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.

Referee #2

General Comments*: The model approach resembles previous studies by Muresan et al. 2016 by using ocean temperatures as a forcing to a dynamic ice flow model. However, Xiaoran Guo et al. expands the approach by going into more detail on seasonality and viscosity changes, while also starting their model in 2004 (not in 1990 as Muresan et al does) where they provide evidence that there is a shift in flow regime. Thus, there is a scientific advance within the field, by exploring ways to improve methods for modelling the behaviour of fast flowing ice streams. These types of model studies requires a lot of technical settings and tuning of the model which is very complicated and hard to explain in an easy-to-understand way. However, in order to satisfy the demand of traceability of results, this is the most important part of the paper. The model setup sections are not doing this sufficiently, in their current state.*

Specific comments*: Model description sections: Initialisation and calibration should be improved to make it clearer exactly what has been done. In particular I am missing information about what basal and surface geometry is used in the inversion process and also how values for basal friction and ice softness are derived. Furthermore, I am curious about the mesh resolution used in the model and in particular how this looks across the shear margins.*

Reply: Yes this is something the other referees mentioned as well, so we have extensively rewritten section 2.3.

First we added the mesh resolution and refer to a new Fig.S1 from line 99:

… but the fast flow area is only around 10 km in width. We use a highest mesh resolution of 500 m that covers the whole fast-flow-area including the shear margin (Fig. S1), while the rest of the glacier has 1000 m resolution.

We change the description of the initialization procedure (line 199 onwards), to answer questions on geometry and method used for inversion.

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 field is shown in Figure 3. Figure S2 shows the discrepancy between observed velocity field (Joughin et al., 2010) and the velocity derived from the inversion.

Basal geometry*: It is not stated anywhere what basal geometry is used. As, the authors also state in the discussion, geometry is the most important factor for ice stream stability and thus* *the results of the retreating calving front should furthermore be mapped on top of a basal geometry map in 2d plan view (seen from above). The retreat pattern relation to basal geometry should be discussed in relation to other studies modelling the future behaviour of Jakobshavns Isbræ.*

Reply: Our basal topography data come from (Gogineni et al., 2012). 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),

We plotted Jakobshavn's retreat in Fig. 5 in 1-d considering the convenience of comparison with previous studies (Nick et al., 2013; Muresan et al., 2016). We added a panel to Fig. 7 showing modelled front retreats along its basal trough using the best set of parameters, as requested by the referee.

In relation to starting in 2004*: To my understanding, and also what you describe for the model, a stiff ice mélange has a butressing effect. Thus, it seems strange to me that the glacier is stable from 2004 and onwards, if it just lost an important butressing?*

Reply: Yes, the annual minimum extent of Jakobshavn retreats ~ 2 km from 2004 to 2005, but then stabilizes until 2007 (Joughin et al. 2010). Front velocities increase slowly from 2004-2007 (~5.9% a^{-1}). Annual maximum extents are stable over the 2004-2007 period. We change the text as :

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). Front velocities increase slowly from 2004-2007 (~5.9% a^{-1}). Annual maximum extents are stable over the 2004-2007 period. This also makes 2004 a good time from which to start transient simulations.

Line by line comments*: Section 1 Generally, there is confusion about the definition of a floating ice shelf and a stiff ice mélange throughout the section.*

Reply: We cannot understand the confusion the referee mentions. We are using standard definitions: ice melange is the broken bergs and sea ice in front of the calving front. Ice tongue and ice shelf mean the floating glacier ice that is mechanically coupled with the inland glacier.

Line 70-72*: Needs a reference*

Reply: Done:

However, in the Jakobshavn case, both Weertman and Coulomb sliding produce very similar fluxes because the basal shear stresses along the main trough are typically only 2 % of the driving force (**Shapero et al., 2016**).

Section 2 Line 102*: what basal map do you use? Line 123-124: Please refer us to a study where the method of solving the inverse problem where two unknown is discussed (or explain in detail here how that would work, and how you can trust the outcome). I think this is an important point as viscosity is non-linear.*

Reply: The method is well established in BISICLES and is possible because it is vertically integrated shear rather than e.g. in ELMER/ice which is a full Stokes formulation. 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).

