## 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.

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

## General comments

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

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.

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.

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? 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.

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.

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.

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.

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

## Specific comments

2/2 A 2012 paper seems outdated in this context.

2/4 specify: *surface runoff*

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.

2/7 also Motyka et al. (2011)

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).

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...*

3/11 Carbonnell and Bauer (1968) have also flow velocity

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

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

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.

3/31 "narrow" for 5 km wide?

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.

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.

Fig2 indicate the 0 mb line, and also the ELA.

5/2 "has been reconstructed"

5/8 Fig 2 does not show changes, but average annual (?) values.

5/10 This number is pretty useless here, rather say by how much the local mass balance has changed at Jakobshavn.

5/11 "coincident"

6/7 "summer advances"? In Section 3.2, Casotto et al write about 5 km advance in winter

6/8 "shorter period"? I would think this is "longer" here.

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.

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.

7/14 Standard use is $h$ or $H$ for thickness and $s$ for surface elevation.

Tab1 What is $A$? Which parametrization is assumed, traditional Patterson (1984), Cuffey and Patterson (2010), or anything else? Units for $A$ are wrong in any case (exponent should be $-3$).

8/4 two- and three-letter variable names are plain confusing. Better use $d_{cw}$, or similar

8/5 "fraction" $Rightarrow$ "fracture"?

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.

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].

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.

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).

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)?

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?

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.

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.

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.

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.

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.

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.

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.

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”.

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

## References

Carbonnell, M. and Bauer, A. (1968). Exploitation des couvertures photographiques aériennes répétées du front des glaciers vêlants dans Disko Bugt et Umanak Fjord, Juin-Juillet 1964. Technical Report 3, Expédition glaciologique internationale au Groenland (EGIG). Tirage à part des Meddelelser om Grønland, Bd. 173, Nr. 5.

Clarke, T. S. and Echelmeyer, K. (1996). Seismic-reflection evidence for a deep subglacial trough beneath Jakobshavns Isbrae, West Greenland. *Journal of Glaciology*, 43(141):219–232.

Felikson, D., Bartholomaus, T. C., Catania, G. A., Korsgaard, N. J., Kjær, K. H., Morlighem, M., Noël, B., van den Broeke, M., Stearns, L. A., Shroyer, E. L., Sutherland, D. A., and Nash, J. D. (2017). Inland thinning on the greenland ice sheet controlled by outlet glacier geometry. *Nature Geoscience*, 10:366–369.

Lingle, C. S., Hughes, T. J., and Kollmeyer, R. C. (1981). Tidal flexure of Jakobshavns Glacier, West Greenland. *Journal of Geophysical Research*, 86(B5):3960–3968.

Lüthi, M. P., Funk, M., and Iken, A. (2003). Indication of active overthrust faulting along the Holocene-Wisconsin transition in the marginal zone of Jakobshavn Isbræ. *Journal of Geophysical Research*, 108(B11).

Motyka, R. J., Fahnestock, M., Truffer, M., Mortensen, J., and Rysgaard, S. (2011). Submarine melting of the 1985 Jakobshavn Isbræ floating tongue and the triggering of the current retreat. *Journal of Geophysical Research*, 116(F01007).

Truffer, M. and Echelmeyer, K. (2003). Of Isbræ and Ice Streams. *Annals of Glaciology*, 36:66–72.