boundary (i.e. calving and ocean melt) are  
12 significantly important for understanding and modelling the retreat/advance of marine  
13 terminating glaciers like JI. Determining terminus positions by using a physical based calving  
14 law with horizontal strain rates (see Sect. 2.1.2) is motivated by the model's ability to  
15 maintain realistic calving front positions (Levermann et al., 2012). Although, the calving law  
16 was designed and primarily used for modelling large ice shelves specific to the Antarctic ice  
17 sheet, our results show that the calving law also performs well for the narrow, deep fjords  
18 characterized by JI (see Fig. 2). The benefit of using such a calving law is that it can evolve  
19 the terminus position with time and thus, potential calving feedbacks are not ignored. As the  
20 terminus retreats, the feedback between calving and retreat generates dynamic changes due to  
21 a reduction in lateral shear and resistive stresses (see SI, Figs. S7 and S8). In a simulation in  
22 which the terminus position is kept fixed to the 1990s position, the velocity peaks are uniform  
23 (i.e. no acceleration is modelled except for some seasonal related fluctuations generated by  
24 the climatic forcing applied) and the mass loss remains relatively small (~ 70 Gt). Therefore,  
25 consistent with Vieli et al. (2011), we find that this feedback between calving and retreat is  
26 highly important in modelling JI's dynamics.

27 As introduced in Sect. 2.1.2, our approach here is to adjust the terminus in the JI region to  
28 simulate the 1990s observed front position and surface elevation based on aerial photographs  
29 and available satellite altimetry observations (Csatho et al., 2008). The glacier terminus in  
30 1990s is known to have been floating (Csatho et al., 2008; Motyka et al. 2011), but details  
31 regarding its thickness are not known. Motyka et al. (2011) calculated the 1985 hydrostatic

1 equilibrium thickness of the south branch floating tongue from smoothed surface DEMs and  
2 obtained a height of 600 m near the calving front and 940 m near the grounding zone. In this  
3 paper however, we choose to use a more simplistic approach in which we compute the  
4 thickness as the difference between the surface elevation and the bed. This implies that our  
5 simulations start with a grounded terminus. The geometry of the terminus plays an important  
6 role in parameterizing ice shelf melting, and therefore our choice could directly affect the  
7 magnitude of the basal melt rates (see SI, Sect. 1.2.8). As expected, the difference in  
8 geometry results in modelled basal melt rates slightly larger than those obtained by Motyka et  
9 al. (2011). As shown in Fig. 6, the glacier starts to develop a large floating tongue in 1999 and  
10 the model is able to simulate with much accuracy its breakup that occurred in late summer  
11 2003 and the subsequent glacier acceleration.

12 Starting in 2010 the retreat of the terminus did not correlate well with observations (see Fig.  
13 2). The terminus and the grounding line retreat does not cease after 2010. Further, observed  
14 front positions (Joughin et al., 2014) suggests that by summer 2010 JI was already retreating  
15 over the sill and on the overdeepning indicated by the red star in Fig. 6. The observed retreat  
16 is not reproduced in our simulations (see Fig. 6) suggesting that additional feedbacks and/or  
17 forcings must continue to disturb the glacier. These feedbacks may not be well represented  
18 (e.g. missing physics, inaccuracies in climatic or oceanic conditions) or simply may not be  
19 captured by the model due to various limitations (e.g. bed topography model constraints and  
20 grid resolution; see SI, Sect. 1.3 for more details). As detailed in SI, Sect. 1.3.2, the basal  
21 topography of JIs channels represents a large source of uncertainty. The terminus retreat is  
22 mostly driven in our model by the sub-shelf melting parametrization that we applied (see SI,  
23 Fig. S4) that is highly dependent on the bed geometry. In our simulation the grounding line  
24 shows stabilization downstream of the sill after 2005 (see Fig. 2 and Fig. 6), which is in  
25 accordance with previous modelling studies (Vieli et al., 2001, Vieli et al., 2011). The  
26 grounding line behaviour is crucial for the dynamics of marine outlet glaciers, as its migration  
27 removes areas of flow resistance at the base and may trigger unstable retreat if the glacier is  
28 retreating into deeper waters. Vieli et al. (2011) found that by artificially lowering the same  
29 bed sill with 100 m, the grounding line eventually retreats and triggers a catastrophic retreat  
30 of 80 km in just over 20 years. Similar to Vieli et al. (2011), the grounding line in our  
31 simulation does not manage to retreat upstream over the shallow sill. In an equivalent  
32 experiment performed with our model, lowering the bed sill by 100 m, did not result in a  
33 retreat of the grounding line over the sill.

1 From a climatic perspective, the summer of 2012 was characterized by exceptional surface  
2 melt, covering 98% of the entire ice sheet surface, including the high elevation Summit region  
3 (Nghiem et al., 2012; Hanna et al., 2014). Overall, the 2012 melt-season was two months  
4 longer than the 1979–2011 mean and the longest recorded in the satellite era (Tedesco et al.,  
5 2013). Furthermore, the summer of 2012 was preceded by a series of warm summers (2007,  
6 2008, 2010 and 2011) (Hanna et al., 2014). Over the average surface melt was already  
7 recorded in May-June 2012 (see Fig. 3 from NSIDC (2015)) when most of the 2011-2012  
8 winter accumulation melted and over 30% of the ice sheet surface experienced surface melt.

9 An intense and long melt year leads to extensive thinning of the ice, and has the potential to  
10 enhance hydrofracturing of the calving front due to melt water draining into surface crevasses  
11 (MacAyeal et al., 2003; Joughin et al., 2013; Pollard et al., 2015) resulting in greater and/or  
12 faster seasonal retreat and an increase in submarine melt at the terminus and the sub-shelf  
13 cavity (Schoof, 2007; Stanley et al., 2011; Kimura et al., 2014; Slater et al., 2015). The  
14 seasonal retreat of JIs terminus started relatively early in 2012, with a large calving event  
15 having already occurred in June. While it seems difficult to attribute this particular calving  
16 event solely to processes related with the 2012 melt season, it does seem probable that the  
17 series of warm summers (2007-2011) together with the 2012 exceptional melt season could  
18 have enhanced hydrofracturing of the calving front and consequently could have induced a  
19 retreat of the terminus which cannot be captured by the model (i.e. in its present configuration  
20 the model does not account for the influence of meltwater runoff and its role in the subglacial  
21 system during surface melt events). In our model, the climatic forcing applied can influence  
22 Jl's dynamics only through changes in surface mass balance (SMB) (i.e., accumulation and  
23 ablation) (see SI, Sect. 1.2.5). Our results suggest that most of the sub-annual signal in the  
24 model is climate driven (see SI, Sect. 1.4 and Fig. S12). A comparison between a simulation  
25 that includes the full climatic variability (monthly temperature and SMB from RACMO2.3)  
26 and a simulation with constant climatic forcing (mean 1960-1990 temperature and SMB)  
27 indicates that the two accelerations, in 1998 and 2003, are related to bed geometry and ocean  
28 melt. Furthermore, our results show that some seasonal velocity peaks could potentially be  
29 influenced by the climatic forcing applied (see Figs. S9 and S12(A,B)). This suggests that  
30 even though the climate does not trigger and sustain long accelerations, the climate certainly  
31 does have the capacity to contribute and accentuate the processes that are responsible for  
32 these accelerations. The modelled sub-annual signal in terms of terminus retreat and velocities  
33 does not always correlate with the observed signal, suggesting that potentially different

seasonal forcings (e.g. ice mélange variability, seasonal ocean temperature variability) may influence the advance and retreat of the front at seasonal scales. The ice mélange can prevent the ice at the calving front from breaking off and therefore could reduce the calving rates. The introduction of an ice mélange parametrization will probably help to minimize some of the sub-annual noise observed in our simulations (see Fig. 3). Furthermore, the 2 km resolution used in this study may not be sufficient to accurately model the seasonal retreat and advance of the front. The smallest calving event in our model is 4 km<sup>2</sup>, which is larger than most of the calving events observed at JI (see SI, Sect. 1.3.1).