Section 2.2*: Should just be titled Forcing (and not climate forcing*

Reply: Done. We change the title to 'Forcing'.

Line 135: What is CTD?

Reply: CTD is defined in Fig. 1 caption as "**CTD (Conductivity Temperature Depth)".**

Line 136: At what depth is the ocean temperature a good approximation?

Reply: We use 300 m depths:

We use ocean temperatures at depth ~ 300 m 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.

Line 153: Use alpha1 and alpha2 instead of the calibrated numbers

Reply: Equation 9 now reads: $\alpha = \alpha_1 + \alpha_2 T.$ (9)

Line 157-160: MAR is used to estimate the runoff in equation 10. Later on Racmo is used as forcing. It is not clear why you use two different models, and when they are used.

Reply: For the period 2004-2014, SMB and surface water run-off forcing come from MAR model outputs. Because RACMO outputs cover only the period of 2006-2099. For the period 2015-2099, our SMB and run-off forcing are from RACMO outputs. We use the overlapping period 2006-2014 to correct the bias between two models outputs. This is now added to text after line 183 at the end of section 2.2.

Line 169: Make it clear that it is your model your are talking about

Reply: Here are talking about the RACMO forcing, which is not from our simulation.

Line 169: Write out SMB

Reply: SMB is already defined in Line 111: where M_s, M_b are surface mass balance (SMB)…

Line 174-177: Please state in what equation this ocean forcing goes into.

Reply: Ocean temperature forcing affects melange buttressing and sub-shelf melting (Eq. 10, 13).

Section 2.3

Line 187: The dataset described here is only 2d, your model is in 3d, so I am not sure what you are using this for?

Reply: BISICLES calculate surface elevations by Eq.1.

Line 188: to my understanding, the sudden disappearance

Reply: We prefer disintegration as it cannot have simply disappeared.

Line 202: please remind us what beta is

Reply: Agreed and rephrased (below).

Line 209-210: What does similar mean? How far off are we talking here? And please state why you use the 1998 profile when the model is starting in 2004.

Reply: New Fig. S3 shows the surface elevation profiles. We use that because the geometry in 2004 is unknown. In fact we only need the height at the grounding line, not the whole profile.

Line 214: Why is it the 8th, needs clarification.

Reply: Agreed and rephrased (below).

Line 217: Aha, good to know already in line 209-210

Reply: Agreed and rephrased (below).

Line 218-219: The glacier is definitely not in steady state in 2004, please rephrase 2.4 Model calibration. This section is very confusing to me. I think it needs a rewrite to become clearer.

Reply: Agreed. We rewrote the section 2.3:

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.

***Line 235**: rephrase sentence Line 235-245: I am confused about this whole paragraph. The following paragraph (Line 246-259) is better structured, could this perhaps be the start of the section?*

Reply: We prefer to discuss optimal parameter values and then sensitivities, so we rewrite the paragraph (Line 228-245):

The parameters in the model, α, β and γ representing mélange buttressing, crevasse depth sensitivity to surface runoff, and shelf melt sensitivity to ocean temperatures need to be estimated. The measured relationship between ocean temperatures and sub-shelf melt rate (Motyka et al., 2011) gives the value of γ to be 0.238. We tune parameters α and β manually to best reproduce Jakobshavn Isbræ's calving front position and surface velocity evolution for the 10 year period 2004-2013. Reproducing the total retreat distance and the temporary stable state after 2012 were secondary desirable features to match. The best set of parameters are α_1=0.82, α_2=0.111, β=0.0638. Since these values come from a manual search we do not claim them to be the best in all parameter space. We assess model sensitivity to the parameter values next.

