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Interactive comment on "Simulated retreat of Jakobshavn Isbræ during the 21st century" *by* Xiaoran Guo et al.

Anonymous Referee #3

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1 Summary statement

The manuscript "Simulated retreat of Jakobshavn Isbrae during the 21st century" by X. Guo and colleagues presents results on the simulation of Jakobshavn Isbrae over the 21st century, calibrated to match its current configuration and recent evolution. The model includes buttressing provided by the ice melange and a calving law based on crevasse-depth, and the forcings are based on Global Climate Models (GCMs). The results suggest that the glacier will continue to retreat and lose mass during the 21st century, reaching 5.6 mm of sea level equivalent by the end of the century.

The paper is well written, usually easy to follow (except for the initialization procedure that is quite complicated), and the figures appropriate. However, some additional ex-

planations are needed to understand the choices made for the calibration of several parameters, for some of the datasets used, or for the initialization procedure. Furthermore, only a couple of figures show the evolution of the glacier over a flow line for one given simulation of the ensemble. It would be valuable to show the spread of the model results for the different parameters used and the different forcings, but also to show the spatial evolution of the ice front not just on a flow line but for the entire basin. Finally, the authors mention that the calving law based on crevasse-depth prevents the calving of the glacier once the thickness becomes too large. This is the contrary of what is physically expected: a tall cliff with a large height above sea level leads to more calving, so there is no reason for the calving to get reduced towards the end of the simulations.

2 Major comments

The bedrock and bathymetry used come from Jakobsson et al. (2012) and Gogineni et al. (2012), while the newer bedrock elevation maps of Greenland typically used in ice sheet modeling are Bamber et al. (2013) and Morlighem et al. (2017), so it is a rather interesting choice. There are probably good reasons for using these maps, but they are not well explained. It would be good quantify the impact of this choice on the simulations compared to other choices, or at least explain the differences expected.

The calving law is based on crevasse-depth based calving only, so that calving happens when deep surface crevasses develop in the presence of surface water. Is this representation of calving sufficient to represent the different types of calving throughout the year and as the glacier retreats to deeper grounds? It seems that the model is not able to simulate calving in winter that is becoming important towards the end of the simulation. Would a different parameterization of calving lead to different results? This is a rather important question as it contradicts the marine ice cliff instability that predicts faster and faster retreat as glaciers retreat to deeper grounds and the ice thickness increases. So what is the impact of choosing a crevasse-depth based calving? This should be addressed in more details in the discussion.

The role of melange and its parameterization are said to have a relatively large impact on the results, but the exact role of melange and the associated processes that could impact calving remains unclear. What happens to the simulations when the buttressing provided by the ice melange is removed?

The processes included in the simulations include many parameters, and these parameters are not always justified or explained. In particular what they physically represent and what the impact is for the simulations. For example: How much buttressing does the melange represent? What is the equivalent ice thickness needed to get a similar buttressing? What is the tuning scalar for the run-off in the crevasses? What ocean temperatures are used for the forcing, and how was this choice made? How are the ocean temperature converted from the far field, to the fjord and to the grounding line region?

The initialization is rather confusing, with a target date of 2004 at the beginning of the simulations, but other datasets with different times are used for the inverse problem (2012) and the relaxed surface elevation (1998). This part should be clarified to better understand the rationale behind the initialization procedure.

I am wondering how reliable the GCMs are to reproduce the temporal patterns of variability of the glacier: to my knowledge, GCMs do not get the right timing for the variability, so maybe some reanalysis data would perform better for that.

Finally, there are not many figures showing the results, e.g., the spatial distribution of ice front position at the end of the simulations for the different cases or the mass loss for the different cases (just a few numbers in the table). It would be a good addition to the paper to add a few figures to get a better sense of how this glacier could change in the future, such as the spread of results, the spatial evolution of the ice front, or the

evolution of mass loss and discharge with time.

3 Specific comments

p.2 l.26: “with” → “leading to a”

p.2 l.26: “5.6 mm sea-level-rise” → “5.6 mm of sea level rise”

p.2 l.28: Why is the model unable to reproduce the winter calving? Is that a limitation of the model parameterization, the representation of calving (maybe the crevasse-depth calving is just one mode of calving and does not cover all the cases), the initial conditions? And what do you think are the consequences of the lack of winter calving?

p.2 Fig.1 caption: as mentioned above, using Jakobsson et al. (2012) is a rather unexpected choice, so it would be good to justify it and quantify the difference in bedrock elevation between this map and the other more standard maps.

p.2 l.30: the 17km/a speed is a seasonal speed happening over a few months in summer and not an annual velocity, it would be good to mention that.

p.3 l.44: “possessed” → “had”

p.3 l.57: “far faster” → “much faster”

p.4 l.64: “must be zero at the grounding line as it begins to float”: I would rather say that it is zero under the floating tongue.