Concerning the ocean conditions, warm water temperatures in the fjord were recorded in 2012. Besides a cold anomaly in 2010, which was sustained until early 2011, the period 2008-2013 is characterized by high fjord waters temperatures - equal to or warmer than those recorded in 1998-1999 (Gladish et al., 2015). In our model, the ice melt rates are determined from the given conditions in temperature (-1.7 °C), and salinity (35 psu) of the fjord waters, and the given geometry (see Sect. 2.1.3 and SI, Sect. 1.2.5). Although, the ocean temperature is scaled based on a virtual temperature that depends on the geometry of the shelf, the 1998 and 2003 accelerations can be modelled without additional variability in ocean temperatures. Further our results suggest that these accelerations are most likely driven by internal glacier dynamics and bed geometry, and not by an increase in e.g. ocean temperature. The fact that we are able to model JIs retreat with no variability in ocean temperature suggests that the retreat and acceleration observed at JI are likely not caused by a year to year variability in ocean temperatures. This conclusion agrees with the observational study of Gladish et al. (2015) who analysed ocean temperature variability in the Ilulissat fjord with JI variability and who found that after 1999 there was no clear correlation. Our results do not, however, imply that the ocean influence in JI's retreat is negligible (see Fig. S5), but rather that the glacier most likely responds to changes in ocean temperature that are sustained for longer time periods. Two additional experiments, where the input ocean temperature ( $T_o$ ) was increased to -1 °C indicate that higher melt rates beneath the grounding line could potentially explain the retreat observed after 2010. In the first experiment, the input  $T_o$  was increased from -1.7 °C to -1 °C between 1997-2014 (Gladish et al., 2015). This generated in our simulation, for the period 1997-2014, an accelerated retreat of the front that does not correlate with observations, and mass loss estimates significantly larger (by ~ 50 %) than those calculated from airborne and satellite altimetry observations (see SI, Sect. 2). In the second experiment,  $T_o$  was increased to -1 °C between 2010-2014 (with  $(T_o - T_f) \sim +0.7^\circ\text{C}$  at the base of the shelf in 2010),



1 and generated in our simulation, for the period 2010-2014, a faster retreat of the front that  
2 correlates well with observations, and an increase of mass loss by ~7 Gt.

#### 4 **Conclusions**

5 In this study, a three dimensional, time-dependent regional outlet glacier model is used to  
6 investigate the processes driving the dynamic evolution of JI and its seasonal variation in  
7 velocity between 1990 and 2014. Here, we attempt to model and understand the recent  
8 behaviour of JI with a process-based model. The model parameters were calibrated such that  
9 the model reproduces the frontal positions (Fig. 2) and ice mass change observations (Fig. 4)  
10 at JI over the periods 1990-2014 and 1997-2014, respectively~~observed at JI during the period~~  
11 ~~1990-2014.~~ We obtain a good agreement of our model output with measured horizontal  
12 velocities, observed ice thickness change. ~~observed mass change,~~ and GPS derived uplift  
13 (Figs. 3, ~~4~~ and 5).

14 ~~We find that our model is not able to simulate the 2012 spike in flow velocity observed at JI,~~  
15 ~~however, we are able to capture the overall trend in the observed velocities (Fig. 3). The~~  
16 ~~acceleration that characterizes JI during 2012 is likely the result of an exceptionally long~~  
17 ~~melting season dominated by extreme melt events (Nghiem et al., 2012; Tedesco et al., 2013;~~  
18 ~~Hanna et al., 2014) and may not be caused by a retreat of the JI terminus to the bottom of an~~  
19 ~~overdeepened basin (Joughin et al., 2014). Modelling the influence of surface processes in~~  
20 ~~flow acceleration only by means of input data from climate models and without an adequate~~  
21 ~~account of supraglacial water storage, drainage, and discharge patterns as well as their impact~~  
22 ~~on flow, results in models that are not able to capture the acceleration caused by such intense~~  
23 ~~melt events. Considering that the GrIS is highly sensitive to changes in the regional and~~  
24 ~~global climate and that such events are expected to intensify in the future (Keegan et al.,~~  
25 ~~2014), our findings suggest that current projections of 21st century mass loss may be~~  
26 ~~underestimated.~~

27 Our results suggest that most of the JI retreat during 1990-2014 is ocean and bed geometry  
28 driven, and that the overall variability in the modelled horizontal velocities is a response to  
29 variations in terminus position. The seasonal variability observed in our simulations is  
30 climate driven. In its present configuration, the model does not account for seasonal ocean

temperature and ice mélange variability that may influence the seasonal advance and retreat of the front.

For the period 1990-2010, the model is able to capture the overall retreat of the terminus and the trend in the observed velocities (see Figs. 2 and 3). The 2010-2012 observed terminus retreat (Joughin et al., 2014) is, however, not reproduced in our simulations, likely due to inaccuracies in basal topography, or misrepresentations of the climatic and oceanic forcings.

Our model results provide evidence for two distinct flow accelerations in 1998 and 2003, respectively. ~~The first was generated by an increase in surface slope and thinning prior to 1998; the latter was triggered by the final breakup~~ of the floating tongue. During this period, JI attained unprecedented velocities as high as 20 km a<sup>-1</sup>. Additionally, the final breakup of the floating tongue generated a reduction in buttressing that resulted in further thinning. As the slope steepened inland, sustained high flow rates have been observed at JI over the last decade. In accordance with previous studies (Thomas, 2004; Joughin et al., 2012), our findings suggest that the speed observed today at JI is a result of thinning induced changes and a reduction in resistive stress (buttressing) near the terminus correlated with inland steepening slopes. Both model and observations suggest that JI has been losing mass at an accelerating rate, and that the glacier has continued to accelerate through 2014 (Fig. 4). Similar to previous studies (Nick et al., 2009; Vieli et al., 2011; Joughin et al. 2012), our results show that the dynamic changes observed at JI are triggered at the terminus. In our model, the terminus retreat is mostly driven by the sub-shelf melting parametrization applied. Thus, our results suggest that ocean forcing is the principal driver for the retreat observed over the last 2 decades. Further, our model provides evidence that the rapid accelerations of JI in 1998 and 2003 are most likely triggered by the bed geometry and internal glacier dynamics, and not by a sudden increase in e.g. ocean temperature.

