Reply to comments from October 10, 2017 by J.H. Bondzio
on N. Steiger et al., “Non-linear retreat of Jakobshavn Isbræ. . . ”

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1 General Comments

1.1 Summary

N. Steiger et al. set up a 1D flowline glacier model of Jakobshavn Isbræ (JI) and perform a sensitivity analysis on various climatic and geometric model input parameters for the glacier’s evolution from the Little Ice Age (LIA) to today and into the future (until 2100). The authors conclude that the fjord and trough geometry are the main controls on the glacier’s retreat history.

The question of what controls the retreat of JI and other marine-terminating glaciers in Greenland for the last decades is of great interest for assessments of the present and future mass balance of the Greenland ice sheet (GrIS). However, I find the present study has several shortcomings, which have to be addressed before the paper can be considered to be ready for publication. My main criticisms are: First, the 1D flowline model used for this study is inadequate to represent JI’s complex flow dynamics, especially for inferring its grounding line position and for assessments of it’s future evolution. Second, the results of the model study carry little to no novelty. Third, the manuscript would benefit of some shortening as well as a clearer structure. I will explain my criticism in more detail below.

We wish to thank the first reviewer for his comments and will address the three main concerns raised here together with the remaining detailed comments in the following.

An outline of how we will rewrite the manuscript to address the suggestions in the review is given in the following. Once we receive all reviews we will provide an updated version of the manuscript.

1.2 Model Fitness

The 1D flowline model used for this study is inadequate to represent JI’s complex flow dynamics. It is known that JI’s flow regime is controlled by intense lateral shear in the shear margins, which has to be fully represented in any model of JI which aims to study its past and future evolution (e.g. Truffer and Echelmeyer, 2003; Joughin et al., 2012; Shapero et al., 2016; Bondzio et al., 2016).

We are very much aware of the fact that JI has complex flow dynamics and that our study applies an idealized flowline model that does not capture all aspects of the dynamics.

However, the aim of our study is to use JI (which has a relatively well documented retreat history) as a test case to investigate the relative impact of fjord geometry (in particular fjord depth and width) to climate forcing, rather than tuning a model to reconstruct the history as realistically as possible. It is clear from our study and previous studies cited, but not necessarily in the scientific community at large, that fjord geometry dominates the long term retreat history of JI (over the climate forcing). The implications of this is that we might be able to predict the long term response of JI given detailed knowledge of the underlying upstream bed topography and fjord width.

Reviewing the literature, there is no clear consensus on the strength of the lateral and basal shear at JI, especially in a width-averaged setting. Some studies find that the base in the trough is very slippery, explaining the high velocities (Luthi et al., 2002; Shapero et al., 2016, Bondzio et al., 2017). However, these are observations at only one distance from the terminus (Luthi et al) and an inverse model that does not include any weakening at the lateral shear margins through crevassing and meltwater penetration (Shapero et al), which is suggested as main reason for the high velocities elsewhere (Van der Veen, 2011). Joughin et al. (2012) only find a very weak bed in the trough using a stiff model, which is in disagreement with the observations, whereas they find a less pronounced difference between the basal resistance in the trough and the margins, when using a soft model. We agree on that models that are used to project the future of JI should be more complex and include a full representation of the lateral shear. However, as we aim to study the effect of trough geometry over a long time period (in which detailed observations are missing to force a complex model) rather than projecting the future, we chose a flowline due to efficiency and a robust treatment of the grounding line and explicit representation of the calving front. In this model, lateral shear is parametrized by definition. However, we use a lateral enhancement factor of 10 to account for strong lateral resistance and will discuss the implications of the parametrization in the revised manuscript.

This 1D flowline model parametrizes the complex interaction of JI's fast-flowing trough and the surrounding inland ice, which is inadequate e.g. during rapid calving front retreat when large variations in ice viscosity and ice stream geometry occur simultaneously in a non-linear manner (cf. e.g. Bondzio et al., 2017). The inadequacy shows e.g. in the overly rapid retreat of the calving front in the model.

Deviation of the modelled retreat from observations are discussed in the paper (see Section 6.3, l.5 and l.32) as a result of the linear forcing, the simplicity of the model, and the lack of knowledge/ width-averaging of the bed topography. We do not believe that the overly rapid retreat is solely a consequence of the parametrization of ice viscosity, but rather a consequence of the width-averaging not capturing a partly grounded floating tongue (discussed in Section 6.3, l.32; Thomas et al., 2003). Note that the effective viscosity in our model is calculated in each time step with a non-linear dependency on the strain rate (see e.g. Nick et al., 2010). However, we agree that a more dynamic ice viscosity and the interplay with ice stream geometry is important, at least for models used for projections. We will discuss this limitation in more detail in the revised manuscript.

