In the reply, the referee's comments are in *italics*, our response is in normal text, quotes and modifications from the manuscript are in blue.

Anonymous Referee #3

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 explanations 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.

Reply: Our glacier geometry data (Gogineni et al., 2012) is derived from the same institution's products as used in BedMachine V3 with data processed by the Center for Remote Sensing of Ice Sheets (CReSIS, Leuschenetal., 2010 updated 2016). For computing resource considerations, we chose the earlier product because it has 500-meter-resolution. The difference between these two bedrock elevations are shown in Fig. S7.

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.

Reply: As we state in our paper at quite some length, our model clearly does not represent winter calving, which does become more important later in observational record. This is beyond dispute.

Winter calving is poorly understood. Its mechanism could be non-hydrostatic processes including the terminus uplifting due to super-buoyant condition with the opening of basal crevasses (James et al, 2014; Xie et al., 2016; Benn et al., 2017), which is beyond the capability of our model. This means that other extensions of calving parameterizations are needed. But this is not particularly relevant to the MICI mechanism in our opinion. Winter calving can occur in later winter (Cassotto et al., 2015) when calving front height is at its annual minimum and presumably at its least vulnerable to structural failure. Hence, MICI cannot explain this type of calving. We add this point to our discussion, although we emphasize that an evaluation of MICI is not the focus of this article.

Our calving criterion does not predict lower calving rates as the glacier retreats into its over-deepened basin. Although calving front height keep increasing during retreats, dynamic thinning rises highly non-linearly along with it, which leads to formation of a thin shelf which is then vulnerable to calving.

Our modeled retreats are not in contradiction to MISI (Marine Ice Shelf Instability). Most of the glacier buttressing is from the lateral margins and not the bed, this means that e.g. the advance of the glacier in the last year due to ocean cooling does not contradict the MISI either since the calving position is not only governed by bed geometry. The cessation of retreat during the later part of our ~ 60 year simulation can be attributed to the strong flow convergence near the glacier front that largely offsets the dynamic thinning (Fig. S6). Notice that the south side of the fast-flow-area in last century is quite close to ice-free land while in later half of this century convergent flow in the south is fed by a substantial area of ice stream. We discuss all these features of calving in section 4.

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?*

Reply: Ice mélange buttressing effects played a decisive role in the recent retreating and evolving of Jakobshavn (Joughin et al., 2004; Joughin et al., 2008; Vieli et al., 2011; Nick et al., 2013). In our model, mélange buttressing does affect calving by altering the stress field that contributes to crevasse penetration depth (Eq. 12), with the sensitivity of the corresponded parameter α tested (Section 2.4). This paper aims at reproducing the evolutions of Jakobshavn in the real world where mélange buttressing matters.

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?

Reply: We explain our parameterization for external forcing into fine details in section 2.2. The verifications of our parameterizations are shown in Fig. 5 and discussed in section 4. Our buttressing parameterization gives a longitudinal resistance that equals to 0.18 times of the driving force at calving front (Eq. 10), for the instance of 2004. The tuning scalar used in model calibration for β is 0.04 - 0.075. We use ocean temperatures collected from a CTD site close to the mouth of Ilulissat fjord (Fig. 1) as an approximation of ocean temperatures near the glacier grounding line. This approximation is based on the existing of an ocean circulation within Ilulissat fjord that brings deep ocean water from outside the sill to arriving at Jakobshavn's grounding line (Gladish et al., 2015).

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.

Reply: Agreed, the statement was imprecise. We have rewritten this in response to other criticism on the method:

1) We solved the inverse problem for basal conditions (Eq. 7) and stiffening factor using 2010 velocities (Joughin et al., 2010) and 2009 geometry (Gogineni et al., 2012), following Cornford et al. (2015). Our friction coefficient and stiffening factor fields are shown in Fig. 3. Fig. S2 shows the discrepancy between observed velocity field (Joughin et al., 2010) and the velocity derived from the inversion.
2) Starting from the inversion of step 1, we let the model glacier evolve freely without calving and with zero SMB and with sub-shelf melting (γ=0.0238) forced by repeating the observed

2004 ocean temperature for 11 years until its surface elevation profile reached a state shown in Fig. S3.

3) We carried out several 10-year simulations each with different β values estimated. These simulations were forced by repeatedly applying the 2004 seasonal climate forcing so that the glacier approaches a steady state. From these, we selected the β that provided a calving front position closest to that observed in 2004. The best β here is 0.034. This is our best guess for the 2004 state. The annual minimum extent of Jakobshavn retreats ~ 2 km from 2004 to 2005 following the loss of melange butressing, but then stabilizes until 2007 (Joughin et al. 2010). Annual maximum extents are stable over the 2004-2007 period. Front velocities increase slowly from 2004-2007 (~5.9% a^{-1} Joughin et al. 2010), and the model simulated velocities increase by about 3% a^{-1}. This period of relative stability also makes 2004 a good time from which to start transient simulations.

