

The Cryosphere Discuss., referee comment RC2
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Review of "Modelling the mass budget and future evolution of Tunabreen, central Spitsbergen" (tc-2021-155)

Anonymous Referee #2

Referee comment on "Modelling the mass budget and future evolution of Tunabreen, central Spitsbergen" by Johannes Oerlemans et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-155-RC2>, 2021

Authors employ the Minimal Glacier Model (MGM) to investigate the past and future evolution of a surging glacier in Svalbard, Tunabreen. MGM is a simple, analytical model that is based on the principle of mass conservation, where the glacier length and thickness change are related to the net mass balance (mass exchange with atmosphere and ocean). Previously, it has been successfully applied by the leading author to other glaciers in Svalbard (Hansbreen, Abrahamsbreen, Monacobreen) and despite (or, in fact, thanks to...) its simplicity gave insights to the processes governing their long-term changes. After reading the manuscript I was left with a question about the novelty of the results compared to previous work and I hope that authors can clarify this in the revised manuscript. In the current implementation of the model, surging is prescribed as a change in ice thickness that can be interpreted as a change in basal conditions that causes enhanced sliding at the bed. Owing to the model structure, several assumptions and simplifications need to be made in order to tune the model to observed record of glacier length. Many of those assumptions are reasonable, nonetheless some are not that easy to justify. My main concern is the choice of a constant calving rate and flat bedrock in the ablation zone.

In my opinion, one of the important deficiencies of this study is a lack of external validation of the model, as it is tuned to entire data set of glacier length. Where there is a possibility to confront the model performance with an independent data set - runs with ELA_{LYR} as the meteorological forcing - the results are not satisfactory and authors prefer to revert to a synthetic climatic forcing based on the inverse modelling approach (which may be considered as a possible over-fitting of the model). Clearly, one of the plausible explanations of discrepancies in the simulated glacier length may be the mass imbalance caused by under- or overestimation of the frontal ablation, mainly due to the use of a constant calving rate. As shown by the authors, the model is indeed highly sensitive to the choice of calving rate (e.g. Figure 5) and therefore any error in the estimation of this variable can have a profound impact on the final results. I do agree that calving rates do not necessarily strictly follow a surge cycle - see Mansell et al. (2012) who confirm rather modest changes in calving on some surging glaciers. However, if we look at the frontal ablation of Kronebreen for example, we can observe a variability between consecutive

years reaching 50% (e.g. Kohler et al., 2016). I do agree that a robust modelling of calving rates of Tunabreen in a longer time perspective (100 years) may be beyond our capacity and it may be easier to stick to one value as authors have chosen. I would like to see a convincing explanation why did authors decide to completely disregard calving rate parametrizations based on the water depth criterion (e.g. Mercenier et al., 2018). An argument that calving depends mostly on water temperature is based on studies covering relatively short period (e.g. Luckman et al. (2015) study covered only a 1.5 year) and therefore cannot be considered reliable over longer timescales. On the other hand, authors have previously applied in MGM parametrizations of calving rate based on the water depth criterion (e.g. Oerlemans et al., 2011) and the results were convincing. I wonder why wouldn't it work for Tunabreen as well? In a comment to the Figure 6 the authors stress how important the glacier geometry is for response to climatic forcing, especially the location of a point where it switches from tidewater to land based. Yet they disregard fluctuations of the water depth along the longitudinal profile by assuming presence of a flat bed down-glacier from point L_1 .