Note that the 1D flowline model does have shortcomings in its representation of ice dynamics, as is noted in our manuscript, however, it also has its advantages. In particular, it is extremely efficient, making it possible to run a very large number of ensemble experiments testing the impact of different forcing factors and choices of parameter values (as an example more than 2000 simulations (only the relevant once shown in the paper) of 250 years were completed during this study). In addition to this, the model includes an explicit, physical treatment of calving and frontal dynamics which allows for the study of transient frontal retreat given changes in the climate forcing (submarinemelt, sea ice buttressing, surface melt and crevasse water depth). In many studies, including those with 3D dynamical flow models, the frontal position through time is not explicitly resolved, and is rather fixed in time or forced to a particular position manually (e.g. Gillet-Chaulet et al., 2012; Cornford et al., 2015; Bondzio et al., 2017). The latter would not have been possible in our study as this is one of the main points of interest – namely the transient response of the glacier front given changes in climate and fjord geometry.

Moreover, from this paper as it is, the parametrization of key physical processes like the lateral influx of ice into the ice stream as well as the grounding line remain unclear. I can not evaluate the appropriateness of their treatment as of now, and can therefore not evaluate whether the grounding line motion and glacier mass balance have been represented realistically.

Thanks for pointing this out. The parametrization of the lateral influx of ice is explained in section 3.3. and we will improve the corresponding paragraph: The lateral influx Q_L is initially calculated at each grid point as the sum of the northern and southern lateral fluxes, given by observed velocities and thickness (Rignot and Mouginot, 2012; Morlighem et al., 2014) weighted with the width of the main trough. As we assume that the influx evolves with time, we scale it with the change in ice flux through the main tough.

Initial lateral flux: $$Q_{L,0}=\frac{v_{L,0}(x)\cdot H_{L,0}(x)}{W_{JI}(x)}$$

Lateral flux with time: $$Q_{L,t}=Q_{L,0}(x)\cdot\frac{v_{JI,t}(x)\cdot H_{JI,t}(x)}{v_{JI,0}(x)\cdot H_{JI,0}(x)}$$

(index JI = main trough of JI; index L = velocities and thickness of the inflowing ice)

The grounding line position is calculated for each time step with a flotation criterion based on van der Veen (1996), in which the ice is floating when the ice thickness is less than the flotation thickness. The glacier front position is calculated with the fully dynamic crevasse-depth calving criterion (Nick et al., 2010; see Section 3.2 in our paper), which calculates calving where surface and basal crevasses penetrate the whole glacier thickness. This enables simulation of transitions from a grounded front to a floating glacier tongue, and vice versa, and is linked to climate through the calculation of crevasse depths by penetration of surface melt water. The moving spatial grid adjusts freely to the new glacier length in each time step, tracking the glacier grounding line continuously based on hydrostasy (Vieli and Payne, 2005; Nick et al., 2009, 2010). This allows for a precise simulation of the glacier front and grounding line position using very high grid resolution. The grid size is set to 300 m initially, which decreases further as the glacier retreats and the length decreases. We will include a more detailed description of the grounding line dynamics in the revised manuscript.

Finally, these model shortcomings need to be mentioned in the manuscript and discussed as a limitation for the interpretation of the model results.

We will include a discussion of the limitations of the ice flow model in section 6.3 "Limitations in simulation of glacier retreat history", where we discuss the model and its difference to 3D models, and how the simplifications applied might impact the results.

1.3 Result Novelty

While the sensitivity study by itself is an interesting model exercise, the results themselves lack novelty. It is known already that the fjord and bedrock geometry control the evolution of the glacier's calving front retreat and grounding line motion (Schoof, 2007; Morlighem et al., 2016), and this paper resembles in its setup strongly the study by Enderlin et al. (2013), who use the same model.

There are indeed other studies showing the importance of bedrock geometry. However, in the community there is a strong emphasis on the role of ice-ocean interactions as a key control on the retreat of marine terminating glaciers such as JI (e.g. Holland et al., 2008; Joughin et al., 2012; Straneo and Heimbach, 2013; Cook et al., 2016; Mengel et al., 2016). In particular, it is often implied that changes in climate, ocean circulation and temperature are the main controls on glacier retreat.

This is only partly true, as once a retreat at a marine margin is triggered, the influence of bedrock geometry and fjord width dominate the transient response of the glacier, as is shown in our study as well as others (e.g. Jamieson et al. 2012; 2013; Morlighem et al. 2016).

In addition, , most studies only consider the recent retreat of glaciers such as JI and not the long term response where fjord geometry becomes more important for the transient response (e.g. Nick et al., 2013; Muresan et al., 2015; Bondzio et al., 2017). In this study we consider the period starting at the Little Ice Age which clearly is novel for JI and gives a significant increase in the data available to study the long term response of marine terminating glaciers such as JI. We would claim that only using the recent observed retreat might be misleading and is not adequate for understanding the long term retreat of marine terminating glaciers in Greenland, in particular, because fjord geometry is such an important factor on longer timescales. In essence, past changes in climate could be more important than contemporary changes in triggering the recent observed retreat given timescales involved. Similarly, the current changes in climate may trigger a delayed rapid retreat in the future of presently stable glaciers.

