# Comments on N. Steiger et al., "Non-linear retreat of Jakobshavn Isbræ..." https://doi.org/10.5194/tc-2017-151

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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.

### 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). 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.

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.

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

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.

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.

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).

### 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.

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.

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.

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.

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.

# 2 Specific Comments

## 2.1

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

## 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.
- p2, l7: "Turbulent melt [. . . ]". Ocean melt processes are not explicitly modelled here.

## 2.3

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

## 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.

## 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.

## 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).

## 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.

## 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.

## 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.

## 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.

## 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.

## 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.

## 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.

## 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)?

## 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.

## 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).

## 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).

## 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.

## 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.
2. p3, l30: "producing 10% of all icebergs released from the GrIS (Weidick and Bennike, 2007)." This observation is not used in this paper.

3. p4, l1-5: Ice margin positions prior to the LIA are not used in this paper.
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.
6. p5, l3: “using a combination of [. . . ]” The technical details for the reconstruction are not contributing to this paper.
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.
8. p5, l12: “due to a warming[. . . ]”. This part of the sentence is not re-used in the paper.
9. p6,l15: “The annual average in 2012[. . . ]” This value is not used later on.
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).

## 2.20

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

## 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).

## 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.

## 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).

## 2.24

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

### 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.

### 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 ($f_{si}$), has been added.

### 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).

### 2.28

p8, table 1: The enhancement factor $E_{lat}$ 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).

### 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.

### 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?

### 2.31

p9, l10-15: The physical motivation behind the lateral influx is unclear. First, what are the variables "velocity" $v_{L,0}$ and "depth" $H_{L,0}$? How and using which criterion have they been defined? How exactly do they change in time? Since the 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.

### 2.32

p9, l13, 14 and Eq. 8: The ice velocity has been denoted by $U$ further above (Eq.1).

### 2.33

p9, Model setup: Please specify how you choose the centerline for your ice flow model.

### 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.

### 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 disintegration of the ice tongue, which is significant for ice flow especially for the warm, soft ice near the terminus.

### 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?

### 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.

### 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.

### 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.

### 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.

### 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.

### 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).

### 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.

## 2.45

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

## 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.

## 2.47

p12, l22: "most reasonable". Please clarify.

## 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.

## 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.

## 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?

## 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.

## 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.

## 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.

## 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 parametrizations, please discuss how confident you can be in locations of grounding line stabilization obtained from your model, and hence moraine formation.

## 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.

## 2.56

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

### 2.57

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

### 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.

### 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?

### 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.

### 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.

## 3 Minor Corrections

1. p2, l18: remain*s* 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 $\epsilon_{xx}$ 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.

## References


Amundson, J. M., M. Fahnestock, M. Truffer, J. Brown, M. P. Lüthi, and R. J. Motyka
2010. Ice mélange dynamics and implications for terminus stability, Jakobshavn Isbræ, Greenland. *J. Geophys. Res. - Earth Surface*, 115(F1). F01005.

Bauer, A.
1968. Missions aeriennes de reconnaissance au Groenland 1957-1958; observations aeriennes et terrestres, exploitation des photographies aeriennes, determination des vitesses des glaciers velant dans Disko Bugt et Umanak Fjord. *Medd. om Grønland*, 173(3).

Bondzio, J. H., M. Morlighem, H. Seroussi, T. Kleiner, M. Rückamp, J. Mouginot, T. Moon, E. Y. Larour, and A. Humbert
2017. The mechanisms behind Jakobshavn Isbræ's acceleration and mass loss: A 3-D thermomechanical model study. *Geophys. Res. Lett.*, 44(12):6252–6260.

Bondzio, J. H., H. Seroussi, M. Morlighem, T. Kleiner, M. Rückamp, A. Humbert, and E. Larour
2016. Modelling calving front dynamics using a level-set method: application to Jakobshavn Isbræ, West Greenland. *The Cryosphere*, 10(2):497–510.

Csatho, B., T. Schenk, C. J. Van Der Veen, and W. B. Krabill
2008. Intermittent thinning of Jakobshavn Isbrae, West Greenland, since the Little Ice Age. *J. Glaciol.*, 54(184):131–144.

Echelmeyer, K. and W. Harrison
1990. Jakobshavns Isbræ, West Greenland - Seasonal variations in velocity or lack thereof. *J. Glaciol.*, 36(122):82–88.

Enderlin, E. M., I. M. Howat, and A. Vieli
2013. High sensitivity of tidewater outlet glacier dynamics to shape. *Cryosphere*, 7(3):1007–1015.

Ettema, J., M. R. van den Broeke, E. van Meijgaard, W. J. van de Berg, J. L. Bamber, J. E. Box, and R. C. Bales
2009. Higher surface mass balance of the Greenland Ice Sheet revealed by high-resolution climate modeling. *Geophys. Res. Lett.*, 36:1–5.

Gudmundsson, G. H., J. Krug, G. Durand, L. Favier, and O. Gagliardini
2012. The stability of grounding lines on retrograde slopes. *Cryosphere*, 6(6):1497–1505.

Joughin, I., W. Abdalati, and M. Fahnestock
2004. Large fluctuations in speed on Greenland's Jakobshavn Isbrae glacier. *Nature*, 432(7017):608–610.

Joughin, I., B. E. Smith, I. M. Howat, D. Floricioiu, R. B. Alley, M. Truffer, and M. Fahnestock
2012. Seasonal to decadal scale variations in the surface velocity of Jakobshavn Isbrae, Greenland: Observation and model-based analysis. *J. Geophys. Res.*, 117:1–20.

Joughin, I., B. E. Smith, D. E. Shean, and D. Floricioiu
2014. Brief Communication: Further summer speedup of Jakobshavn Isbræ. *The Cryosphere*, 8(1):209–214.

Morlighem, M., J. Bondzio, H. Seroussi, E. Rignot, E. Larour, A. Humbert, and S.-A. Rebuffi
2016. Modeling of Store Gletscher's calving dynamics, West Greenland, in response to ocean thermal forcing. *Geophys. Res. Lett.*, 43(6):2659–2666.

Nick, F. M., A. Vieli, M. L. Andersen, I. Joughin, A. Payne, T. L. Edwards, F. Pattyn, and R. S. W. van de Wal
2013. Future sea-level rise from Greenland's main outlet glaciers in a warming climate. *Nature*, 497(7448):235–238.

Rignot, E. and J. Mouginot
2012. Ice flow in Greenland for the International Polar Year 2008-2009. *Geophys. Res. Lett.*, 39, L11501:1–7.

Schoof, C.
2007. Ice sheet grounding line dynamics: Steady states, stability, and hysteresis. *J. Geophys. Res.*, 112(F03S28):1–19.

Shapero, D. R., I. R. Joughin, K. Poinar, M. Morlighem, and F. Gillet-Chaulet
2016. Basal resistance for three of the largest Greenland outlet glaciers. *J. Geophys. Res.*, 121(1):168–180. 2015JF003643.

Sohn, H., K. Jezek, and C. van der Veen
1998. Jakobshavn Glacier, West Greenland: 30 years of spaceborne observations. *Geophys. Res. Lett.*, 25(14):2699–2702.

Truffer, M. and K. Echelmeyer
2003. Of isbræ and ice streams. *Ann. Glaciol.*, 36:66–72. International Symposium on Fast Glacier Flow, Yakutat, Alaska, Jun 10-14, 2002.