Reviewer: Francisco Navarro

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

GENERAL COMMENTS:

The authors describe and discuss in the paper the future evolution of the mass budget of Tunabreen, a tidewater glacier in Svalbard that has experienced various recent surges with increasing frequency. This is done by using a so-called minimal glacier model (MGM), in which dynamic processes are parameterized. The focus of MGMs is on the exchange of mass with the environment (atmosphere and ocean). Simple models such as the MGMs have been claimed to have the advantage to allow exploring the parameter space in great detail. However, over-parameterisation of processes has also clear disadvantages, as only a minor part of the physics of the system being modelled is being represented by means of physically-based equations. Aside from this, the model presented in the paper has additional limitations (and a bias) which are either not discussed or not sufficiently discussed. My review will focus on these aspects, as well as on some shortcomings such as a certain lack of regional contextualization of the glacier under study (and its surges). The main general comments follow.

We regret that the reviewer does not have more appreciation of the approach we take. It is of course true that more comprehensive models with spatial resolution can deal with more physical processes in detail. However, we believe that higher-order models are overvalued, and that many problems involved in formulating the boundary conditions or the various tricks required to keep the numerical scheme stable are often swept under the carpet. It has NEVER been demonstrated that higher-order models do a better job in simulating glacier records that are 100 to 150 years long, compared to simple models. In our view, when the interest is in long-term evolution, it is more natural to consider a glacier as a damped mass-budget system with inputs and outputs, rather than as a pile of subtle ice mechanical processes.

1) CONTEXT: The glacier characteristics, with emphasis on those related to surging, should be put into the context of the Svalbard glaciers:

- Is it a large/medium/small tidewater glacier compared to the rest of Svalbard tidewater glacier?
- Is it surface slope within the usual range?
- Are its typical calving rates similar to those typical of other Svalbard glaciers (or higher/lower than usual)?
- How many known surging glaciers have been identified in Svalbard?
- What is the usual range of surging periods?
- Do usually glacier surges in Svalbard initiate at the front and propagate upwards?
- Has any other Svalbard glacier known to have experienced surges with increasing frequency?

Our paper is not intended to be a general paper about surging, but is instead focused on providing a model of a glacier that has undergone some surges. Here we face the usual conflict between a journal that wants a paper to be compact and concise, and a reviewer that wants to see more extensive descriptions.

However, we will add a few lines to provide more context.

2) SIMPLIFIED BED GEOMETRY: The real glacier bed geometry is approximated by a simplified geometry consisting of a flat portion below sea level and an inclined portion (with positive slope upwards) nearly all above sea-level. Several aspects should be discussed here:

– Why the authors did not consider another flat portion in the uppermost part of the glacier? (as suggested by Fig. 4)

– Making flat the submerged part of the bed will have an effect on the advance/retreat rates (for instance, fast retreat on reverse-bed slopes) and, most importantly, on the stability conditions, i.e., the modelled behaviour could differ significantly from the real glacier behaviour.
– In several occasions along the text (this will be pointed out in the specific comments) references are made in the discussion of the model results to the fact that the glacier terminus in on the deeper or in the shallower part of the submarine bed. Such comments are meaningless (as comments referred to the model results) because the simplified bed geometry has a constant water depth (ice thickness below sea level) for its submerged part.

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.

3) BIAS IN THE MODEL: A goal of the model presented in this paper is "to determine how sensitive the glacier is to ongoing and future climate warming". The interaction with the climate is established via a ELA evolution. For (recent) past climate, a ELA based on the temperature record at Longyearbyen does not provide acceptable results ("the correlation with summer temperature ... explains only 25% of the ELA variability") and therefore the authors use instead a forcing function (for the ELA as a function of time) whose parameters are calibrated against the glacier length observations ("the climate forcing is reconstructed by inverse modelling on the glacier length observations"). Later, when the (forward) model is run under this climate forcing, it is claimed that "the simulated glacier retreat is in good agreement with observations". Was something else expected, having calibrated the ELA (climate) history with the observed length fluctuations? There is a clear bias in the model, which makes the model results of limited value.

If the temperature record at Longyearbyen could not be used as a proxy for the ELA fluctuations in Tunabreen, theoretically the wisest approach (though very costly) would have been the use of a regional climate model (in hindcast mode), downscaled for Tunabreen and combined with a mass balance model to reconstruct the ELA history. I am aware, anyway, that the necessary data for the downscaling of the RCM and for the mass balance model are not available, so neither this approach is realistic. Nor the use of reanalysis data solves the problem, as such data are only available since around 1950 (and we still would have downscaling difficulties). In other words, the authors have done probably the only thing that they could do. But not because of this we have to be "permissive" with their model results but, on the contrary, be somehow skeptic and keep always in mind the model limitations implied by this bias.

Why is it so clear to the reviewer that there is a bias in the model, rather than in the representativeness of the observations, or in the meteorological model used to simulate glacier mass balance? The simulation of glacier mass balance with climate models over LONGER periods of time (>100 years) has hardly been done, because it is problematical. In a recent study concerning an alpine glacier (Tschierva Glacier, Switzerland; Oerlemans et al, 2021, Journal of Glaciology) it was shown that it is just impossible to simulate an observed glacier length record by forcing a glacier model with climate model output.

