Reply to comments from January 15, 2018 by M.P. Lüthi
on N. Steiger et al., "Non-linear retreat of Jakobshavn Isbræ. . . "

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Review: Steiger et al.; tc-2017-151

Dear colleagues,
This is not a paper that is easy to judge. It presents a nice modeling study of the long-term retreat of one of Greenland's major glaciers with some nice figures. However, the results and conclusions are less convincing (except for the commonplace "geometry is important") than expected.

We would like to thank you for your input and would like to reply to your arguments in the following. In this study, we present three very important points that are crucial to be considered in any reconstruction and projection study and that we will make more clear in the revised version:

- *Geometric pinning points—defined by the trough bed and width—determine the position of grounding line stabilization during retreat and advance of the glacier, whereas climate forcing determines the timing of retreat from a stable position.*
- *A delayed rapid glacier retreat can result from temperature changes taking place decades ago due to the long internal response time and the stabilization on pinning points.*
- *We present the novel idea that narrow sections in the glacial trough can give the position of moraines, which providing a guide for the work of geomorphologists.*

To clarify the novelty of the findings we would like to point to our response to the first review:

There are indeed other studies showing the importance of bedrock geometry (e.g. Schoof 2006; Enderlin et al 2012; Jamieson et al. 2012; 2013; Morlighem et al. 2016). However, we wouldn't say that its is commonplace and most of these studies focus on synthetic glacier geometries, without a model validation using realistic changes to the external forcing and the width- depth size ratio. Also, 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 dominates 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 addition, as pointed out in the paper, past changes in climate could be more important than contemporary changes in triggering the recent observed retreat given the long timescales involved. Similarly, current changes in climate may trigger a delayed rapid retreat in the future for glaciers presumed stable.*

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 Alaska.

We have revised the paper accordingly to make these novel points clear to the reader.

In my opinion the paper could be brought in a form that is interesting for the reader if the shortcomings of the model were worked out. As detailed below, the model formulation is too simple for the task at hand, and some of the parametrizations seem to fail. Or maybe only the forcing should be more realistic. Describing what goes wrong, and why, could provide important hints of the required model physics or parametrization.
Sincerely, Martin Lüthi

Thanks for the suggestions of how to improve the paper. In the revised manuscript we now discuss the shortcomings of the model in greater detail than was done in the original Section 6.3. In addition to the limitations of a simple model we also note that the 1D flowline model has its advantages. In particular, it is extremely efficient, making it possible to run a very large number of ensemble experiments exploring the impact of different forcing factors and choices of parameter values (as an example, more than 2000 simulations each 250 model years were completed during this study, only a subset of the most relevant ones are shown in the paper). 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 (submarine melt, 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). Also, the parametrizations are physically based and link processes that are still not completely understood to air and ocean temperatures. However, as the past changes in climate are not accurately known we choose a simple linear increase to be able to better isolate the glacier responses to geometric effects.

General comments

It is not clear what the authors want to achieve in this paper. The setup and the introductory sections target Jakobshavn Isbræ. The results, however, do not match the measured evolution of this glacier, despite the arbitrary tuning of many model parameters to somehow achieve an agreement.

In the revised manuscript, we removed the background section on JI to avoid confusion on why we apply the model to JI. The aim of this study is to study the external, glaciological and geometric controls on JI in response to a linear forcing on long time scales. Rather than a simulation tuned perfectly to the observed retreat history of the past 150 years of JI, we perform a sensitivity study to investigate the importance of the fjord geometry of JI in governing the response of the glacier. However, the model parameters are not arbitrary, as the application of the flowline model to JI constrains the parameters so that the modelled velocities and frontal positions during the LIA and today correspond to observations from JI. As parameters such as crevasse water depth and sea ice *buttressing cannot easily be quantified, it is important to relate them to observed values such as the calving rate.*

The reasons for this mismatch are not investigated in the Discussion, but general observations are presented, that are not novel, and are also not clearly worked out. It is not clear by how much this study advances the topic since the many modeling papers of tidewater glaciers published during the last three decades, and notably those of F. Nick on this and other glaciers.

