Reviewer: Anonymous

NOTE: Quotations from review in normal face. Our response in *italic*

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).

Note: the model is not 'analytical' in the usual sense. The glacier system is reduced to a zero-dimensional nonlinear differential equation that is solved numerically.

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.

The fact that a certain type of model is applied many times does not reduce the 'novelty' of a study, as the purpose of our paper is not to present a new model. Other models (i.e. SIA) have been applied to many glaciers. We note that Tunabreen has never been modelled! In fact, the number of studies in which SIA or higher-order models <u>with proper calibration over the past have 100 years</u> have been applied to Svalbard's tidewater/surging glaciers is close to zero. The question we approach is that of the future of Svalbard glaciers – a very valid question in our view. As we write in the discussion "Although in the end one would like to repeat the simulations presented here with a comprehensive glacier flow model with spatial resolution, it will not be an easy task to prepare the necessary input fields and get the calving and surging at the right place and time!" So we believe our study is original and relevant.

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.

We will change the calving law to the water depth-dependent version used in earlier studies and calibrate it with the measurement.

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).

The uncertainty in the ELA record from meteorological measurements/modelling is quite large and it is not possible to decide whether the discrepancy between observed and simulated glacier length is due to a model deficiency or to poor quality of the forcing function. A good validation of the model would only be possible were there any ELA (i.e. mass balance) measurements on Tunabreen. But as we all know, this is not the case.

We do not quite understand what is meant by 'overfitting'. Our philosophy simply is that a projection of future behaviour should be based on a proper simulation of the past record. Otherwise, the initial state (imbalance) is not correct and may cause an unrealistic evolution of the glacier.

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.

As noted above, we will change the calving law to the water depth dependent version used in earlier applications of the model. At the same time we think that the impact of smaller irregularities of the bed (order of 10 m) along a profile which is supposed to represent a width-averaged geometry should not be overestimated.

Specific comments:

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

The sea floor bathymetry is relatively flat in the inner fjord, and while we can add a contour line or two, it really does not add that much to the figure.

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)

This is true. A model without spatial resolution cannot handle this. However, we believe that in the case of Tunabreen the effect of a changing ice divide on the overall mass budget is limited. But this is hard to demonstrate...

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

Indeed, M_m should not have the index. We are not quite sure what is meant by 'frontal ablation'. The calving flux is the ablation rate times the ice thickness at the front times the glacier width.

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

Yes, for consistency that would have been better. However, in earlier model descriptions (notably Oerlemans, 2011), H_m was preferred because it is easier to use as a label in graphics. We will change this.

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?

It is the opposite, which causes the confusion. The derivation can be found in Oerlemans (2011), which can directly be downloaded from the internet.

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

We do not see the problem. A bar or dot has the same function as an index.

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?

At L124, we state that the bed topography is based on the Fürst et al (2018) reconstruction as well as the 2015 helicopter radar; we will also add that non-glaciated surface topography (i.e. the mountains) is used to constrain the bed reconstruction.

The bed topography between 0 and 14 km looks like it would have been approximated much better with a parabola than a straight line.

It would lead to a much more complicated model formulations, but in the end it would have a limited impact as long as $L > 15\ km$*, because it is the mean bed profile that matters.*

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

The bed undulations in the lower part of the glacier have an amplitude of typically 10 to 20 m, which is comparable to lateral (across-fjord) variations as seen in the bathymetry in front of the glacier (see map at https://toposvalbard.npolar.no; last accessed 2 September 2021). In view of the flowband geometry adapted here, the undulations are thus seen as irregularities with only a local effect on the calving process. We also note that due to depositional and erosional processes the glacier bed may be subject to significant changes even within a time span of a hundred years.

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

L is not x. Together with the description after Eq. (8b) it is clear.

line 140: Either close the bracket or remove it

Yes

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.

In the present model, the tributary glaciers are just considered to be buckets. When they have a positive budget they spill over and supply mass to the main glacier. The model has no intention to describe the physical process of how the tributaries flow into the main stream. Although in reality a tributary glacier with a negative mass budget can for a short period still deliver still some mass, this will always be a small amount during a limited period of time. We will add a few sentences to make it clearer.

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 $L>L_1$.

The calving law can easily be changed and all the calculations can be redone.

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

Will be corrected

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?

The criterions is always fulfilled, as the reduction of the frontal thickness is not so large (max 10 m for the largest surge). It is perhaps a bit counter-intuitive, but a survey of the literature shows that there is no systematic relation between the phase of a surge and the calving rate. We will add references.

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

This makes little change since the response time is long and fluctuations are immediately integrated.

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)?

The main difference is the larger trend in Van Pelt et al. (2019) [ELA-rise of 4.6 m per year, as compared to 2.5 m per year from the reconstructed forcing]. We will mention this.

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.

With the new calving law this will be reconsidered.

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?

The control parameters have just been varied to provide the best fit between observations and simulation in a rms-sense. The behaviour with respect to the control parameters is smooth, and to present an extensive analysis really serves no point in our judgement. In fact, the degree of sensitivity is demonstrated by the experiment depicted in Figure 8.

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

'Numerous studies have shown'? Perhaps a few qualitative analyses of some extreme cases (like Columbia glacier). However, we will formulate this point more carefully...

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?

To our knowledge there are no mass balance programs on surging glaciers in Svalbard, except for Kongsvegen (but not before the last surge in 1948).

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.

KM 23 is not missing – this is on purpose (like the scale for E on the right-hand side). We tried to get the maximum of information in the figures in a transparent way – apparently not appreciated....

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?

Yes, varying calving rates is another candidate. We will discuss this in more detail. We add at the end of section 4:

The mass-balance simulation with a regional climate model by Van Pelt et al. (2019) yields a mean increase of 4.6 m a^{-1} of the ELA over the period 1957-2018. This is substantially larger than the 2.5 m a^{-1} found for the reconstructed forcing. The discrepancy is substantial and hard to explain. However, since the simulation by Van Pelt et al. (2019) does not go further back in time than 1957, a thorough comparison remains difficult.

Referring to the sensitivity of the glacier length to the calving parameter (Figure 5), it is obvious that the use of a fixed calving parameter is a limitation of the calibration procedure. It would even be possible to keep the ELA fixed and vary c. Possibly variations in c and the ELA work in parallel, because both quantities presumably increase with atmospheric temperature (assuming that water temperature in summer is related to air temperature). However, increasing c would imply a smaller increase in the ELA, and the discrepancy between the reconstructed ELA history and the simulation by Van Pelt et al. (2019) would become larger.

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

The relevant line in Figure 4 is the gray line 'Model b', where the bed goes below sea level at ≈17 km.

Figure 10: In the Paris run, there appears to be a sufficient time lag between the surges to mimimize 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?

A few calculations will be done on this. However, the number of possible ways to describe the surge frequency / amplitudes is endless....

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?

Yes, it would be so nice if other studies with different models (SIA, higher-order) would be done to investigate this point. But to our knowledge, no such study has been carried out....