## **Author contributions**

I.S.M. was responsible for the numerical modelling part. J.B. provided the bed model. M.R.V.D.B. and P.K.M. provided climate data. S.A.K and B.W. provided observational data. I.S.M. and S.A.K created the figures and wrote the manuscript with contributions from A. A. J.B., T.V.D., M.R.V.D.B, B.W., P.K.M, K.K., and C.K.

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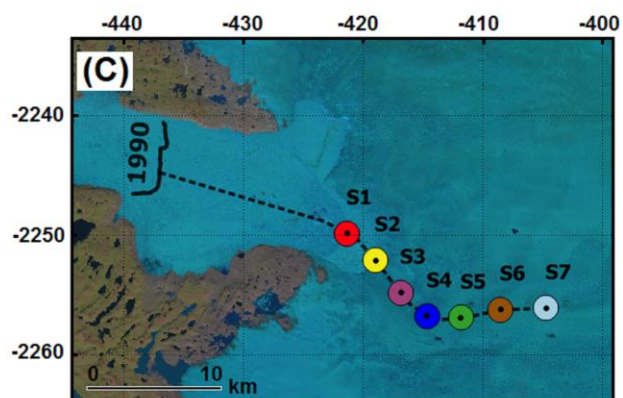
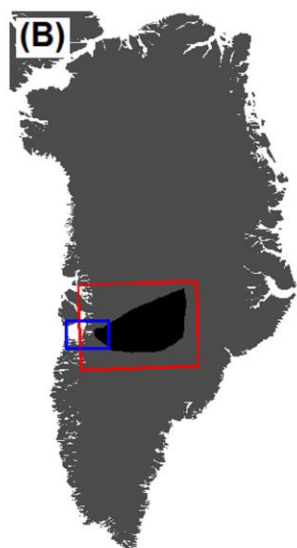
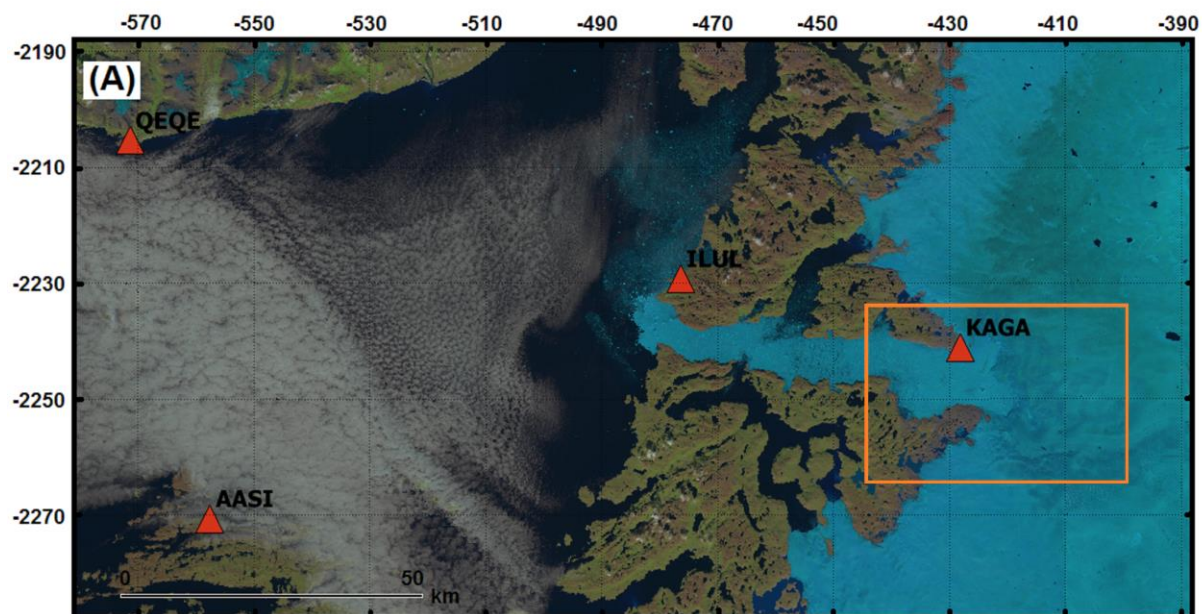
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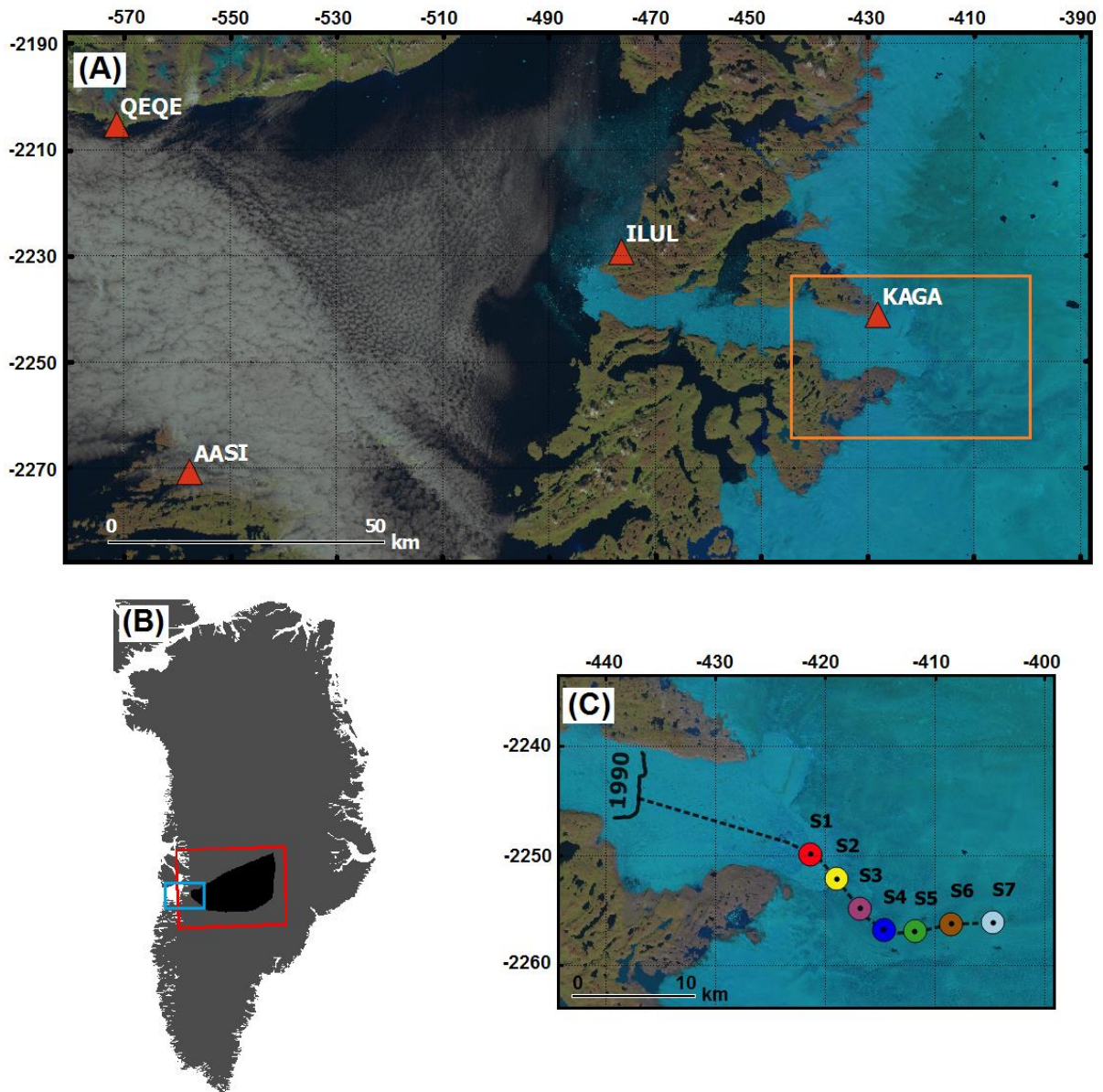
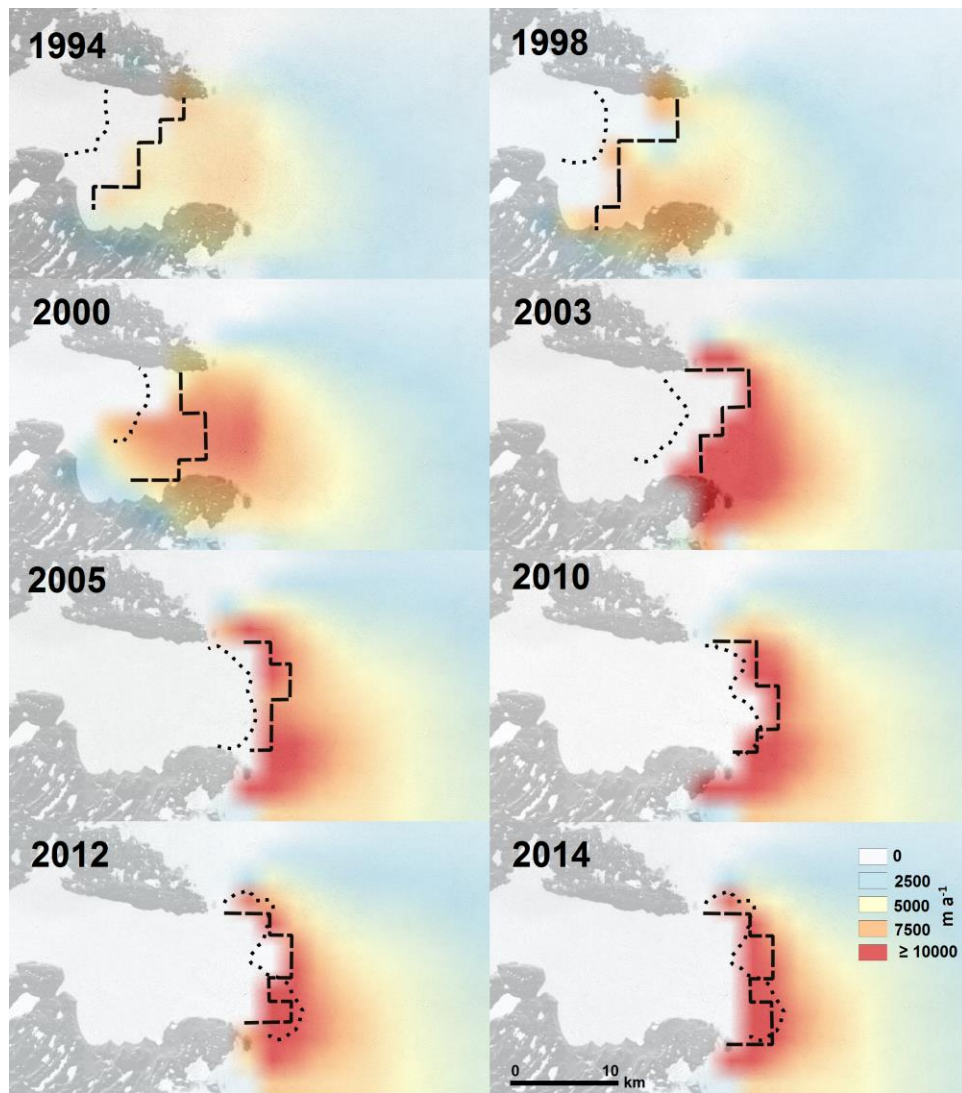