However, perhaps the most novel finding of our study is that an understanding of the relative impact of fjord geometry can help predict the position of past moraine positions in fjord systems such as Jakobshavn. This information will be particularly valuable in assessing the geometry and glacial history of similar fjord systems such as those in Greenland, Norway, Patagonia, and in Alaska.

To make the novelty of our study clear to the reader we will further emphasize these points in the revised manuscript, as well as discuss our study in light of the references mentioned, as well as the recent publication on JI by the reviewer (which was not available at the time of submission of this manuscript to TCD).

In my opinion, the argument of inference of moraine formation from glacier geometry is flawed. You argue that the bed geometry controls grounding line stabilization, and therefore the grounding line stabilization can be used to infer the bed geometry (i.e. moraine formation). Thus by knowing the bed we can infer the bed. This is a circular argument.

We apologize for the misunderstanding. Our argument refers both on the dependency of grounding line stabilization on bed geometry and trough width (see Figure 7), and how likely positions of moraines can be inferred from the fjord geometry. We will make this clear in the revised manuscript.

Note that due to the stabilization of the grounding line on inland up-sloping bedrock features, moraines may also be formed on top of bedrock bumps. Measurements of the bedrock in fjords like Jakobshavn are difficult to conduct, as gravity-derived measurements rely on knowledge of the density of the bed, which is dependent on the sediment thickness and the underlying bedrock geology (Boghosian et al., 2015). We that using the simulated duration of stabilization of the grounding line through time (figure 7) as a proxy for moraine build-up could help to provide information on the likely positions of moraines in the fjord.

We will make this clear in the revised manuscript.

Moreover, the inadequacy of the model for JI, which does not allow to capture stable grounding lines on retrograde slopes, large errors in model input data near the grounding line, as well as the lacking description of the grounding line treatment leave me as of now sceptical towards any quantification of grounding line stabilization using this model, cf. Point 2.54.

The model used does capture stable grounding lines on retrograde slopes, provided there is a narrowing in trough width as shown by Jamieson et al. (2012), who used a similar model. Regarding the lack of description, see above.

Finally, while it is tempting to produce projections of JI's future evolution, I believe that these projections are not reliable due to the above-mentioned model shortcomings, and similar projections produced using the same model have been presented elsewhere before (Nick et al., 2013).

We do not intend to project JI's future evolution as this has already been done – as you commented. Therefore, we wrote that it allows an "extrapolation into the future" (p. 20, l.14), which is rather done to increase the timespan and range of geometric variability, with the aim to study the non-linearity of the glacier retreat related to the geometry. We will improve section 6.4. and clarify that this is not meant as a projection on JI's future.

Note, however, that our study carries important implications for future modelling, such as the importance of including a long time span (e.g. since the Little Ice Age) to account for delayed responses to previous changes in forcing conditions. Previous studies that present projections, in contrast, only had a limited temporal range of observations to test the response of the model and to test its performance before attempting to simulate further retreat upstream.

1.4 Structure

The paper's structure should be presented more clearly. I recommend to adhere more strictly to the structure of theory, results and discussion. Some results and experiment setups are presented in the discussion for the first time, for example. For reasons of readability, I recommend to stick to the "1 paragraph, 1 message" structure, and start every paragraph with a sentence that states the paragraph's main message.

Thanks for the advise. We will improve the readability of our manuscript.

The paper is too long. The paper's main message is that geometry controls the glacier's retreat. Accordingly, only the information required to support this hypothesis should be included. Many observations listed in section 2 are not needed to support the results, and the model description in section 3 has already been given elsewhere (e.g. Enderlin et al., 2013). On the other hand, if the authors wish to give an overview over existing observations on JI, then an overview over previous model studies should also be included.

We will shorten section 2 on the background of Jakobshavn Isbræ and rather include references for the model description. We will also include more details of recent studies in the discussions (in particular the reviewer's recent paper on JI, Bondzio et al. 2017).

The naming of model variables is sometimes inconsistent. The ice velocity is denoted sometimes with U , at other places with v, for example. Please include a complete table of model variables in the paper.

Thanks. This will be included in revised manuscript.

The experiments performed in this study should be described more clearly at one location in the paper only. For example, the results of a stepwise change in climate forcing is introduced only in the discussion. I suggest inserting a complete table of experiments, including all parameters and their values, in section 4.

The stepwise change is only used as an example in the discussion and was not considered one of the main results, which is why we wanted to keep it separate. However, we will restructure this part for *better readability.*

Finally, the paper would benefit from a careful reread, as there are small grammatical errors and some misleading sentences. For more details, see the specific comments below.

Thanks for many constructive specific comments on the text, see our detailed reply below. All comments will be included in revised manuscript.

2 Specific Comments

2.1

p1, abstract. I suggest shortening the abstract by summarizing the findings more.

We will rewrite the abstract with a stronger focus on our main findings, after we received the other review(s).

2.2

The introduction carries too many details which are both widely known and not strictly needed for this study, and can therefore be dropped, e.g.:

• p1, l21 – p2, l2: The observations concerning the mass balance and flow regime of the GrIS.