Basal friction coefficient values downstream of the 2010 grounding line were set equal to that in the nearest 2010 grounded location. This was necessary because steps 2 and 3 involved grounding line advance beyond the region for which basal friction coefficients had been inferred. The geometry after this spin up procedure, and the friction coefficient and stiffening factor distribution from the inversion in step 1 were used as the initial condition for model calibration.

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.

Reply: In model calibration, our forcing data comes from observations and MAR regional surface mass and energy balance model (Alexander et al. 2016) driven by the ERA-Interim reanalysis (Dee et al., 2011).

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.

Reply: Agreed. We add Fig. 7c and Fig. S5 to show Jakobshavn's retreats on 2-D plane view under the forcing for best guess and up bound of mass loss projection.

3 Specific comments

p.2 l.26: "with" → "leading to a"

Reply: Done.

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

Reply: Done.

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 condi- tions? And what do you think are the consequences of the lack of winter calving?

Reply: In our calving criterion, crevasse penetration depth depends on surface water run-off which equals to zero in winter, thus no calving occurs in simulation. As discussed in section 4.4, these winter calving might be the consequences of non-hydrostatic processes related to the opening of basal crevasses. The existance of winter calving has greatly reduced the range of seasonal fluctuations in front position, which inhibited the growing of a temporary ice shelf that would buttress the grounded ice. Thus, lack of winter calving would cause underestimation of dynamic thinning as the glacier grows in winter.

p.2 Fig.1 caption: as mentioned above, using Jakobsson et al. (2012) is a rather un- expected 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.

Reply: You may notice that Jakobsson et al. (2012) provides ocean bathymetry data (sea floor elevation). Our glacier geometry data (Gogineni et al., 2012) is derived from the same institution's products as used in BedMachine V3 with data processed by the Center for Remote Sensing of Ice Sheets (CReSIS, Leuschenetal., 2010 updated 2016). So no substantial differences really exist. For computing resource considerations, we chose the earlier product because it has 500-meter-resolution.

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.

Reply: Yes, we modify the sentence to say:

Jakobshavn Isbræ (Fig. 1) is Greenland's largest and fastest outlet glacier, with transient speeds of up to 17 km a^{-1} (Joughin et al., 2014).

p.3 l.44: "possessed" → "had"

Reply: Done.

p.3 l.57: "far faster" → "much faster"

Reply: Done.

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.

Reply: Agreed. We change it to say: … it is zero at the grounding line as it begins to float.

p.4 l.74: the melange does not really belong to the ice shelf or the glacier ("its float- ing 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.

Reply: We modify the sentence to say:

Loss of buttressing from the weakening mélange or enhanced submarine melting could have triggered the dramatic changes seen in Jakobshavn Isbræ at the end of the 20th century.

p.4 l.80: There is also a new study on Jakobshavn by Bondzio et al. (2018) using a2d plan view model and a different calving parameterization, so it would be good to include these results in the comparison.

Reply: At the end of this paragraph we added:

Bondzio et al. (2018) applied a similar calving model that remove any ice where tensile stress exceeds a threshold, as simulated with a SSA (Shallow Shelf Approximation) model, regardless of ice thickness. To represent seasonal fluctuation of front positon, their stress threshold is a stepwise constant function in time with low values in summer. After calibration, their model can closely reproduce the observed behavior from 1985 to 2018 when forced only with ocean temperatures.

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

Reply: Done.

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

Reply: In BISICLES, the whole ice is in hydrostatic equilibrium (Cornford et al., 2013).

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

Reply: We modify this line to say:

An approximate stress balance equation (Schoof and Hindmarsh, 2010).

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?

Reply: Local ocean circulation in Ilulissat fjord driven by buoyancy plume brings deep water from outside to the grounding line of Jakobshavn, and renews the fjord within 90 days in summer (Gladish et al., 2015).

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

Reply: We found good correlation between ocean temperatures on the ice-ocean interface and velocities of ice front (Fig 2). We further discuss its validation in section 4.1.

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.

Reply: Agreed. Our basal drag coefficient is fixed in time.

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?

Reply: Our parameterization of mélange buttressing is similar to Nick et al. (2013), which also alters the stress balance at calving front. We have not run other simulation types, but the results could be compared with other authors, as we do in the discussion section.

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

Reply: For example, in 2004 our buttressing parameterization gives a longitudinal resistance that equals to 0.18 times of the driving force at calving front (Eq. 10).