Given the simplicity of the model and the linear climate forcing applied it is surprising that the flow line can reproduce the highly nonlinear retreat history of JI over the past 150 years. Still, as pointed out, there is a mismatch with observations as discussed in original Section 6.3. This discussion is now expanded giving more details as requested.

Note that it is not our intension to reproduce the retreat history in detail—for this a more complex model is required (e.g. Bondzio et al., 2017), as well as an accurate history of changes in climate (which is not available back to 1850). Instead, our intentions are to investigate the source of the highly non-linear response of the glacier despite a linear climate forcing. Note also that the original studies of F. Nick and others did not include the longer retreat history in their analysis as done in this paper, instead these earlier studies were focused on the more recent as well as future retreat of JI.

Many details on Jakobshavn Isbræ are given in the text. But then this complex glacier system is modeled with a code that lacks almost all features that were discussed in the sections before. It might well be that the general behaviour of tidewater glaciers can be captured by simple models (this is even true for much simpler models than Equation (2)), but real Jakobshavn Isbræ behaves differently than almost all assumptions implicitly stated in Section 3.1.

As noted above, the manuscript is now revised and the background on JI has been removed to avoid confusion. We are aware that JI is a complex glacier, which makes it even more surprising that the disintegration of its floating tongue can be modeled (although exaggerated) by linearly changing crevasse water depth, SMB, submarine melt rate and sea ice buttressing.

The presented model contains many tuning parameters with values that seem to be chosen ad hoc. This is not bad in general, but the predictive power of such a model is severely reduce since most physics is missing (ice flow, stress transfer, basal stress coupling, calving rates etc.) and just parametrized. I'm not generally opposed using simple models, but a very good rationale should be given (which is completely lacking in the introductory sections), and the approximations and parametrizations should be clearly stated.

Most models include tuning parameters and most of the parameters used in our model are linked to air or ocean temperatures, and serve as forcing parameters with magnitudes linked to observations (see Sections 4.1 and 4.2). The parametrizations for the stress balance and calving are physically based. Note that there is still no clear consensus how to correctly implement calving, buttressing and submarine melt rate in models. We are confident that the parameter choices we made to account for the long term history of JI give a reasonably good representation of its behaviour. In the revised manuscript details of the tuning parameters are given in Section 2.

Following the arguments for the parametrizations in Section 4.1 which all seem quite arbitrary, one wonders why such a complicated model has been used. Would a simpler model with less tuning parameters also do the job?

For the purpose of studying the impact of fjord geometry, it is necessaire to include a free evolution of a floating tongue (Vieli et al., 2001), a physical calving law (Benn et al., 2007; Nick et al., 2010) and a robust treatment of the grounding line with a moving grid (Vieli and Payne, 2005). The used parameters allow the application of a climate forcing and the tuning of the model using observational data. The physically-based parametrizations and the corresponding parameters are described in Sections 2 and 3 in the revised manuscript.

Why care about ice temperature if viscosity is altered ad hoc with enhancement factors, and why care about water in crevasses if calving is somehow parametrized.

Ice temperature is not implicitly included in the model. Instead it is used to chose the right rate factor. The viscocity is not changed ad hoc, reather it is calculated via the strain rate and rate factor (Nick et al. 2010). Regarding calving, all models for marine-terminating glaciers require a parametrization of this process. The crevasse-depth criterion is one such parametrization which has been extensively tested by Nick et al., 2010. It calculates the opening of crevasses as a consequence of tensile stresses. The role of water in crevasses as a link to climate forcing is described in Section 2.2.

Section 4.2 continues arguing about parameters that are undetermined, and some ad hoc choices are made. Why care what these model parameters mean in real life? It might be worth a section in the Discussion, but most of Section 4.2 seems unnecessary and confusing. Nothing is known anyway, so why argue? Just clearly state what forcings are used, i.e. with explicit formulas that everyone can understand and repeat.