Okay, we can shorten this:
Marine-terminating glaciers export ice from the interior of the Greenland Ice Sheet (GrIS) through deep troughs terminating in fjords. Dynamic discharge accounts for about half of the current GrIS mass loss (Khan et al., 2015) and is impacted by several processes linked to air and ocean temperatures...

• p2, l7: ”Turbulent melt [. . .]”. Ocean melt processes are not explicitly modelled here.

Agree, drop sentence.

2.3

p2, l14: ”Notwithstanding widespread acceleration[. . .]”. Please rephrase.

Despite widespread acceleration...

2.4

p2, l19: ”Here we therefore expand the range of climatic conditions[. . .]”. In this paper, you do not expand the range of climatic conditions, you use an expanded data set of climatic conditions reaching to the LIA in your model.

Here we therefore use an expanded data set of climatic conditions reaching from the Little Ice Age (LIA) maximum in 1850 to present-day.

2.5

p2, l24: “The glacier’s speed tripled within 20 years”. The acceleration took place after 1998, giving a time span of only 14 years until 2012.

Well, comparing the numbers in 1992 and 2012, it gives a speedup by 286% (annual average) – 420% (summer value) (see Joughin 2014).

3

2.6

p2, l32: “landward sloping”. Unclear formulation. I assume you mean landward down-sloping. In this case, the term “retrograde” is commonly used (as you do further down in the manuscript).

Thanks, will rename

2.7

p2, l34: The cited studies make the findings that are stated in this study.

Enderlin et al. (2013) explicitly studies the impact of fjord width on glacier geometry. The main findings of this study (geometry main control on retreat) are therefore not new. Moreover, Gudmundsson et al. (2012) finds that stable grounding lines on retrograde bedrocks in a deep trough are possible due to lateral stabilization. This geometric setting is exactly what defines JI, which is why 1D flowline models are inadequate for realistic modelling of JI.

As stated in comment 1.3, we are awarethat there are other studies (as e.g. Enderlin et al and Gudmundsson et al) showing the importance of bedrock geometry. However, our application on a real glacier system allows for a validation of the findings with observations and we expand on a much longer time scale than most studies do. Also, we propose important implications such as the delayed rapid retreat as a response to preceded climatic changes or the build-up of moraines at geometric pinning points. This communicates the importance of the fjord geometry to readers beyond glacier modellers and promotes closer futurecollaboration between geomorphologists and modellers.

2.8

p2,l35-p3,l2: Your result is that geometry controls retreat. In your literature overview you show that your findings are not new.

See 2.7

2.9

p3, l3-5: “Enderlin et al. (2013a) also showed that non-unique parameter combinations can exist for the same front positions, [...]” Perfect to be picked up in the discussion, as this corresponds to your findings as well.

Thanks for the suggestions. We will pick up the possibility of same front positions with different parameter combinations in the discussion.

2.10

p3, l5-6: “However, very limited knowledge exists (Lea et al., 2014; Jamieson et al., 2014) regarding the interplay between bedrock geometry, channel-width variations and external controls on a real glacier.”: This interplay has been addressed previously by several studies, e.g. Enderlin et al. (2013); Morlighem et al. (2016). Your findings corroborate their studies, which should be mentioned in the discussion.

We will include previous studies that show similar findings in our discussion section 6.1 Geometry more important than climate.

2.11

p3., l10: “non-linear frontal retreat”. It is not possible to conclude from Bauer (1968) that the retreat prior to 1960 was non-linear or gradual, as the temporal sampling of the front positions is too sparse.

We agree on that there were for sure some linear periods in-between, and we don't know much about the first period. But we are here considering the whole time period, in which the retreat was for sure non-linear (see Figure 6) and other literature mentioned in p3. L10.

2.12

p3,l11: “43.2 km” The error on calving front positions, both from Bauer (1968) and given the seasonal variability in 2015, is larger than 0.1 km. The precision of the number given here and elsewhere in the manuscript is therefore too high.

Okay, good point.

2.13

p3, l12: “The aim of the study[. . .]”. I recommend stating the study’s aims clearly at the beginning of the paragraph in a positive formulation.

Thanks, we will rewrite the sentence in the revised manuscript.

2.14

p3, l12: Model validation means checking the accuracy of the model’s representation of the real system. Therefore, if you do not aim to represent JI as closely as possible, you can not validate your model. If validation of the model is not your aim, why don’t you just perform your experiments on simplified geometries, as done earlier in Enderlin et al. (2013)?

See 2.13

2.15

p3, l14-22: This paragraph describes the content of the paper, and can be shortened in ways of: “Section 2 reviews the state of knowledge on JI’s observations used for model validation, Section 3 describes the numerical ice flow model used here, Section 4 . . . ” etc.

We will shorten the paragraph in the revised manuscript.

2.16

p3, l24: “JI is the fastest and most active glacier on the GrIS (Legarsky and Gao, 2006),[. . .]”. A better citation supporting your statement would be e.g. Rignot and Mouginot (2012).

Okay, thanks.

2.17

p4, l24: “and has been studied extensively during the last decades”. Please provide some key citations (e.g. publications by K. Echelmeyer, I. Joughin, M., Fahnestock, M. Truffer and others, as well as some modelling studies).

