

Interactive comment on “Modelling the late-Holocene and future evolution of Monacobreen, northern Spitsbergen” by J. Oerlemans

Anonymous Referee #1

Received and published: 21 March 2018

This manuscript describes a spatially-lumped model for a tidewater glacier, and then uses that model to simulate the evolution of Monacobreen in Svalbard over the Holocene and under future scenarios of climate change. The model and climate forcing are calibrated to recent observations of terminus positions of the glacier, and the impact of a recent glacier surge on glacier mass balance is also investigated. The principal novelty of the paper lies in the use of a long record of local climate during the Holocene based on lake sediment proxies of equilibrium line altitude.

Beyond the scientific content of the paper, my primary concern is that it isn't clear what the central scientific purpose of the paper is. The author has not clearly stated the

C1

scientific goal of the paper. At the end of section 1, the author poses three questions that they will answer in the course of the paper, the first of which is: “is it possible to simulate the Holocene evolution of Monacobreen?” I'm not sure that simply answering such a question in the affirmative qualifies as an sufficient goal for a scientific paper. Clearly, it is possible to make a simulation of the Holocene evolution of this glacier, many papers have used many different kinds of numerical models to simulate glaciers evolving under forcing. But simply making a simulation doesn't make this an original scientific contribution to the literature. Other questions that one might want to answer are: “Is the simulated Holocene evolution of this glacier realistic or similar to observations?” or “What controls the evolution of this glacier during the Holocene?” These questions are grappled with to a certain extent in the paper, but that goal isn't stated clearly anywhere. Since this paper is primarily focused on describing the “minimal” model to be used, they do not explore these other questions in detail. This is my main critique of the paper: I am not clear on what we have learned from the modeling exercise as described. The author could greatly improve the paper by clearly stating what new things have been learned by doing these simulations.

Major points: 1. This is a “Minimal Glacier Model” in that it is spatially-lumped. However, there are so many processes and details included (i.e. tributary flux, SMB feedbacks, calving, surging) that I am not sure it is useful to call this model minimal. In particular, the model has so many parameterizations and associated parameters (at least 15 parameters in the main model, and another 50 parameters associated with the tributary geometry), the model will produce exactly the behavior that you have prescribed in these parameterizations. Perhaps more critically, the model has many tuned parameters (just a few: E_0 , E_1 , S_0 , t_0 , λ , c), and it is calibrated based on just a few observations (1907, 1997 terminus positions). How does this validate the choice of those parameters to make projections? The paper could do with more discussion of how the parameters are tuned and whether those parameter values chosen represent a unique parameter set that reproduces the observed terminus positions.

C2

As the author says, one of the benefits of minimal models is that their dynamics and processes are transparent, but it is unclear how you use this aspect of the model to add to your analysis. Some dynamical questions I had from your analysis that weren't discussed in the paper: (a) what sets the e-folding time scale of glacier response?, (b) what sets the glacier sensitivity to forcing? (c) what minimum set of processes would capture all the dynamics necessary to predict the glacier response to forcing?

2. The abstract is more of a laundry-list of model details and results, rather than a summation of the substantive takeaways from the paper. Please shorten (i.e. by removing model details). The abstract would be a good opportunity to summarize what the scientific contribution of the paper, which it does not currently include.

3. In section 4 you indicate that the glacier is more sensitive during some periods than others. Why? This bears exploring further.

4. Section 4: you have more than just the two observations of terminus position - how do these compare to the model? Do they provide unique calibration points? What observations would you need to provide stronger calibration constraints?

5. In section 5 you show that most of the change occurs after 2100. Presumably this is due to the slow e-folding time scale of the glacier. It would help to have a sense for why the e-folding time scale of the glacier in this model is so long.

6. Page 5, Line 29: you say "it is unclear to what extent the very high ELA values since 2000 represent an expression of natural variability on the decadal time scale" - however you have a long data set of ELA values that you can use to test this question. Perhaps the most novel aspect of this study is the long time series of climate forcing you have, why not use it to answer these interesting questions?