p.4 l.74: the melange does not really belong to the ice shelf or the glacier (”its floating melange”). I am also surprised to see “desintegration” associated to “melange” because the melange changes a lot seasonally, which is rather common, so I don’t understand why these changes would be qualified of desintegration.

p.4 l.80: There is also a new study on Jakobshavn by Bondzio et al. (2018) using a

2d plan view model and a different calving parameterization, so it would be good to include these results in the comparison.

p.5 l.92: “BISICLES continuum ice sheet dynamics model” → “BISICLES ice sheet model”

p.5 l.100: “in hydrostatic equilibrium”: the floating part only is in equilibrium”

p.6 l.107: “an approximate stress balance equation”: replace by the name of the approximation and a reference as they are many difference approximations of the stress balance equations

p.8 l.136-137: How do you use the ocean conditions outside of the fjord to constrain the conditions inside the fjord and close to the grounding line?

p.8 l.140: “working hypothesis”: there is not much in the discussion addressing this hypothesis and whether it was a valid one.

p.8 l.148: calving has been shown to be the main driver of the velocity Bondzio et al. (2017), so that changes in calving front positions could explain most of the dynamic changes of Jakobshavn Isbrae over the past three decades, so that changes in basal conditions indeed have a small impact for this glacier.

p.8 l.149-150: This is an interesting way to change the buttressing at the front. How different would the results be with another method to account for this buttressing?

p.8 l.152: How much buttressing does this represent? What would be the equivalent ice thickness needed to get a similar buttressing?

p.9 l.156-157: What is this runoff symbol?

p.9 l.163 and l.175: How do you link the far ocean field temperatures to the ocean temperature in the fjord and then the temperature at the grounding line?

p.10 l.178: The depth of the ocean temperatures used should depend on the geometry

of the fjord, including the highest depths of the sills. What is the depth of the ocean floor in Jakobshavn’s fjord, and are there sills blocking the entry of the deepest waters?

p.10 l.186: “last 2 decades” while the rest of the paper rather show results since 2004.

p.10 l.187: “bedrock topography and ice thickness data in the year 2009 come from Gogineni et al. (2012)”: As mentioned above, why use this dataset and not the more recent Bamber et al. (2013) or Morlighem et al. (2017) topography? Also, is this the same dataset as Jakobsson et al. (2012) shown of figure 1?

p.10 l.192-193: I am not sure to agree with this statement: the glacier was continuing to change following its ice tongue collapse as shown in Joughin et al. (2012).

Fig.3: the stiffening factor inferred with inverse problems is often difficult to physically explain and it is here mostly equal to 1. How different would the results be if it was just assumed to be equal to 1 everywhere and the basal traction coefficient was adjusted accordingly?

p.11 l.199-200: Why are the velocity from 2010 and the geometry from 2009 used in the inverse problem why the simulation is initialized to reproduce 2004?

p.12 l.201: “friction coefficient” and stiffening factor

p.12 l.210: Why is the model run until the profile matched the 1998 profile? I thought the target date was 2004?

p.12 l.211: How is it changed exactly?

p.12 l.214: Are you trying to get a stable state (How do you define stable by the way? What are the variables considered?) or to match the 2004 front position? I am also a little surprised that you are mentioning a “stable state” as the glacier has been continuously since at least the 90’s.

p.14 l.254: Why is considered to be the total calving? Is it the difference between the ice front positions in 2013 and 2004 or the sum of the annual ice front change position?

The later sounds more appropriate to ensure that the timing of retreat is appropriate.

p.16 Fig.5a: There was an earlier mention of the relative stability after 2004, but this figure actually tends to show that there is not much stability at this period, with the summer front position retreating more every year.

p.18 Fig.6: Is it the bottom or the 300 m depth temperature?

p.19 l.312, 314, 318: What GCM was used to force RACMO?

p.19 l.303: change reference: Joughin et al. (2010) probably did not guess what would happen in the 2013-2017 period.

p.20 l.326-329: are these results shown on a figure?

p.20 Table 1 and lines 335-341: the numbers in the table and in the paragraph are somehow different (2068 vs 2029 Gt of mass loss by 2100 for example), but I might have missed something. It would also be appropriate to add the results from Bondzio et al. (2018) in the comparison here.

p.22 l.384: Does the fast flow go all the way to the sides of the fjord? It is a bit surprising to me that the bedrock provides so little resistance compared to the sides.

p.22 l.399: Is it possible to test this assumption of the impact of the shear-margin weakening mechanism?

p.26 l.448: There may also be some limitations associated with the initial conditions (ice too thick close to the ice front as shown on Fig.7).

p.26 l.462: “stimulate” → “simulate”

p.28 l.496: Did you use each GCM individually or used the mean of the 7 models?

4 References

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