Specific comments:

line 60, Figure 3: Maybe include the sea floor bathymetry as well

lines 73-74: This set up of x-axis assumes a stable position of the ice divide which, generally, doesn't have to be met as there are documented examples of the ice divide migration during a surge (e.g. Fridtjovbreen)

line 79: In Eq (1) there is M_m , not M . Shouldn't F be the frontal ablation?

line 94: If you use bars as notation of a mean, why not use \bar{H} as well instead of H_m ?

line 103: Is is time derivative of Eq. (2) that is being substituted to Eq. (3) or is it the opposite? For more clarity, at least some intermediate steps of this substitution should be provided. Is Eq. (4) complete?

line 110: \dot{b} is a balance rate while \bar{b} is the mean bed elevation, it is confusing to use the same letter b for two distinct variables

lines 119-122, Figure 4: there is only one longitudinal radar profile between km 12.5 and 20 (Figure 3), how was the band average of bed topography calculated over this section? The bed topography between 0 and 14 km looks like it would have been approximated much better with a parabola than a straight line.

lines 129-130: How about the undulations in the lower part of the glacier? Would they have a limited impact as well?

lines 135-136: Yet in Figure 3 one can clearly see that the bed goes below the sea level at km 13-14

lines 133-141: Please be consistent with the order of subequations a and b, once $x < L_1$ is placed first, later $L > L_1$.

line 140: Either close the bracket or remove it

lines 146-148: How do you explain the physical meaning of this decoupling? Ice should

flow from the tributaries to the main trunk regardless of their net mass balance as long as they are dynamically connected.

line 156: Can you provide some uncertainty estimation of the assumed calving rate, for example its standard deviation over 2012-2019?

line 158: $\tan^{-1}(d/3)$ is equal to ~ 1.5 for $d=40$ m. I hope this is just a typographic error in Eq. (12). Otherwise there is a problem with mass conservation in the model as F is overestimated by roughly 50% when $L > L_1$.

line 165: d is missing in the second term in the bracket.

lines 166-168: I wonder how often is the second criterion met in your simulations, especially when H_m decreases during a surge? Shouldn't calving rate increase substantially when the glacier reaches flotation?

lines 200-202: Have you considered adding some noise to this smooth function, possibly with the same variance as observations?

lines 209-210: How does this synthetic ELA record compare to ELA calculated with Van Pelt et al. (2019) model for years 1957-2020 (lines 194-196)?

lines 252-253: Calving rate in your study is constant. Second, I wonder if this regime cannot be explained with your assumed $\tan^{-1}(d/3)$ term in Eq. 13 that can be considered more as a trick to make the calving flux go down to 0 smoothly at the 0 water depth as mentioned in lines 158-159.

lines 259-260: How sensitive is the model to the choice of these parameters? Can you provide more details on the optimization procedure you applied and its results?

lines 256-257: Yet the assumption of a flat bed in the ablation zone (Eq. 8b, $b(x)=b_d$ when $x > L_1$) disregards such feedbacks during frontal recession, whereas numerous studies have shown that calving rates decrease when glacier reaches a pinning point or shallow bed

lines 265-266: Can you compare your modelled mass balance perturbations due to a surge with any observations, either from Tunabreen or some other Svalbard glacier?

Figures 7 and 8: Having same axis on both figures would make them easier to compare. Km 23 is missing in Figure 8 y-axis.

lines 295-299: Are there any other plausible explanations of this discrepancy? How about changing calving rate, or more generally, assumed simplifications in the frontal ablation calculations in the model?

lines 313-314: According to Figures 3 and 4, calving would stop further inland, at km 13-14.

Figure 10: In the Paris run, there appears to be a sufficient time lag between the surges to minimize their effect on the glacier length change, contrary to the recent two observed surges (as described in lines 274-275). How would this prognostic simulation change if there was no time for the glacier to adjust between the surges?

lines 370-371: This conclusion was reached with the same approach/method as used in this paper. How about other studies, do they confirm your findings about the small impact of surging on the long-term evolution of the glacier length?

References:

In several instances, the volume, issue or page numbering is missing. Please update the online first records where the article has been already published.

Additional references:

Köhler, A., Nuth, C., Kohler, J., Berthier, E., Weidle, C., & Schweitzer, J. (2016). A 15 year record of frontal glacier ablation rates estimated from seismic data. *Geophysical Research Letters*, 43(23), 12-155.

Mercenier, R., Lüthi, M. P., & Vieli, A. (2018). Calving relation for tidewater glaciers based on detailed stress field analysis. *The Cryosphere*, 12(2), 721-739.