Figure 1. (A) Landsat 8 image of Ilulissat fjord and part of Disko Bay acquired in August 2014. The dark orange triangles indicate the GPS station locations (GPS data shown in Fig. 5). The polygon defined by light orange borders outlines the location of Fig. 1C. (B) Grey filled Greenland ~~contour~~ map. The black filled polygon highlights the JI basin used to compute the mass loss (Fig. 4) and is identical to Khan et al. 2014. The polygon defined by red borders indicates the computational domain. The light blue border polygon represents the location of Fig. 1A. (C) Coloured circles indicate the locations plotted in Fig. 3. The thick black line denotes the JI terminus position in 1990s. The dotted black line represents the flow-line location plotted in Fig. 6. The coordinates given outside defining the image gridding (A) and (C) are in polar-stereographic projection units (km).





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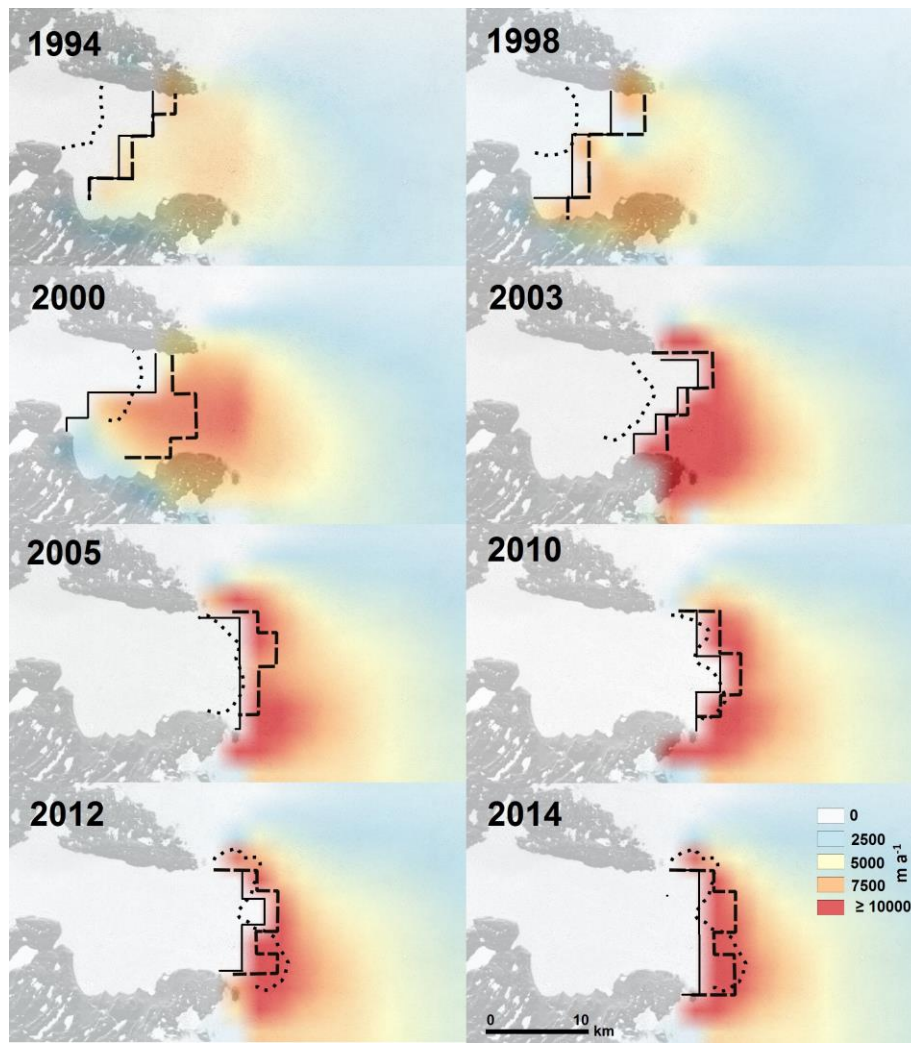
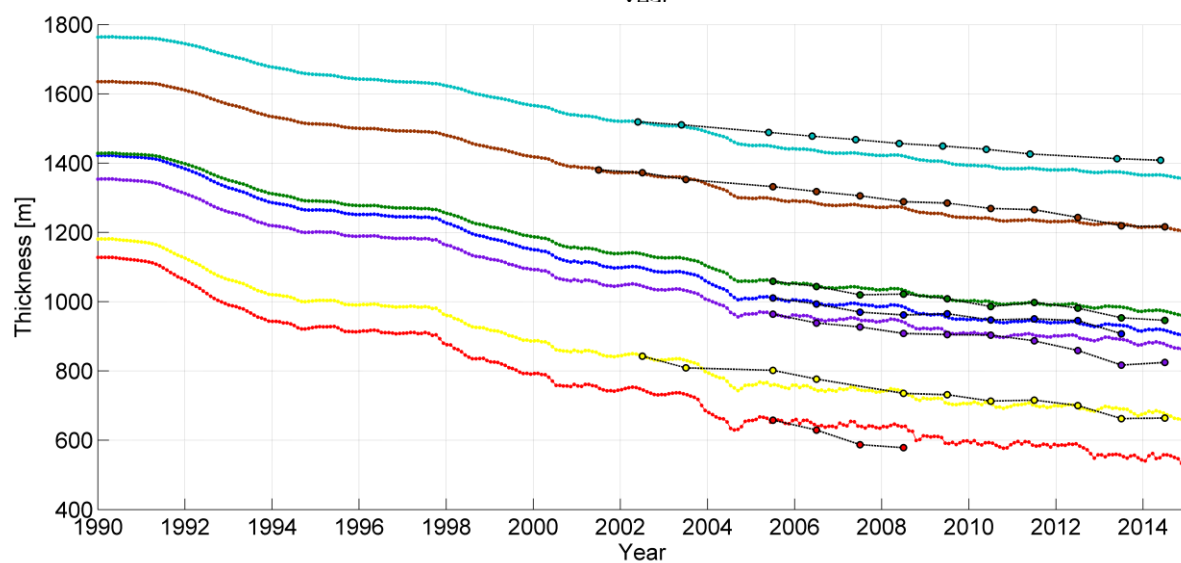
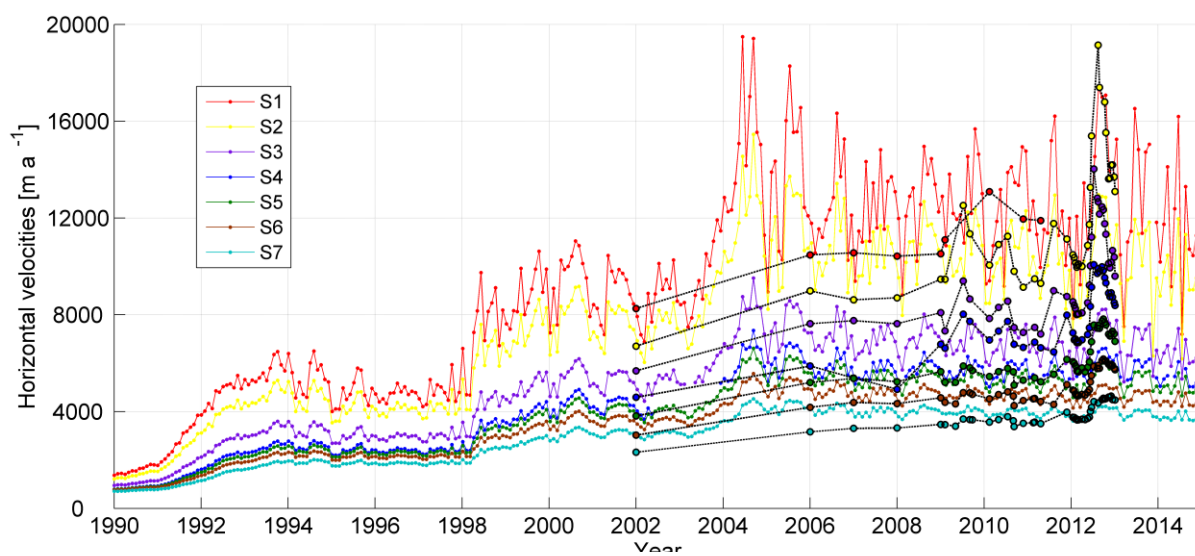


Figure 2. Modelled velocities at Jakobshavn Isbræ for December are shown for seven different years. The black dotted line denotes the observed front position and the thick black dashed line represents the modelled grounding line position. The velocities are displayed imposed over a Landsat 8 image acquired in August 2014.





