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

1 General statement

With the manuscript "Modelled glacier dynamics over the last quarter of a century at Jakobshavn Isbræ", Muresan et al. use numerical modeling to simulate the behavior of Jakobshavn Isbræ between 1990 and 2014 and compare their simulations with observations of ice front position changes and mass loss from the glacier. They conclude that bedrock elevation and ocean forcings have mainly controlled the evolution of the grounding line and calving from positions over the last twenty-five years and that most of the seasonal signal is driven by climate forcings.

This manuscript aims to understand the processes and mechanisms that have triggered the destabilization of Jakobshavn Isbræ, and to reproduce its evolution since the 1990's. This is a very complex topic that many previous studies have already partially addressed. The subject of the study and the approach are compelling but they are many limitations and inconsistencies between the different sections. For example, the main text states that using a grid with a 2 km resolution probably impacts the results while the supplementary material states that using a 1 km resolution grid does not alter the results.

The text is also sometimes poorly written and it took me several readings to actually figure out how the ocean forcing was used, or to understand the conclusions regarding the impact of the climate forcing on ice dynamics. Several statements are not supported by any results (i.e. p.11 l.8, the seasonal signal is climate driven) while strong conclusions are made from these statements.

Finally, the values provided in Table S3 for the melt rates seem excessively high (1387 m/yr in 2000, there is just no way there can be any ice shelf with these kind of values). The parameterization for the ocean forcing seem is a major control of the evolution of the glacier, and there is no analysis of its impact except for a few sentences at the end of the discussion that are not supported by any material.

2 Major comments

As mentioned above, the impact of the grid resolution is not clearly investigated and explained. This could be a major limitation of the model, as calving events of 4 km^2

have a very large impact on ice dynamics. This problem is rarely mentioned in the limitations of the model (not in the abstract p.2 l.11 or the conclusions p.16 l.15 for example). Furthermore, the supplementary material suggests that using a 1 km grid would not make a big difference by running an additional stress balance simulation. Would that be similar for transient runs? I understand that there are some limitations that prevented the authors to do the simulations with a 1 km resolution, but this is a major limitation. Furthermore, it is surprising to see that the vertical resolution is 20 m, resulting in 200 vertical layers, while the horizontal grid is 310 by 213.

The grounding line is said to evolve within the grid cells according to *Feldmann et al.* [2014]. However, figures showing grounding line position do not show continuous advance and retreat of the grounding line, but rather jumps between a few positions spaced by relatively large distances (fig. S7A and fig. S12C for example). How do the authors explain this surprising behavior that usually happen when no sub-grid parameterization is used?

As mentioned above, the melt rates provided in table S3 are extremely high, especially between 1998 and 2003. This should at least be mentioned and explained. This is an ocean parameterization more that an ocean forcing.

The description of the calving should be clarified: there seems to be two criterions used, one based on the eigencalving parameterization, and the second one based on the ice thickness. How are they combined? Also the description mentions (p.5) that ice is removed at a rate of at most one grid cell per time step and sub-grid scale ice front advance and retreat are used. However the smallest calving event is said to be 2 by 2 km (one grid cell)? What is the difference between these two processes?

The eigenvalue calving has been tested on Antarctica, where large ice shelves spread in all directions. Greenland has very narrow fjords with almost no lateral velocities, which makes the across flow strain rate very small and noisy. How does this impact the results?

One of the conclusions is that climate forcing drives the seasonal (sub-annual) evolution of the front position, grounding line position and ice dynamics. However, the only climate forcing happens through SMB changes. This impacts the ice thickness and driving stress, but the changes are really small and these processes happen on much longer time scales. How can changes in SMB only trigger these large changes on very short time scales? This should be better demonstrated in the paper.

The enhancement factor chosen is equal to 0.6 for the SSA. This is a very surprising value as enhancement factors are usually greater than 1. For Greenland, the calibrated values are between 3 and 6 [*Cuffey and Paterson*, 2010]. How can this value be explained?

p.9 l.12: the authors mention that the high melt rate is responsible for the flow acceleration, and that their model is able to reproduce the acceleration. However the model does not include any process to include this high melt so it seems that the ability to reproduce the acceleration is likely to be just a coincidence.

Fig. S7 is very informative of the general behavior of the model and is a good summary of the evolution of the glacier. It should be added to the main text.