In our opinion, it is important to use reasonable values for the parameters, so that the retreat of the model glacier as far as possible reproduces the observe retreat history and velocities of JI. We have included a clear presentation of which parameters are used in Table 1 and Section 3 of the revised manuscript.

Also, I was missing the rationale for a linear forcing. While not a lot is known, at least for temperature we have some ideas of the timing, and ocean temperatures increased almost step-wise around 1997. So it is likely that a more realistic forcing would provide more realistic results.

Yes, we agree! A more realistic forcing would provide a more realistic result. But we are applying a linear forcing and a step forcing to show that rapid retreat does not need to be caused by suddenly higher temperatures, but instead can be caused solely by the geometry. When applying a more realistic forcing, it becomes difficult to argue wether the forcing or the geometry cause the non-linearity. Also—as you correctly pointed out before—the parameters are not directly linked to temperature, so that a more realistic forcing would just add more unknown complexity.

The Discussion seems to distract from the fact that the model (or the forcing) cannot be used for Jakobshavn (maybe because it is too complex), and looks at details of calving models (role of bed topography, glacier width, moraines) that have been treated in many papers.

In the revised manuscript we elaborate on the reasons for the deviation of the simulated retreat from the observations. However, as mentioned, it is not the aim of the study to reproduce 150 years of retreat history of JI, it is rather to use the long history of frontal positions to constrain the range of *parameters choices in the model. Also, although the importance of bed topography and glacier width is known in the community, many studies use short time periods to train their models (e.g. Nick et al., 2013; Muresan et al., 2015; Bondzio et al., 2017), disregarding the potential long-term glacier response to accumulated changes in climate. It is also true that many previous studies focus mostly on the ocean and atmosphere as drivers of rapid retreat of 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).*

Some newer literature (e.g. Felikson et al., 2017) should be included and discussed.

Thanks. We have also include other recent literature (such as Morlighem et al., 2016; Bondizio et al., 2017; Felikson et al., 2017) in the revised manuscript.

Specific comments

2/2 A 2012 paper seems outdated in this context. *Thanks, this is updated.*

2/4 specify: surface runoff *OK, thanks*

2/5 this is not as simple as said here. might cause crevasses to penetrate deeper. Whether this promotes calving (once the crevasses have been advected to the terminus) is also not so simple, since maybe long-during hydrofracturing actually drains crevasses, if links to the subglacial drainage system have been opened. Only if water supply starts close to the terminus, the process is very likely to enhance calving.

You are right that calving and hydrofracturing is a complex process, which is not fully understood. However, it is a key process which must be implemented in models of marine-terminating glaciers and ice sheets. Here, we adopt the the crevasse-water depth criterion (Nick et al., 2010) which is a physically based parameterization of calving. However, note that crevasse water depth is one of several parameters destabilizing the glacier front and increasing the calving rate. It is also true that it is mostly valied close to the terminus. This point has been adressed in the discussion of the revised manuscript.

2/7 also Motyka et al. (2011) *thanks*

2/13 not sure what "consistent" means here. Acceleration is not coupled to warming (there are indirect effects which can cause acceleration and/or deceleration). *Thanks, consistent -> correlated*

2/30 "Destabilization": I would not call this destabilization, since the glacier is still stable, but retreating rapidly. Maybe in a dynamical systems representation, this you could discuss this in terms of stability, but this context is missing here. So better use The rapid retreat... *Okay, thanks*

3/11 Carbonnell and Bauer (1968) have also flow velocity *The full paper was unfortunately not available to us, which is why we did not included the citation.*

3/25 or even century, as Rink, Wegener, Mercanton etc have measured its speed since 1875.