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

Reply: We changed this symbol to '*R*'.

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?

Reply: Local ocean circulation in Ilulissat fjord driven by buoyancy plume brings deep water from outside to the grounding line of Jakobshavn, and renews the fjord within 90 days in summer (Gladish et al., 2015).

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?

Reply: The deepest water outside the sill can flow over the sill and reach the grounding line of Jakobshavn (Gladish et al., 2015). Accordingly, we chose 300 m depth which is the highest depth this CTD record can reach (Fig 1). Generally, sill depth is ~ 200 m, Jakobshavn's fjord depth is ~ 800 m.

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

Reply: "last 2 decades" now become "last decade".

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?

Reply: Our glacier geometry data (Gogineni et al., 2012) is derived from the same institution's products as used in BedMachine V3 with data processed by the Center for Remote Sensing of Ice Sheets (CReSIS, Leuschenetal., 2010 updated 2016). So no substantial differences really exist. For computing resource considerations, we chose the earlier product because it has 500-meter-resolution. You may notice that Jakobsson et al. (2012) provides ocean bathymetry data (sea floor elevation).

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).

Lines are: "The aim of this initialization was provide a state rather similar to 2004, that is barely retreating on inter-annual scales (Joughin et al., 2010) and small changes of annual mean velocity in the following 3 years. Therefore"

Reply: Agreed, the statement was imprecise. We have rewritten this in response to other criticism on the method (see above). So this is already changed in reply to the other referees. December front positions during 2004-2006 are close to stable, which are reproduced in our initialization procedure step 3. Notice observation extends no further prior to the summer of 2004.

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?

Reply: We are satisfied with our inversion results, which is verified in the new Fig S2. Our inversion followed the standard built-in procedures in BISICLES (Conford et al., 2015). Here we do not want to explore techniques of solving the inverse problem, the paper is already quite long and reasonably complex, and we think it stands on its own without this.

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?

Reply: There are no thickness data available for 2004. Our initializing procedure aims at rebuilding the geometry of 2004 from the closest dataset available (Gogineni et al., 2012).

p.12 l.201: "friction coefficient" and stiffening factor

Reply: Agreed. Done.

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 contin- uously since at least the 90's.

Reply: These questions are moot now as we rewrote section 2.3 (see above) in response to both yours and the other referee's confusion.

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?

Reply: Line 254 now becomes:

1. Total calving front retreat from 2004-2013 measured by the difference between 2004 and 2013's annual maximum extent.

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.

Reply: Agreed. December front positions during 2004-2006 are close to stable, which are reproduced in our initialization procedure step 3. Notice observation extends no further prior to the summer of 2004.

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

Reply: The 300 m depth.

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

Reply: We change the sentence to include HadGEM2-ES here.

Figure 6. Climate forcing for future projection under the RCP4.5 scenario taken as 300 m depth ocean temperatures from HadGEM2-ES (orange) compared with the ensemble mean (red) of 7 Earth System Models (HadGEM2-ES, BNU-ESM, MIROC-ESM, IPSL-CM5A-LR, CSIRO-Mk3L-1-2, NorESM1-M and MPI-ESM-LR), (right axis), with their linear trends. Annual maximum monthly surface water run-off near Jakobshavn Isbrae's terminus from RACMO (forced by outputs from HadGEM2-ES) is shown in blue.

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

Reply: Yes, but the dataset is updated to include the more recent period and is still cited as Joughin et al. (2010). https://nsidc.org/data/nsidc-0481. So we think our usage is quite standard.

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

Reply: Yes. Please see new Fig. 7 and Fig. S5.

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.

Reply: Sorry, 2029 should be 2068. We add a sentence in this paragraph:

Another SSA model (Bondzio et al., 2018) projects larger retreats than ours with a calving parameterization that predicts the location of calving depends on tensile stress regardless of ice thickness. Comparing with our physics-based formula for crevasse depth (Eq. 12), their calving criterion implies a nonlinear relationship between crevasse depth and stress, which might leads to overestimated retreats.

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.

Reply: The basal trough beneath Jakobshavn is quite deep and narrow, which makes large velocity gradients perpendicular to flow direction at the two sides of the trough.

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

Reply: Not possible because we cannot measure the ice viscosity directly. We made cross model comparison between ours and Bondzio et al., (2016).

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).

Reply: We add a sentence here:

The reason could be the discrepancy between our initial ice thickness and real geometry in 2004.

p.26 l.462: "stimulate" → "simulate"

Reply: Done.

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

Reply: We modify this sentence to say:

We project Jakobshavn Isbræ's future dynamic changes with climate forcing data from RACMO (2014-2099) and an ensemble mean of 7 Earth System Models for the RCP4.5 scenario.