Yes, sure.

2.18

p3, l25-26: “We use this relatively well-observed glacier to analyse the controls of the geometry and external forcing on its rapid retreat since the LIA.” Imprecise formulation: you use a numerical model and available observations to analyze the controls on the glacier’s retreat.

Yes, thanks.

2.19

Several observations are not necessary for this study, and should be dropped.

1. p3, l28: “inland of Disko Bugt”. This is an unusual location description,

and probably not necessary here, as the paper treats the ice sheet only. *Okay.*

2. p3, l30: “producing 10% of all icebergs released from the GrIS (Weidick

and Bennike, 2007).” This observation is not used in this paper. *Okay, dropped*

3. p4, l1-5: Ice margin positions prior to the LIA are not used in this paper.

No, but the link between grounding line stabilization and moraine build-up is used. See the improved suggestion below.

4. p4, l9: “even”. I am not sure why a re-advance of 3km in 1991 is worth

mentioning given an annual front fluctuation of 2.5 km.

5. p4, l14: “Future simulations[. . .]” The future simulations are not used for discussion in this paper. *Will be included.*

6. p5, l3: “using a combination of [. . .]” The technical details for the reconstruction are not contributing to this paper.

Okay, being dropped.

7. p5, l10: “Surface melting on the GrIS has increased by 63% ”. Please use

this to motivate your climate forcing choice, otherwise I’d drop it.

We use it for the crevasse water depth, see p.11 l. 5

8. p5, l12: “due to a warming[. . .]”. This part of the sentence is not re-used in the paper.
Okay, can be dropped

9. p6,l15: “The annual average in 2012[. . .]” This value is not used later on.
Yes, it is. See Fig. 5.

10. p6, l24: “The high discharge rates observed at JI may have already been

initiated around 8 kyr BP, [. . .]”. This observation is not used in the

paper (you start in 1850). *Okay, will be dropped*

2.20

p5, l6: “the upper area”. Perhaps better: “at higher ice surface elevations”? *Okay*

2.21

p5, l15: “two warming periods that are followed by a retreat of JI’s calving front”. Worth mentioning here also is the intermittent thinning of JI during these periods, stated by Csatho et al. (2008). *Yes, we will mention it.*

2.22

p6, l4: “seasonality of calving front migration;”. At the end of this statement, several sources like Sohn et al. (1998); Joughin et al. (2004); Amundson et al. (2010) could be cited.

Thanks for providing important references.

2.23

p6, l14: “Velocities display a strong seasonal cycle and [. . .]”. This holds only for the time after the break-up, cf. Echelmeyer and Harrison (1990).

Interesting point! We will add the citation and mention that velocities display a strong seasonal cycle after the break-up.

2.24

p6, l22: “most resistance”. More precise: “most resistance to ice flow” *Okay*

2.25

p6, l28: The array of observations identifies important processes for JI, which any model that is used for modelling JI’s behavior has to capture. It would thus be useful to briefly state the most important process i.o. to motivate the model choice. Model shortcomings have to be stated clearly, and their implications for the discussion of results have to be clear.

As suggested in 2.19, we will drop some of the observations that are not used for the study. The motivations for the model choice are the efficiency that allows to include a time span from the Little Ice Age, which we think is crucial, but also the explicit physical treatment of the grounding line and the use of a calving criterion to calculate the front positions.

2.26

p7, Sect. 3.1 & 3.2: The description of the ice flow model and calving parametrization has been given extensively in Enderlin et al. (2013), and can be replaced here using a reference to that paper. Only equation 6 could remain, as a new factor (fsi), has been added.

For the revised manuscript, we will consider to remove some equations and rather refer to Nick et al. (2010), which is the main reference to our model.

2.27

p7, l18: “The grounding line position [. . .]”. Please clarify what you mean by “robustly” and explain the grounding line treatment, which is a key process for the mechanics and results described in this paper (cf. e.g. section 6.2).

We will include a more detailed description of the grounding line treatment, see our reply to comment 1.2

2.28

p8, table 1: The enhancement factor Elat is kept constant in time. Please discuss how this affects the results, as it is known that the ice viscosity in the shear margins drops significantly in response to glacier acceleration and calving front retreat (e.g. Bondzio et al., 2017).

The stated reference was not published yet at the time of our submission. We will refer to it in the revised manuscript and discuss the implications of using a constant enhancement factor.

2.29

Equations 3 & 4: Using a multiple-character symbol, e.g. cwd, for a variable is in conflict with standard notation in equations, where it is usually read as the product of three variables c, w, and d. Consider using different, one-symbol variables in the manuscript.

Okay, we will rename the variable.

2.30

p9, l7: “In the model [. . .] whole floating tongue (not shown here)”. A conclusion is missing here. Which parametrization has been applied eventually?

If only one has been used, the description of the other one can be dropped.

Furthermore, which distance is used for the distance-dependent melting rate?