Model points: 1. What are the assumptions under which equation (3) is valid? Are their assumptions always true during transient glacier evolution (i.e. surges) in this study?

2. Write $S(t)$ and $\bar{s}(L)$ in equation 3 to make functional dependencies clear.

C3

3. What is the physical reason that tributaries only supply mass to the main stream when they have a positive budget? Should the mass supply occur simply whenever ice flows from the tributaries to the main stream? Won't this occur even if the tributary mass balance is negative?

4. What is d in equation (9)?

5. What is κ in equation 10? Should this equation read $H_f = H_m * \max(\kappa, \delta)$? The units don't make sense as it is currently written.

6. In section 2.3 you indicate that the water depth is variable in the fjord. Should this lead us to believe that the sub-glacier topography is also variable, rather than smooth as you have assumed? How much uncertainty does lack of knowledge of sub-glacier topography introduce to the results that you present?

7. The surge function as formulated in equation 14 is an exponential function that describes a single surge, but not the periodicity of surges. The periodicity should be included in the equation itself, otherwise the reader has to figure out that t_0 is itself a regularly-spaced set of surge onsets, which is not so clear.

8. The fact that there is a single observation of a terminus position from 100 years before the only observed surge does not uniquely prove that the surge period is 100 years. As you say, there is no proof that a surge ended exactly in 1907 when the terminus position was observed. Even if it had, this could mean that the glacier has a surge period that is some whole fraction of 90 (1997 - 1907), like 3 surges of 30 year period or some period greater than 90. Also, it is possible that the surge period changes as the glacier retreats (e.g. because surface mass balance changes considerably).

More broadly: what is learned by including prescribed surging in this minimal glacier model? The surging is simply prescribed to exactly fit a single observation anyway. There are so many factors about the surge that are not known, I don't think you can conclude that the surface mass balance effect is small (especially since your equation

C4

for H_m is unlikely to hold during a surge since it assumes a quasi-steady glacier profile). And if the effect of the surge is small and the focus of this study is not surging, why include this additional complication in your minimal glacier model to begin with? If you are going to keep surging in the model, the justification needs to be stronger.

Minor details:

There are typos, spelling and grammar errors throughout this manuscript. It would be helpful to the readers and reviewers if you went through the next draft more closely to correct these.

When shortening a term to an acronym (e.g. Little Ice Age - LIA), please always include the long version at the first occurrence in the paper and the acronym in parenthesis, as not all readers may be familiar with that the acronym stands for.

Page 1, Line 36: Citation needed

Page 2, Line 6: Citation needed

Page 2: it would be useful to distinguish between studies about mountain glaciers and tidewater glaciers.

Page 2, Line 20: what issue of boundary conditions? which boundary conditions? citation needed.

Page 2, Line 36: Why does surging and tributaries make modeling a challenge? Please elaborate.

Page 3, Line 6: "Freely-evolving length L "

Page 3, Line 10: why is it clear that they make a major contribution to the total mass budget.

Generally, avoid use of "it is clear" or "of course" in places where you make statements that aren't actually obvious to everyone.

C5

Page 3, Line 27: In what sense was this previous study successful?

Page 4, Line 7: Citation needed

Page 4, Line 16: please explain why larger slopes mean weaker SMB feedback

Page 5, Lines 1-3: these sentences are not necessarily true (others have used complicated calving models to formulate general calving laws) and not necessary. Observations have shown calving flux varies linearly with depth at many tidewater glaciers. Just say this.

Page 5, Line 24: time scales of what?

Page 5, Line 26: I don't follow the logic of this argument for setting $\lambda = 15000$ m. Why not just say you tuned it to give the right modern terminus position?

Page 6, Line 12: calibrated instead of measured

Section 2.5: this is not part of the model description, therefore shouldn't be included in section 2.

Page 7, Line 13: it is not necessarily the case that overdeepenings cause hysteresis.

Page 7, Line 25: explain why this observations puts a lower limit on ELA

Figure 1: what is the red filled circle on the map?

Figure 6: why not plot the observations of glacier length here for comparison

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-10>, 2018.

C6