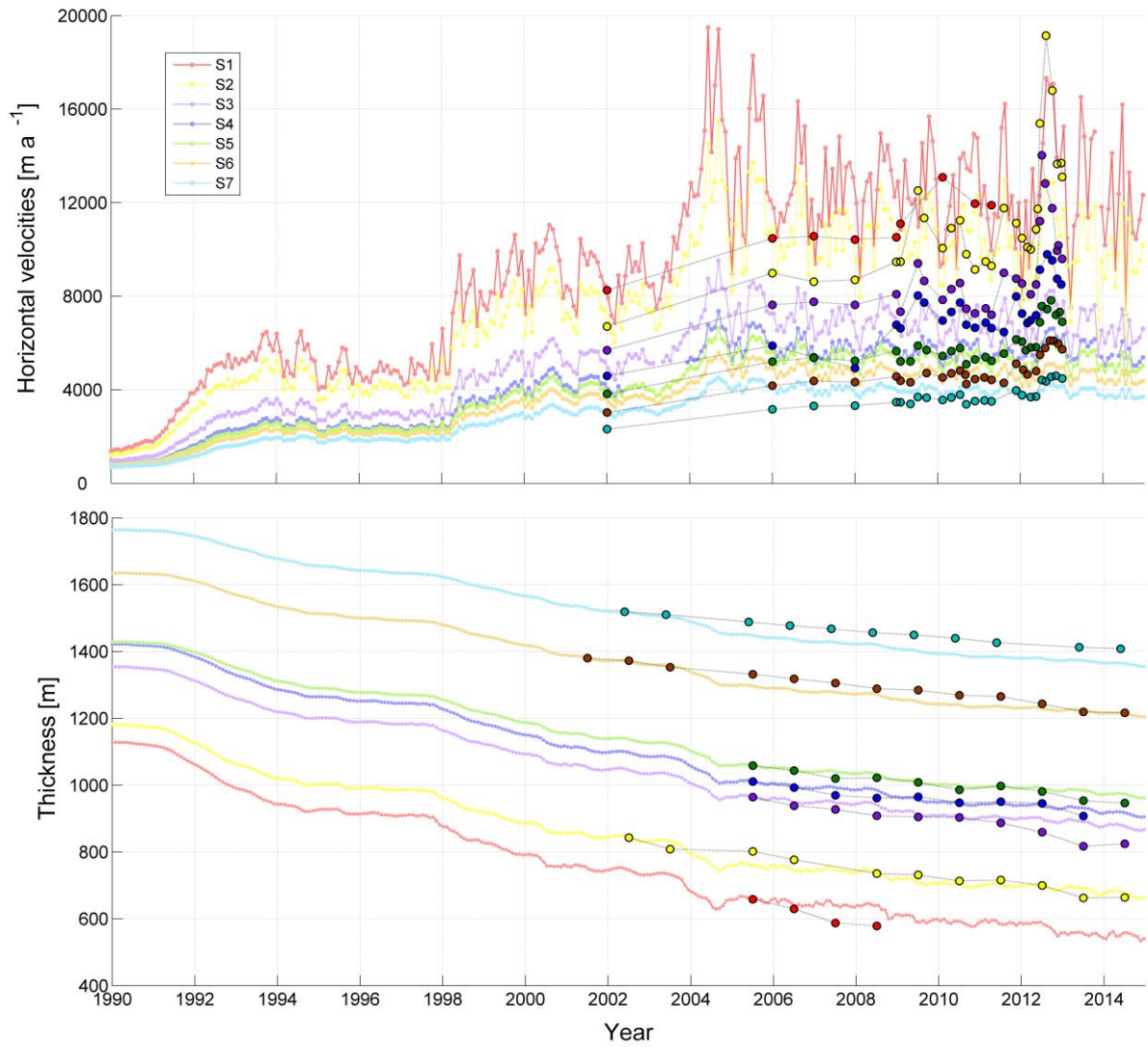


Figure 3. (A) Time series of modelled (filled circles) versus observed (filled circles with black edges) velocities (Smith-Joughin et al., 2010) (top figure) and ice thickness changes (Krabill, 2014) (bottom figure) for the period 1990-2014 at the point-locations (S1 to S7) shown in Fig. 1C. The same colour scheme is used for the modelled and the observed data. The observed thickness has been adjusted to match the model thickness at the first year.

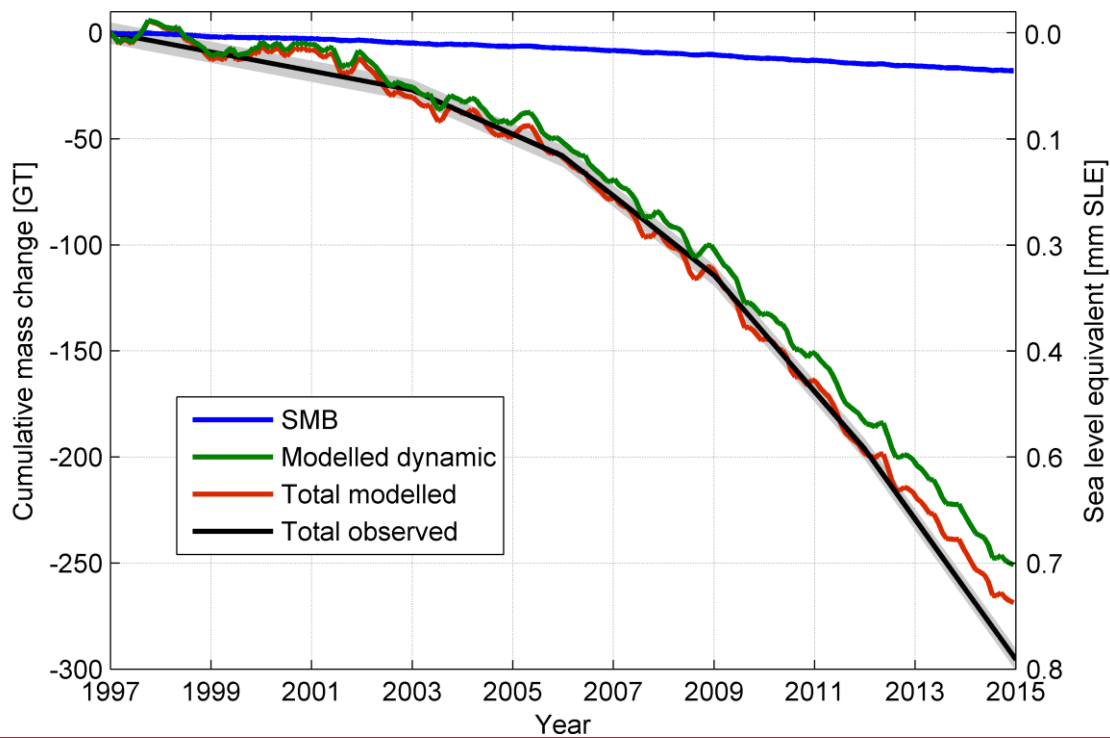
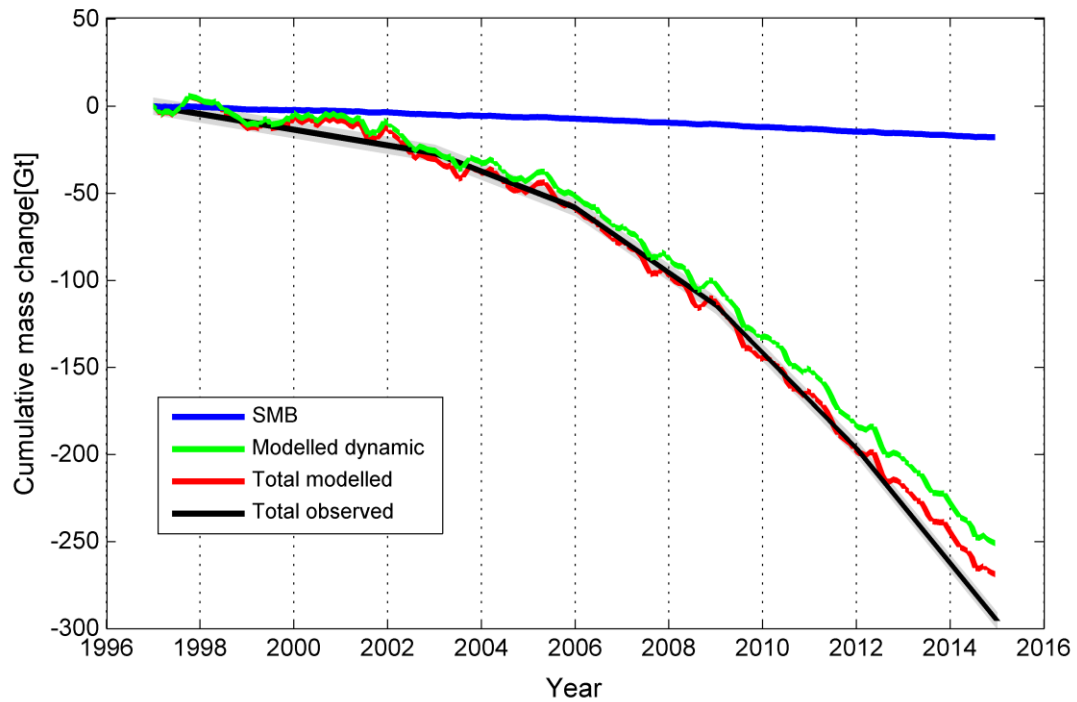