3/28 "see Fig": leave away "see". *Thanks*

3/30 a 2004 paper for discharge seems outdated in the context. The same formulation of page 6/28 should be adopted, but the same information is given twice. *We will include more recent papers*

3/31 "narrow" for 5 km wide? *Relative to the depth it is narrow, but you are right we won't call it narrow*

3/32 cite Clarke and Echelmeyer (1996) here, who actually measured the trough. Morlighem's interpolations, while important, are sometimes very much off from measurements.
Fig1 it would be helpful to show the outlines of the fast-flowing ice stream for the reader unfamiliar with Jakobshavn.
We will cite them, but we used Morlighem's data, since they are the best once that existed for the whole trough at the beginning of the construction of this study.

4/9 It seems important tio mention that Jakobshavn had a long, floating terminus (e.g. Lingle et al. (1981); Motyka et al. (2011)) which rapidly disintegrated.
This is mentioned in 4/10 in the original manuscript.
Fig2 indicate the 0 mb line, and also the ELA.
Fig2 is removed in the revised paper.

5/2 "has been reconstructed" *thanks*

5/8 Fig 2 does not show changes, but average annual (?) values. *Yes, annual data averaged over a 10 year period. Fig 2 is removed.*

5/10 This number is pretty useless here, rather say by how much the local mass balance has changed at Jakobshavn.
The value for the increase in surface runoff is used for the crevasse water depth. This is explained better in the revised Section 4.2

5/11 "coincident" *thanks*

6/7 "summer advances"? In Section 3.2, Casotto et al write about 5 km advance in winter
Sorry, that was a typo, thanks for correcting

6/8 "shorter period"? I would think this is "longer" here. *That's right, thanks*

6/19 The cited papers used the "old" Paterson values for A in the flow law and enhanced deformation for Wisconsin ice. With that there is no need to invoke basal motion to account for the high observed velocities, depending on the assumed temperature profile. Lüthi et al. (2003) (Fig. 5) and Truffer and Echelmeyer (2003) showed that changing the basal resistance has a minor influence on basal stress field.

I think there is no clear consensus on the influence of a change in basal resistance. Here, we would like to reflect on our response to treviewer 1:
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.

6/23 This was the whole point of Lüthi et al. (2003). But we and also Truffer and Echelmeyer (2003) independently obtained about 50% of driving stress, with two pretty good FE codes.

As shown in the comment above, there is no real consensus about the strength of the driving stress, but a short discussion is included in Section 5.3 of the revised manuscript.

7/14 Standard use is h or H for thickness and s for surface elevation. *Okay, this is being changed.*
Tab1 What is A? Which parametrization is assumed, traditional Patterson (1984), Cuffey and Patterson (2010), or anything else? *Yes, as stated in the table caption.*
Units for A are wrong in any case (exponent should be −3). *okay, thanks*

8/4 two- and three-letter variable names are plain confusing. Better use d cw , or similar
Okay, all parameters with several letters are renamed.

8/5 “fraction” Rightarrow “fracture”? *Thanks.*

Eq8 These are very strong assumptions that need better motivation. They certainly are not proportional to ice stream speed, but rather to the elevation gradients between stream and sides.
This is the best we can do in a one-dimensional model and it is based on mass conservation.The flux that enters the main trough from the sides has to exit through the main trough. It is reformulated in the revised version.

9/18 Is Camp-2 identical with the SUSIE Air Greenland landing site, in vicinity of which also GPS station KAGA is located. If so, please rename accordingly, also in [12/23].

Thanks for this comment. The name of the positions is changed to KAGA in the revised manuscript.

8/18 Why would one assume a steady state around 1850? In the introduction the whole history since the ice age was laid out, and I’m convinced that there was never a steady state.

No you are right, this was an unclear formulation.But it was in a transition between advance and retreat, which makes LIA a natural starting point for the simulations.

10/15 What is averaged, the temperature, or the rate factor which varies exponentially with temperature? It seems that the temperature is averaged which seems not very relevant for ice deformation studies. Also note that most deformation happens in the bottom 20% (or so) of the ice column, mainly due to high shear stress and high temperature (up to temperate).