We will clarify that a constant value for submarine melt is used eventually, which is justified by stating that we also used a distance-depend melting rate that gave results insignificantly different to the results with a constant value. The distance is the distance between the calving front and the grounding line.

2.31

p9, l10-15: The physical motivation behind the lateral influx is unclear. First, what are the variables “velocity” vL,0 and “depth” HL,0? How and using which criterion have they been defined? How exactly do they change in time? Sincethe lateral inflow is such an important component of the glacier’s mass balance, and thus grounding line and calving behavior, these questions have to be answered clearly in the manuscript. Only then we can gauge whether they physical motivation behind the lateral inflow parametrization is sound.

*The implementation of the lateral fluxes is inspired by Lea et al. (2014) and based on mass conservation. Due to the one dimensionality of the model, the influx at the lateral margins (Q = v*H) has to be divided by the width of the main channel at each grid point. We here assume that the lateral influx changes velocity and thickness in a similar rate as the main trough and therefore scale the original flux with the flux change of the main trough. See also our response to 1.2. We will include a better description of the parametrization of the inflow in the revised manuscript.*

2.32

p9, l13, 14 and Eq. 8: The ice velocity has been denoted by U further above (Eq.1). *Okay, we will change this.*

2.33

p9, Model setup: Please specify how you choose the centerline for your ice flow model. *It is explained on p.11, l.6 “We first calculate a centreline as a smoothed line following the mean latitudinal position of each observed glacier front”, but we can include it in the model setup.*

2.34

p9, l22: “Bathymetry data and subglacial bed topography data for JI [. . .]”. For brevity reasons, please only describe the data sets you use. This sentence should be moved to section 2, or can be dropped altogether, as it does not contribute to the matter of the paper.
Okay, we remove observations that are not used in our study.

2.35

p10, l12: “Temperature profiles[. . .]”. Again, results from other studies do not need to be explicitly repeated in great detail, since for the purpose here we are only interested in the total temperature range. Moreover, ice temperatures at the ice divide are likely colder than -20 degrees Celsius, as even the present-day average annual surface temperature there is about -25 to -30 degrees Celsius

(Ettema et al., 2009).

2.36

p10, l17: “Little change in ice temperature over the time scale[. . .]”. I believe this to be incorrect. Bondzio et al. (2017) show that the ice stream can indeed warm by several degrees Celsius during the flow acceleration following the dis-integration of the ice tongue, which is significant for ice flow especially for the warm, soft ice near the terminus.

Again, that study was published after our submittion, so we will refer to it and change our statement.

2.37

p10, l25: “linear increase”. Please elaborate exactly how the submarine melt rate increases linearly. From when to when? From which value to which value?

As we state in the beginning of the paragraph, all parameters are changed linearly from 1850 to 2015 and all values are given in table 2. However, we will clarify this.

2.38

p10, l27: “same gradual change in the SMB gradients from the LIA” is confusing to read as I they have been called “SMB profiles” in the caption of Fig. 2.

Well, the gradient is the change along the x-axis, whereas the profile is a plot of the SMB values along the x-axis.

2.39

p10, l31: “The sea ice buttressing can be assumed to be linearly dependent on the ocean and air temperatures[. . .]”. Please cite the observational study that motivates this choice. The decrease in sea-ice cover by a factor three is not obvious as of now.

As far as we are aware, there is no study on the relation of sea ice buttressing and air and ocean temperatures. We write that it may have decrease by a factor three, which is used by Nick et al. (2013), but since this is an unknown parameter, we use a very large range for the sea ice buttressing factor, (from no change in sea ice buttressing to a reduction of 33%).

2.40

p10, l33: “However, a temperature increase [. . .]”. This parameter choice multiplies the longitudinal strain rate by up to a factor of 3, which will largely increase calving (cf. Eqs. 3-6). It should be discussed as of how realistic the results using such high values are.

In this context, we refer to air and ocean temperatures in the fjord, which both have increased by approximately 1.5C in the fjord. The calving however, is only related to melt water penetrating into the crevasses, not to temperatures.

2.41

p11, Fig. 3: It is hard to read the exact values of the parameters used here in this 3D plot. Using a grid in the back planes may help. The exponent in the unit of the submarine melt rate is off. However, instead of this figure, I recommend using a table which lists all parameters and their values, and which names the experiments.

We can include all values in a table and add a grid to the figure. The exponent in the unit is due to a matlab bug and will be fixed.

2.42

p11, l1-4: The description of observations belongs into Section 2. Please include citations. Also, it is usually better to describe temperature changes in absolute numbers, as a “50%” increase in temperature is ambiguous for the various temperature units in use.

We will be more clear on the separation of observations and values used in our study. However, absolute temperatures are not used in our model, only values that are linked to temperature in a poorly known relation (crevasse-water-depth, submarine melt). Therefore, we are would like to refrain from stating absolute temperatures.

2.43

p11: The physical motivation of the parameter choices for submarine melt and crevasse water depth is poor. It would make much more sense to me if the author would simply say: “In order to perform a sensitivity analysis to different parameters, we vary parameter X from . . . to . . . ”. A sensitivity analysis should be in the center of every modeling study in order to gauge the robustness of the model results w.r.t. its input parameters (cf. also Enderlin et al., 2013).