3 Minor comments

p.1 l.25: "an attempt" is a surprising word. The model should simply describe results.

p.1 l.26: "ocean parametrization" would be more accurate that "ocean forcing"

p.2 l.6: A sentence should not start with "And".

p.2 l.9: the bed geometry is not changing and does not initiate any change. Also, the floating ice does not care about bed elevations as it is already floating.

p.2 l.10: Consider changing "slight failing" to "limitation".

p.2 l.12: Observations do not "suggest", they are measurements that show the evolution.

p.3 l.4-9: I would only mention flow line models to describe their results.

p.3 l.11: "One process"

p.3 l.12: "thinning and/or retreat"

p.3 l.25: "to model and understand the recent behaviour of JI": that sounds really ambitious.

p.3 l.27: "in which the grounding lines ..."'

p.4 l.2: "Ice sheet model"

p.4 l.6: "SIA-SSA": acronyms should be defined before being used. References should also be added for these stress balance approximations.

p.4 l.5-9: Not clear, should be rephrased

p.4 l.20: what year is used for the altimetry observations?

p.4 l.27: at this point of manuscript, the reader has no clue why RACMO results should be interpolated on a 2 by 2 km grid.

p.5 l.1: "grounding line parametrization". Consider adding also "initialization procedure" or something equivalent in the section title.

p.5 l.8: What year is used for the other terminus positions?

p.5 l.14-18: Not clear, rephrase.

p.5 l.20: How is the value of k chosen?

p.6 l.1: What is "LI"?

p.6 l.17: Should add one sentence to describe the parameterization in *Feldmann et al.* [2014].

p.6 l.21: "regularly spaced layers within the ice": Are the vertical layers of uniform thickness (20 m as stated earlier) or regularly spaced between the bed and surface elevations?

p.6 l.27-29: Rephrase

p.6 l.30: Is the vertical resolution 10 or 20 m?

p.6 l.31: "integrate" → "simulate"

p.6 l.32: It is not really coupling if it is one way, but rather forcing.

p.7 l.8: "sub-shelf ice temperature": Not clear. Is this sub-shelf ocean temperature or ice shelf temperature?

p.7 l.25: The description of the ocean forcing is not clear. I read it several times and I am still not sure what is used.

p.7 l.28: What does "indirectly" mean?

p.8 l.21: "between 1990 and 2014"

p.8 l.26: "approximately 2 to 4 km": so just one or two grid cells!

p.8 l.29: " new oceanic and atmospheric conditions": what is new here? I thought the same RACMO data were used.

p.9 l.5: "Disregarding" → "Apart from"

p.10 l.3: "temporally"

p.10 l.9: "the front continuous in 2002": rephrase

p.10 l.19: "had thinned"

p.10 l.24: "JI remained"

p.10 l.26: "in 2004 onward": rephrase

p.11 l.8: How is the seasonal signal driven by climate? What processes are responsible of such variations? Investigating the different processes included in the model should allow to better answer these questions.

p.11 l.8: How does the acceleration propagate inland?

p.11 l.21: How large is the uplift?

p.12 l.4: “physically based”

p.12 l.8: The eigencalving is combined with the thickness criterion. How much of the calving is due to each of these mechanisms? As the flow is parallel to the fjord, the second value eigenvalue is close to zero and therefore this mechanism is likely not to modify the position of the front.

p.13 l.4: “The terminus and the grounding line retreats do not ...”

p.13 l.5: “suggest”

p.13 l.6: “overdeepening”

p.13 l.20: “bed sill by 100 m”

p.13 l.22: What do the authors mean by “equivalent”?

p.13 l.30: “Surface melt above average was already ...”

p.14 l.8: “related to the 2012 ...”

p.14 l.16: again, it is not clear how SMB forcing can cause sub-annual changes

p.14 l.31: “the 2 km resolution ... may not be sufficient”: the supplementary material says pretty much the opposite.

p.15 l1. “Concerning” $\rightarrow$ “Regarding”

p.15 l.18-27: Additional figures should help support these conclusions.

p.16 l.15: Another major limitation is the coarse mesh, that should also be discussed here.

Fig.1 caption: replace “polygon” by “rectangle”. “Khan et al. (2014)”

Fig.2: Eight different years are shown (not seven). By the way, why not show the results every year? How are these years picked?

Fig.3: How is the thickness adjusted? This should also appear in the text.

Fig.4: The grey error bar is hard to distinguish on the figure. Caption l.5-7 should be rephrased.

Fig. S1 (bottom right): Only three curves appear, if some are superimposed, it should be mentioned in the caption. Also the range of values used for Fmelt seems inconsistent with the values listed in Table S2.

References

Cuffey, K., and W. S. B. Paterson, *The Physics of Glaciers, 4th Edition*, Elsevier, Oxford, 2010.

Feldmann, J., T. Albrecht, C. Khroulev, P. F., and A. Levermann, Resolution-dependent performance of grounding line motion in a shallow model compared with a full-Stokes model according to the MISMIP3d intercomparison, *J. Glaciol.*, *60*(220), 2014.