Figure 4. Modelled and observed cumulative mass change for Jakobshavn Isbræ. The blue curve represents the mass change due to the SMB variability after the 1960-1990 baseline is removed. The green curve represents the modelled ice dynamics mass change. To estimate the mass change due to changes in ice dynamics, we subtract the SMB mass change (as calculated

based on RACMO 2.3 (Noël et al., 2015)) from the total modelled mass change. The red curve represents the total modelled mass change including both SMB and ice dynamic changes. The black curve with grey error limits represents the total observed mass change including both SMB and ice dynamic changes. The modelled mass change for the period 1997-2014 is ~269 Gt and the observed mass change is ~296 Gt.

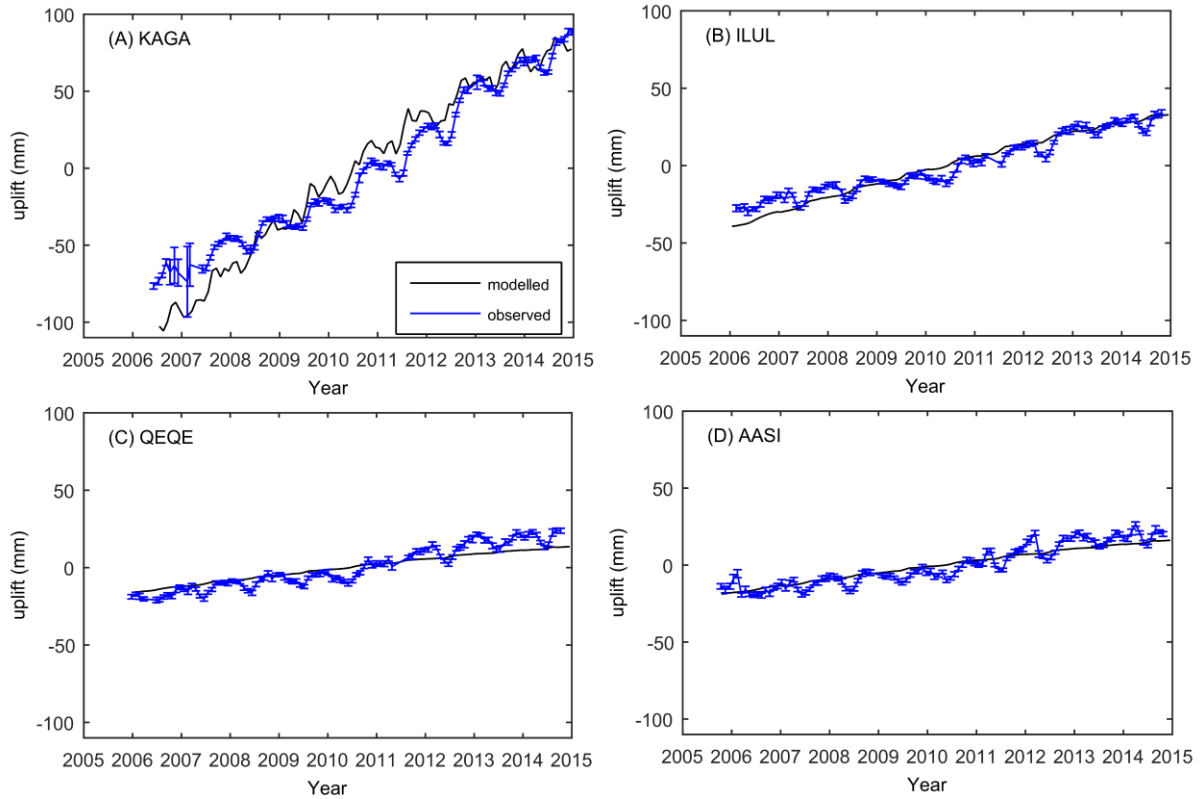


Figure 5. Observed versus modelled uplift in mm for the stations KAGA (A), ILUL (B), QEQE (C) and AASI (D). The positions of the four GPS stations are presented in Fig. 1A.