Line 274-284: *This whole paragraph needs clarificatio*

Reply: This paragraph (Line 274-284) has been rewritten:

The two biggest mismatches occur with the 2007 and especially 2013 velocities (Fig. 5). 2013 has the lowest simulated surface water run-off (Fig. 2) of all the years since 2004. The Benn calving model we use is sensitive to runoff, with reduced run-off leading to lower crevasse-penetration-depth and reduced terminus fracturing thus increasing its buttressing force. Furthermore 2013 had relatively cool ocean temperatures which were lower than the average of 2004-2013. The cool ocean temperatures also increased buttressing, leading to low simulated annual mean velocities. Jakobshavn Isbræ did not in fact slow down very much in 2013 because there were calving events that are unrepresented in our model. The relevant mechanisms are discussed later.

Figure 5A: *How is the calving front retreat defined? Is it just a comparison at points along a centre flow line? And it this representative of the general retreat?*

Reply: Yes, it is defined as the distance along the center-flow-line as shown in the new Fig. S4 and Fig. 7c.

Section 3 Figure *7: I would be more interested in seeing the retreat from above, the center flowline bedmap does not explain the stop of retreat.*

Reply: Agreed. We add a new panel to Fig. 7 showing the 2-d map. 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.

Line 322: *Make it clear that you are talking about you model version of Jakobshavn Isbræ.*

Reply: In our modelled results under this forcing, Jakobshavn Isbræ continues its retreat (Fig. 7) for 18 years after 2013, producing a total grounding line retreat of ~18 km upstream.

Section 4: *It confused me a bit that you called this Discussion as a lot of important results are also presented here.*

Reply: Our modelled seasonal cycle of shear margin weakening is the main distinguishable feature comparing with previous studies. So it is convenient to put results, such as viscosity changes, here to make comparison with others.

Line 370-372*: This sentence does not make sense to me, does freshly calved ice bergs really provide any resistance?*

Reply: Agreed. We deleted this sentence.

Line: 376-377*: here you call it a stiff ice mélange, I think you should use this term throughout, especially in the intro.*

Reply: We are using standard definitions: ice mélange is the broken bergs and sea ice in front of the calving front. Ice tongue and ice shelf mean the floating glacier ice that is mechanically coupled with the inland glacier.

Section 4.2 *I am a bit confused, are the results of changes in the effective viscosity shown in figure 8 results from your forward run? And if so, how does the fact that you are keeping ice softness constant influence these results? I think there must be an effect in the softening from the thermodynamics as well?*

Reply: Yes. Fig. 8 shows results from the forward run. We plotted $\Phi\mu$ (Eq. 5) in Fig. 8. Our Ice stiffness factor Φ is fixed but ice viscosity μ varies in time. Bondzio et al., 2017 used a thermomechanical ice flow model to evolve the ice viscosity, which depends on a damage parameter that soften the ice in the shear margins. But their damage parameter also stay constant in time. Thus both ours and their models only consider the contribution from strain rate weakening in time to evolving viscosity. Thermodynamics could play some role in changing viscosity, presumably if the ice temperatures increased over time. We suppose that this would be a minor effect compared with mechanical softening, and the temperature of the ice is fixed in our model.

Section 4.3 *Good to have comparisons with previous results, I think a key point, which you focus very little on, is that the retreat stops in the same area in all the studies (if I understood this correctly)? I think that if you also add figures showing basal geometry and retreat as suggested earlier, this point is easily added.*

Reply: Yes we added the 2-d views of the past and predicted front retreats to Fig. 7c and Fig. S4, S5. In previous studies, Nick et al. (2013) and Muresan et al. (2016) didn't reproduce Jakobshavn's retreats to the bottom of its over-deepened basin, which we did (Fig. 5).

Line 498-499*: What do you mean by two-dimensional ice flow patterns?*

Reply: Here 'ice flow patterns' refers to 'ice velocity and viscosity structures'. Thus:

We successfully model two-dimensional ice velocity and viscosity structures and their seasonal variations for Jakobshavn Isbræ, which are missing from several previous modeling studies. Moreover, capturing these two-dimensional structures allows us to handle the influence of horizontal velocity shear on effective ice viscosity, which impacts on speedup processes of Jakobshavn Isbræ.