The temperature only goes into the model indirectly via the rate factor, which is kept constant. If temperature is constant, the rate factor is constant as well. Yes, most deformation happens at the bottom, but the model is depth averaged and we are only interested in the main ice flow along the flow line.

And maybe a more important question: is the averaging of A done for the horizontal stress transfer (which is dominated by the very cold ice) or the vertical shearing (which is dominated by the bottom warm temperatures)?

We will stress that a constant rate factor is used in depth and width, both for the calculation of the viscosity and the longitudinal stress gradient.

8/25 Obviously, anything enhancing crevasses will reduce the glacier length. The statement, however, is unclear. Are all quantities changed at once? Why is the result stated before the experiment is described?

Yes, the parameters are changed linearly and at the same time. It is not a results that the parameters force the model to retreat 43km, but a forcing constraint. Only the parameter combinations that achieve this retreat are used here. This has been elaborated in Section 3.2 of the revised manuscript.

10/30 Are these three parameters independent of each other in the model description? Since I did not double-check, it would be nice if the authors would provide this information, and also show what individual changes in these parameters do to the glacier.

The parameters themselves are independent, but they all cause a glacier retreat, so that the choice of each parameter depends on the choice of the other parameters to simulate the 43 km retreat since the LIA. We have included this information in the revised manuscript (Section 3.2)

11/1 What does this mean: "temperature has doubled"? From 272 K to 544 K. Please give absolute values, temperature percentage is meaningless in this context.

Thanks, we have included absolute values. (From 1.5C to 3C)

11/5 Why care about water in crevasses? Nothing is known (except: there is no water in crevasses for 3/4 of the year), so you are free to force the model with whatever works.

You are right that nothing is known, but we know that surface runoff has increased by 63% and that calving rates have approximately doubled (Joughin 2004). The values for calving rates vary among different studies, but that's why we apply a large range.

13/1 Trim lines are usually lower than the center line height of a glacier. SUSIE/KAGA is also not on the central branch, but probably mostly affected by the (former, until 2010) North branch, which is completely ignored in the model.

Okay thanks, we didn't think about that. It may explain why our surface is larger at that position than the observed trimline.

13/7 Are all parameters varied simultaneously? It would by very helpful to just give the formulas for these changes with time.
Fig6 Very difficult to see the different colors, and match them back to Fig. 3.

Yes, they are changed simultaneously. They are only changed linearly and the rate of change is given in Table 2 of the revised manuscript. Fig 3 is removed, and the colors are consequently arbitrary.

15/1 This is a very generic property of a nonlinear system. Tidewater glaciers with over-deepened beds are good examples thereof, and this remark, re-iterated several times, is not at all novel or surprising.

As mentioned earlier, we largely relate to the trough width and find that there is no clear consensus in the community on the link of the non-linear retreat to trough geometry. Many studies try to explain rapid retreat solely based on changes in temperatures.

15/13 Now, this stability investigation would be interesting if done correctly (in the sense of dynamical system analysis).
Fig7 So we see that the glacier during its retreat rests on narrow hills, and rapidly transits through wide depressions. In my opinion no surprises here. Analyzing your system equations (1, 2), you would find exactly that.

We are not sure what you mean by dynamic system analysis. We use a novel way of quantifying grounding line stability and the figures show clearly the difference between the different geometries.

16/9 or even: stochastic or no forcing at all. This is well known, with a special variant termed by Post "the tidewater glacier cycle".

Yes, also stochastic or no forcing at all. This is included in the revised manuscript. Although the non-linear retreat of marine-terminating glaciers is known, it is still an interesting exercise to pinpoint the origin of the non-linearity. Also, the tidewater glacier cycle rather describes the lengthening and shortening of the glacier tongue, whereas we refer to stabilization of the grounding line.

21/5 A very unclear statement, since moraines are part of the (basal) geometry.

True, but we say that they build close to geometrical pinning points, and that the trough width can help finding positions of moraines. This has been revised in the new manuscript.