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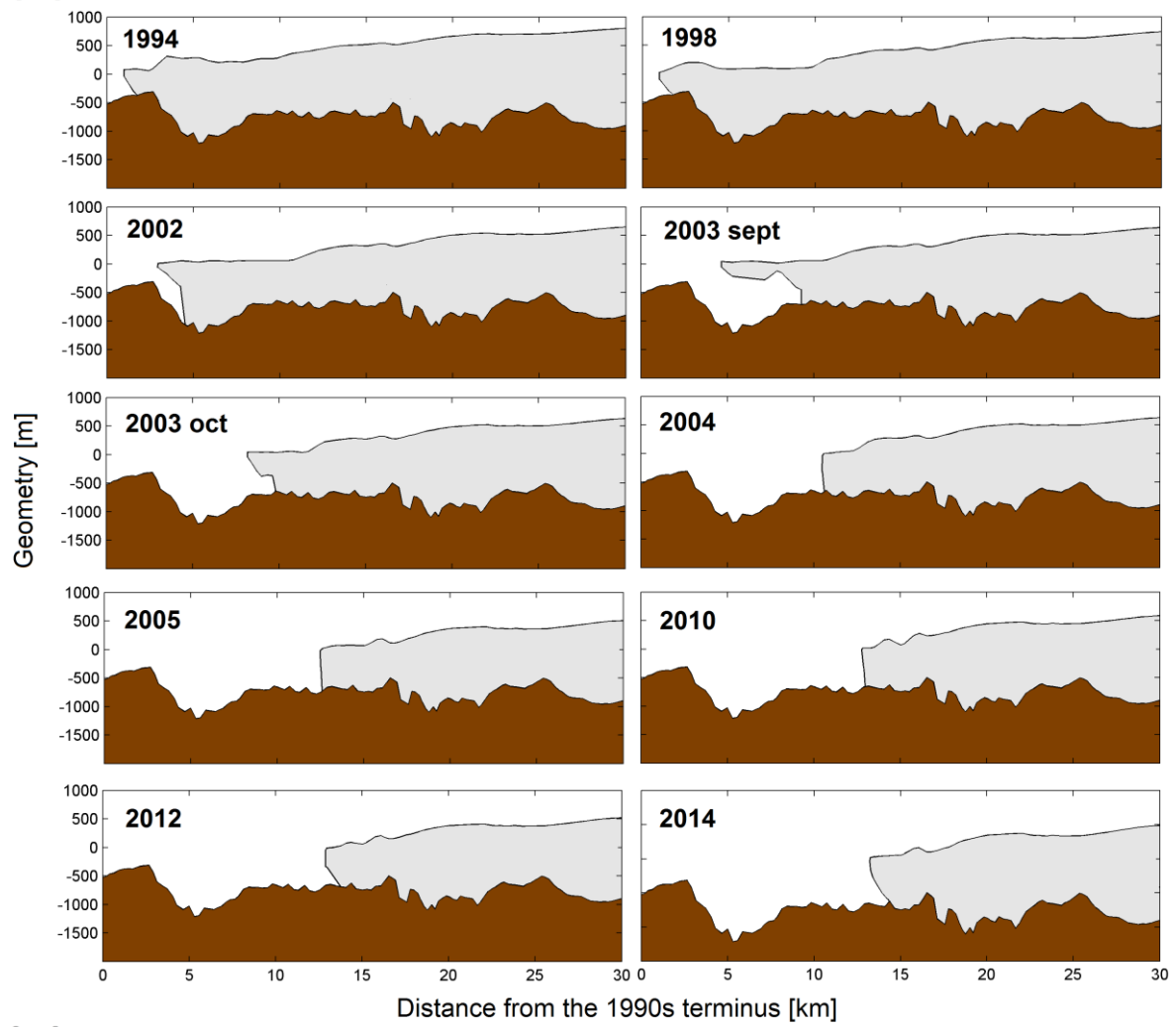
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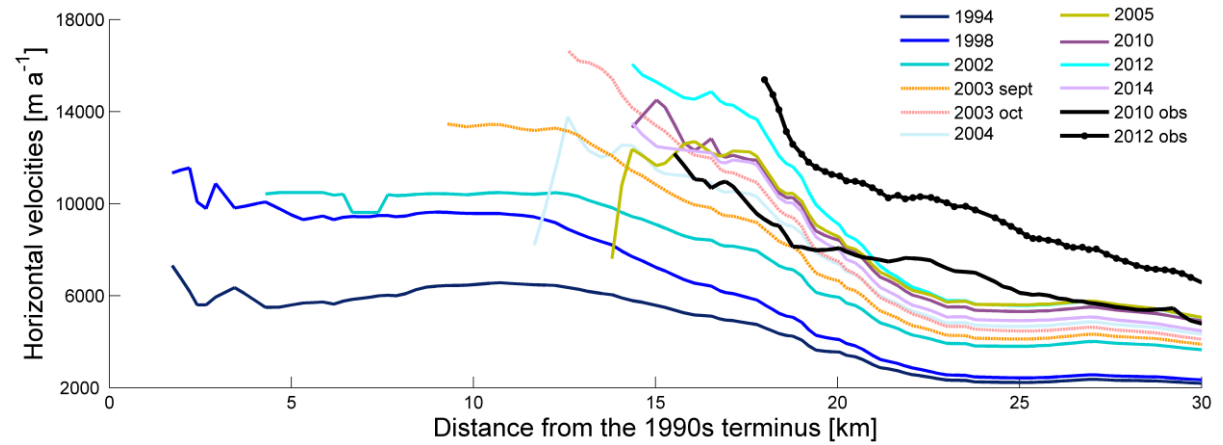
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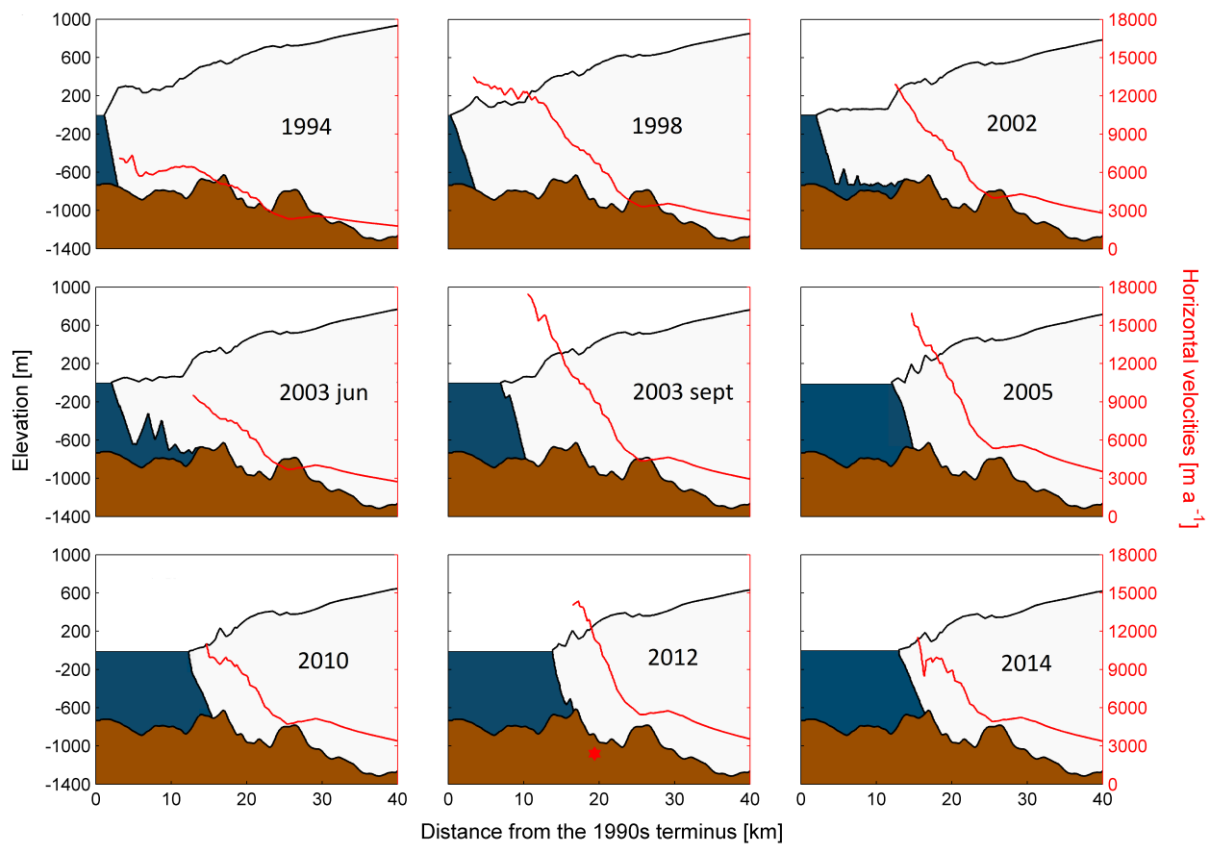


Figure 6. Modelled evolution of surface elevation (ice shelves thinner than 50 m are not shown) (A) and horizontal velocities (B) of Jakobshavn Isbræ for December along the flow-line shown in Fig. 1C. Note the acceleration in speed (B) between 1994-1998 and between September-June 2003 (orange) and October-September 2003 (light pink) corresponding to the final breakup of the floating tongue. The red star denotes the observed 2012 terminus position. The black lines in (B) denote observed horizontal velocities as produced from TerraSAR-X (TSX) image pairs collected between 20 November 2010–01 December 2010 and 08 December 2012–19 December 2012 (Joughin et al., 2010; Joughin et al., 2011).