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 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. They argue to be able to infer points of grounding line stabilization – and hence moraine formation – from the trough geometry using an ice flow model.

1.2 Novelty

The study has two main threads:

The first is the "non-linear" dynamic response of the glacier, controlled by the bed topography, once the calving front retreat has been triggered. The idea that bed topography controls the calving front retreat is not new (cf. e.g. Enderlin et al., 2013; Morlighem et al., 2016), and the study does not provide substantially more information beyond this statement. The thread in its current form should therefore be dropped. It would be worthwhile to quantify and analyze the degree of non-linearity of the glacier response, but this is difficult with the irregular real-world glacier geometry used here. An idea would be to use an artificial bed topography and to quantify exactly how the bed topography translates into a front retreat rate. When using this approach, it would need to be discussed how much of the response is due to model physics and how much due to model parametrizations.

The second thread is the argument to infer stable grounding line positions by combining an ice flow model with geomorphological information, i.o. to infer potential sites for moraine formation. This thread is novel (to my knowledge).

Two comments on this: First, I'd suggest to motivate more clearly why it is important to identify these sites for readers that are not familiar with the subject. Second, I have reservations about the practicability of the method, cf. section 1.3.

1.3 Inferring Stable Grounding Line Positions Using an Ice Flow Model

The second thread of the paper assumes that it is possible to determine the exact grounding line position over time from an ice sheet model only. I question this assumption for the following reasons:

- First, there is a large spread in the grounding line position across different ice flow models – and even for different mesh resolutions within the same model – for otherwise equal model setups (cf. e.g. Pattyn et al., 2013).
- Second, observational errors in bed topography and ocean melting rate near the grounding line are large. However, ice flow models are highly sensitive to small errors in exactly these model input parameters.

Hence, the likelihood of predicting the 'correct' grounding line position using only one model is small. The likelihood decreases further with the duration of the simulation, as errors add up and are amplified by dynamic englacial non-linear processes.

1.4 Model Fitness

I have concerns that the 1D flowline model used here is not able to accurately capture the "correct" stable grounding line positions, as important physical processes for grounding line stabilization such as lateral stabilization (cf. e.g. Gudmundsson et al., 2012) are missing or parameterized at best. The study would have to show that the setup presented here is able to match the grounding lines obtained with 2D or 3D ice flow models. If the authors choose to only present the idea of the second thread here, I'd suggest to discuss which models would be better suited to capture the grounding line in future work.

My current understanding on numerical modelling of JI and other isbræ-type outlet glaciers is that lateral physical effects (stress transfer, mass influx) have to be explicitly modeled due to their high importance for the glacier dynamics and their capacity for rapid, non-linear change themselves (Truffer and Echelmeyer, 2003; Joughin et al., 2012; Shapero et al., 2016; Bondzio et al., 2017). The model results obtained from using a 1D flowline model as used here have therefore to be interpreted with care, especially since some of the model parameters used here are unphysical (cf. specific comment 2.2).

2 Specific Comments

2.1

p2, l10: "Compared to previous studies [..]": A review of modelling studies and their findings that treat the same problem (JI) is lacking. A few studies that you might want to discuss are Truffer and Echelmeyer (2003); Vieli and Nick (2011); Joughin et al. (2012); Enderlin et al. (2013); Muresan et al. (2016); Shapero et al. (2016); Bondzio et al. (2017). In particular, the main differences to Enderlin et al. (2013) have to be pointed out.

2.2

p5, Table 1: The model uses unphysical model parameters. Crevasse water depths of up to 395 m are higher than observed, and submarine melting rates of only 175 m/a are lower than observed (Motyka et al., 2011). Moreover, these two model parameters tend to influence calving in the same way (higher respective values lead to higher calving rates). Why have they thus not been chosen in the range of observed values? Please motivate your model parameter choice.

2.3

p7, Eq.5: What are $Q_{JI,0}$ and $Q_{JI,t}$? Please discuss that this scaling of the lateral ice influx allows only for small perturbations in mass flux, as geometric changes will alter the lateral influx along the ice stream over time. If, for example, the ice flow velocity of JI doubles, your lateral influx will double as well, which is contrary to what happens when you model the lateral physics explicitly: then, a thinning ice stream thins the surroundings of an ice stream, which (initially) reduces the mass flux into the ice stream. Your parametrization of lateral influx therefore potentially "overfeeds" the ice stream in comparison. The motivation and discussion of this parametrization is important, as the lateral mass influx affects the grounding line position directly.

2.4

p9, l14,15: It is not clear to me why you use the "mean latitudinal position" of each calving front? Moreover, if you mean the latitudinal coordinate of each calving front position, the please explain how you deal with the fact that the glacier trough is bent: fronts along a North-South oriented section of the trough would then receive the same "latitudinal position".

2.5

p15, l20-22: The idea presented here is not new, cf. e.g. Vieli and Nick (2011).

3 Minor Corrections

1. p2, l32: “still”: This is either a typo or it suggests that you do not agree with the hypothesis that the ocean has an influence on the glacier. Please clarify.

2. p5, table 1: The dot on ϵ_{xx} is misplaced.

3. p3, l2: “long timescale”: Please be more specific. A centennial or decadal time scale?

4. p6, Eq. 3: The equation interrupts the text flow. Section 2.2 needs to be restructured for text flow.

5. p6, Eq. 4: Due to hydrostatic equilibrium, $D = \rho_i/\rho_w H$. Hence, Eq. 4 can be simplified to the form of Eq. 6 in Enderlin et al. (2013).

6. p6, l14: The model variable SMB, a, is usually put between two commata.

7. p7, l8: “The intention”. This sentence is incomplete. The intention is to use a realistic geometry to do what exactly?

8. p7, l17: “bed topography profile”: This is a repetition of p7, l9.

9. p8, l1: Bondzio et al. (2017) showed that the study attributes the glacier’s high flow velocities to the interplay of both the slippery bed and the dynamically weakening shear margins, not just a slippery bed.

10. p8, l2: There is a question mark at the location of the citation in the text.

11. p8, l26: “outside JI”. Please clarify.

12. p9, l4: The sea-ice buttressing in the model is an enhancement factor for the calving rate (Eq. 4). High sea ice buttressing occurs for low values of f_{si} and vice versa. Therefore, I assume it is a typo when you state that high submarine melting would be necessary for low sea ice buttressing and vice versa?

13. p9, l8: “The values”: Please specify which values you mean.

14. p11, l30: The glacier’s total SMB is about 30 to 40 Gt, which is half of the modelled grounding line flux past 2015. I would therefore use a word other than “stabilizing”.

15. p15, l2-9: This introductory paragraph is hard to follow. Please rephrase.

16. p17, l18: This is a one-sentence paragraph.

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