Okay, good suggestion, we will do this in the revised manuscript.

2.44

p11, l7-11: “It is thereby tuned [. . .]” From my understanding, figure 3 does not show how the parameters have been tuned to each other in order to achieve the observed retreat. Neither does it show their interdependence, as it only shows the parameter space used in this sensitivity study. Please clarify this statement.

Figure 3 shows both the parameter space, but also that the parameters are dependent on each other, so that e.g. the needed crevasse-water-depth is determined by the values used for submarine melt rate and sea ice buttressing in order to achieve the observed retreat. We will clarify this.

2.45

p12, l1: “well-known”. This is an overstatement given the handful of calving front positions available from Bauer (1968) until the 1960s.

Okay, we will reformulate this.

2.46

p12, l1-6: Please rephrase, the message of how the forcing perturbations are constrained is unclear. Furthermore, it is unclear how you obtain the calving front positions w.r.t. your flowline. Do you mean you take them as the intersection of the flow line used in the model with the observed calving front positions?

Moreover, “latitudinal position” is an unsuitable coordinate, as JI’s flow is not straight in the lowermost $\sim$ 60 km.

As we apply a linear increase in forcing parameters, we only tune the model to fit the observation during the LIA and in present-day and let the model glacier evolve freely in-between, only forced by the linear increase in forcing parameters. The observations for the time-period in between are not used as a model input, but for comparison with the model output.

We will explain this more detailed in the revised manuscript.

Will also change latitudinal position -> cross-trough position.

2.47

p12, l22: “most reasonable”. Please clarify.

Okay: the forcing perturbations that are within a physical range.

2.48

p12, l28 to p13, l2: “The position of Camp-2 [. . .]”. It is hard to understand from this sentence how the position of Camp 2 on a surface bump leads to the overestimation of the ice thickness. Please clarify. Please explain which surface has been smoothed, ideally already in the model setup section.

We apologize for the misunderstanding. The surface has not been smoothed, but the model may overestimate surface bumps due to similar reasons as it overestimates the rapid retreat. Therefore the elevation of Camp-2 should rather be compared to an averaged height. We will rephrase this in the revised manuscript.

2.49

p13, l7: “forced by [. . .]”. Please specify the parameters used for the forcing, as well as from when to when they have been increased linearly. In order to understand the results, the reader needs exact information on all forcings used in the model.

The sentence is “forced by [...] SMB, submarine melt rate, crevasse water depth and reduction in sea ice buttressing”. These are the parameters used for the forcing. The forcing is described in Section 4.2. In the results, where we explain that we only apply linear forcings starting in 1850 to 2012, with the parameter combinations given in Figure 3.

2.50

p14, figure 5c. Please clarify, are these annual average or maximum velocities?

Please include the description of what is the grounding line flux (the circles?).

Please add what is the color-coding of the circles. Please remove one of the y-axes to the right of plot c). Please use y-axis limits so that the circles are contained within the frame. Which locations from Joughin et al. (2014) are used for the velocities? Could you show them in a map?

Okay, we will improve Figure 5c and add a more detailed description of the observational data. The positions of the velocities are given in Joughin et al. (2014)—please see figure 1 and 2 therein—and we use yearly averaged values.

2.51

p16, l8: “Our results highlight the importance [. . .]”. This is no novel result, compare e.g. Morlighem et al. (2016); Enderlin et al. (2013), who should be cited here.

Okay.

2.52

p17, l6: “a highly complex and non-linear response [. . .]”. This response is linked to the variations in glacier width and depth, and therefore scale with their respective complexity, isn’t it? I would formulate it like that.

Yes, thanks for the suggestion.

2.53

Section 6.2 lacks a clear structure. It starts out by suggesting to use the model and fjord geometry to infer moraine positions, but then turns and wants to use moraine positions to understand the non-linear response of marine ice sheet margins. Both arguments remain unfinished, so please specify how you want to achieve either of your goals.

We will rephrase this section. However, as we suggest that moraine positions are linked to grounding line stabilization, the argument can be turned in both ways:
The knowledge of fjord geometry or length of stabilization from model studies can give indications on moraine positions; conversely, big moraine systems indicate positions of grounding line stabilization.

2.54

Moreover, while the geometric constrictions determine where the calving front stabilizes, there is little information on where the grounding line stabilizes, as this depends to a large degree on e.g. the ice stream’s mass balance (i.e. lateral influx), the bedrock topography, and the treatment of the grounding line in the model (which remains unclear). Furthermore, as you state in your introduction, stable grounding lines on retrograde slopes are possible due to lateral stabilization (Gudmundsson et al., 2012). Last but not least, in order to answer whether moraines will form you need to know which process is dominant at a grounding line: substrate erosion or deposition. How can you tell that from your model? Given all the uncertainties in model input data and model parametriza-tions, please discuss how confident you can be in locations of grounding line stabilization obtained from your model, and hence moraine formation.

