<u>Anonymous Referee #2</u>

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 misplaces. We have updated the bed profile and it is now fully consistent with the profile shown in e.g Joughin et al., 2014, Nick et al., 2013 (nature).

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.

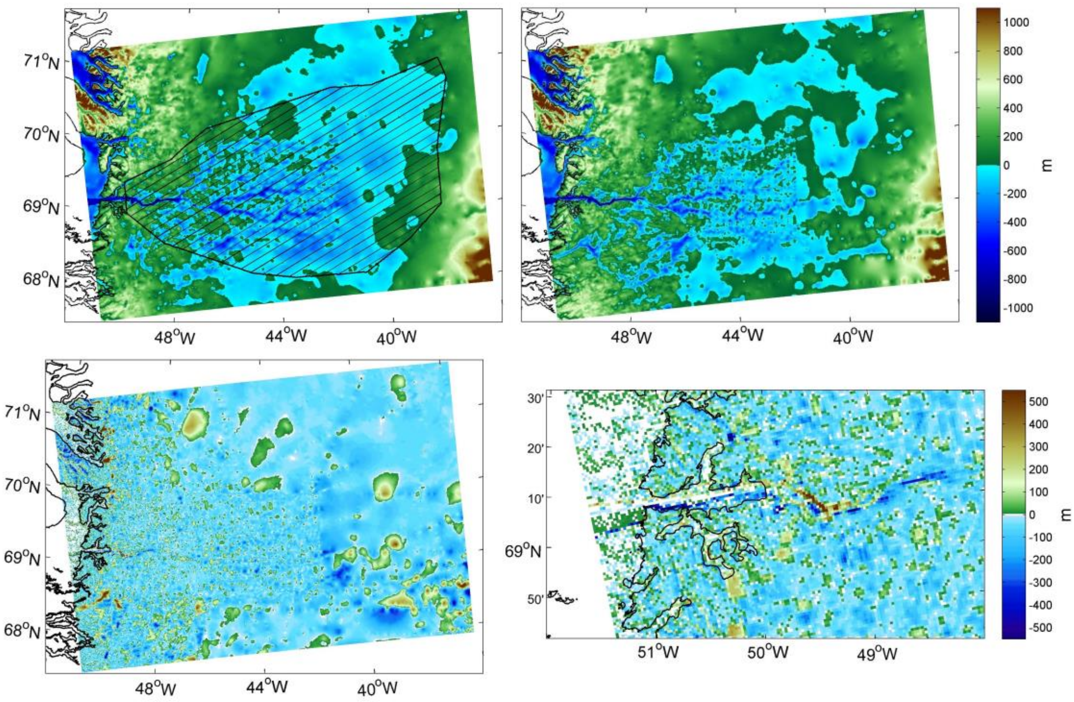

Figure 1. Bed topography from Bamber et al. (2013) (top-left) and from Morlighem et al. (2014) (top-right). The difference (M-B) is showed in the two bottom figures.

These aspects may not affect the current study, but an effort should me 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 groundling 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 respond throughout the entire simulation is not some further manifestation of this same transient? If the climate forcing is held steady at it's 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_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 lase 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.