The finding of our study that the knowledge on fjord geometry can help predict the position of past moraine positions in fjord systems is clearly novel. Therefore we write that “this hypothesis remains to be tested with a proper model of sediment dynamics and constrained by a number of well-studied, diverse glaciological and climatic environments.” However, our model uses a detailed treatment of the grounding line (see comment 1.2) with a high resolution of less than 300 m and the ability to provide stable positions on both bedrock bumps and narrow sections, as shown in previous studies (Enderlin et al., 2013, Gudmundsson et al., 2012).
We will discuss the confidence of our hypothesis with our model and provide suggestions for further investigation in the revised manuscript.

2.55

p18, l8: “can be predicted from geometric information[. . .]”. You mentioned further above (p17, l13) that you cannot tell whether fjord width or depth is the dominant control on glacier retreat. Therefore, please specify which geometric information should be used to predict moraine formation, and how you can conclude that.

We suggest that the width and depth are both important and the hypothesis needs to be tested further.

2.56

p19, l14: “sea ice buttressing”. How can an increase in sea ice buttressing trigger retreat?

A reduction in sea ice buttressing can trigger retreat, we will change this in the revised manuscript.

2.57

p19, l21-24: The message of these sentences is not clear to me. Please rephrase.

Okay we will rephrase the explanation on an exaggerated crevasse water depth to account for the lack of submarine melt at the vertical calving front.

2.58

p19, Sect. 6.3. The discussion of which model parameter the glacier model is most sensitive to should happen in section 6.1. Instead, in section 6.3 you should discuss the limitations to the conclusions you are drawing, see comments above.

Yes thanks. We will move this part of the discussion.

2.59

p19, l27: “SMB curve is lowered by 50 % [. . .]”. Lowering the SMB curve by 50% yields -4.5, not -6 m.w.e. yr^{-1} at the terminus. Do you mean multiplying it by a factor two?

No, the combination of doubling the frontal gradient (S_a1 in Equ.7) and lowering of the whole curve (a_0 in Equ.7) by 50% gives a SMB of -6 at the front.

2.60

p20, l6: “For further studies [. . .]”. The reconstruction of the retreat has been done already by Bondzio et al. (2017), who should be cited here.

Okay, we will include the citation.

2.61

p20, Sect. 6.4 summarizes model results instead of discussing them. The results should be moved to the results section, and discussed here.

As we do not intend to project the future of JI, we did not want to include this section in the results. We will, however, reformulate this section as implications for the future and for further projective modelling studies.

3 Minor Corrections

Thanks for the correction of minor mistakes. We will correct all of them once we got the second review and revise the manuscript.

1. p2, l18: remains challenging

2. p2, l18: “constrains” feels wrong choice here. Perhaps better inhibits?

3. p2, l19: Here, we [. . .]

4. p2, l21: [. . .] provides the context for [. . .]

5. p2, l27: “largest sea level contribution”. Better: largest contribution to sea level rise.

6. p3, l5: “very limited”: A matter of taste, but the emphasis caused by “very” can be omitted here.

7. p3, l20: “Section 6,” An excess comma.

8. p3, l29: “is drained”. A matter of taste again, but using active form is more engaging for the reader.

9. p4, l13: “still ongoing retreat”. Perhaps better: “which is ongoing as of today”.

10. p5, l11: “is contemporary to”. Better perhaps: “coincides with”?

11. p7, l4: “Ice thickness changes with time are calculated [. . .]”. Please rephrase, “changes” reads as a verb, but is a noun, which is confusing to read.

12. p7, l7: “The mass balance B”. B misses a dot.

13. p7, l24: “penetrates”. Use plural here, as both surface crevasse depth and basal crevasse depth penetrate the glacier thickness.

14. p8, eq. 6: formatting: the dot over xx spreads over to the subscript characters.

15. p9, l6: “relatively” can be dropped.

16. p12, l16: “straightened” is repeated three times here, please rephrase.

17. p12, l19: “geometry”. Perhaps better: model setup?

18. p12, l26: “(Gudmundsson, 2003)”. As you are presenting your results here, the citation can be dropped in my opinion.

19. p13, l5: “surface pumps”, typo, do you mean surface bumps?

20. p13, l11: “the glacier terminus changes”. Please clarify what characteristic of the terminus changes. I assume it’s the configuration?

21. p17, l18: “In addition [. . .]” This starts a new topic, and deserves a new paragraph.

22. p19, l9: “among others those presented in Fig. 3”. Please name them here for completeness. Also,

discuss here earlier studies that found a similar result (e.g. Enderlin et al., 2013).

23. p19, l19: “order of magnitude”. This formulation is usually used with respect to magnitude, and is confusing in this sentence, as it misleads the reader to believe that the melting rate has to be multiplied by a factor 10 or so in order to achieve what a few percent in the crevasse water depth. I believe that you want to say is that the submarine melt rate has to be changed by tens of percent to achieve an effect that is reached by changing
the crevasse water depth by only a few percent, right? Please rephrase accordingly.

24. p21, l2: “Straightening”. Please add: the bed.

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