We agree that the statement "the simulated glacier retreat is in good agreement with observations" *is a bit silly. However, reconstructing the forcing from the length record is the only way to arrive at a proper initial state for integrations into the future. In our view, the sentence* "which makes the model results of limited value" *is not justified.*

4) MODEL LIMITATIONS:

The authors should discuss in the most suitable place (either the corresponding paragraph, the introduction or the discussion sections) the main limitations of the model employed. In particular:

a) Limitations inherent to MGMs: it would be good that the authors would briefly discuss the main limitations of the MGMs.

We feel the description is quite adequate and states clearly the philosophy of MGMs. But we will add a few more lines.

b) Simplified bed geometry (already discussed in general comment 2).

See text above about undulations.

c) Bias involved in the climate forcing (already discussed in general comment 3).

See above

d) Tributary basins with fixed geometry. Considering a fixed geometry (including surface geometry) for the tributary basins poses some problems. It is correct that, if the surface geometry of the tributaries is assumed to be constant, then the supply of mass from a given tributary to the main trunk will be given by the net surface mass balance on the tributary (as stated by eq. (11)). However, assuming a fixed surface geometry for the tributaries when the main trunk's geometry varies with time in not very realistic (it is equivalent to considering a "step" in the surface geometry when passing from the tributary to the main trunk). Moreover, $MM \leq 0$ is not physically possible, because, as long as the tributary has some dynamics, there will always be a supply of mass by advection from the tributary to the main trunk. In the "real world" they could become uncoupled (physically disconnected) when the main trunk terminus would retreat upglacier past the point of junction with the tributary. But this is also physically inconsistent with a constant surface geometry of the tributaries.

The tributary basins have a fixed geometry. 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 describ 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 some mass, this will always be a small amount during a limited period of time.

e) Prescribing surges prevents the model to be applied for predictions (although the conclusion that the surge occurrence does not have long-lasting effects on the glacier front position makes this not so relevant – but at least should be cited).

We do not understand this comment; for us it is the other way around…

f) A constant calving rate does not seem a suitable choice for a frequently surging glaciers, as surges imply more intense frontal crevassing and therefore more frequent calving events.

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 bed irregularities (order of 10 m) along a profile which is supposed to represent a width-averaged geometry is limited.

Many of the model assumptions leading to the above limitations could be maintained in the paper, but they should be discussed as described so the readers are fully aware of the limitations inherent to the model and its results.

SPECIFIC COMMENTS:

ABSTRACT:

- P1, L16-18: As the ELA history is reconstructed by matching observed and simulated glacier length because there was a modest correlation between Tunabreen's ELA and Longyearbyen's temperature, I would suggest altering the order of the two sentences in lines 16-18. *Yes*
- P1, L19: As this would be expected due to the bias mentioned in general comment 3, I would try to change the sentence so it does not seem a substantial finding. *Yes*
- P1, L23: becomes -> would become? *Yes*

1) INTRODUCTION:

A main modification is this section is to describe Tunabreen and its surges in the context of Svalbard glaciers and surges, as described in general comment 1.

- P2, Figure 2: Perhpas it would be worth noting somewhere that the front positions before the surges (for the first two surges around 25.7-25.8 km; for the last two surges, around 24.7-24.8 km) all are located in the bed bump that can be appreciated at 24- 26 km in Fig. 4 (this is only relevant to observations, not to the modelling results, because of the assumption of a flat submarine bed). *We think the uncertainty in the width-averaged depth is too large to put emphasis on it*
- P3, L53: "effect of reverse bed slopes" and "variable calving rates" are both cited here among the feedback mechanisms that can be dealt with by using MGMs. However, these two are not considered in the present paper. The sentence is correct, as it is general for MGMs, but could induce the reader to think that all of these feedback mechanisms will be considered in the paper. Try to think in a writing that avoids this (or perhaps just say that these two particular ones will not be considered in the present paper).

  This comment becomes less relevant because we are going to use a water-depth dependent calving law, implying that the effect of various bed slopes is in principal included.

2) GLACIER MODEL:

• P4, L78, eq. (1): perhaps a reader not fully familiar with the mass balance terminology and units could suspect an inconsistency in the dimensions of the various terms in the equation (time derivatives of volume together with [apparent] masses). This could be avoided by describing the units of the various variables involved. Also, in line 79 it would be good to refer to F as *volumetric* calving flux (to distinguish from mass flux) and stating that it is expressed as an specific value over the glacier area (i.e., volumetric calving flux divided by the glacier area). *M_s should be M. Otherwise, we state clearly that mass and volume are equivalent because ice density is taken constant. We will add 'volumetric' at a few places.*

2.1 *Prognostic equations for glacier length*: 3

- P4, L89: Some text is missing here (likely, definitions of N, L, H_m and "Deriving"). *The quantities L and W are defined already (line 71).*
- P4, L93, eq. (3): This is eq. (4.2.1) in Oerlemans (2011). Although Oerlemans (2011) is easily downloadable through the Internet, it would be convenient to add here a couple of lines justifying/explaining this parameterization. *The confusion is caused by the fact that in line 103 "of Eq. (2) into Eq. (3) " should be of Eq. (3) into Eq. (2)"*
- P4, L98: Add a reference here supporting this statement. *Yes*
- P5, L111, eq. (6): State that the variable h is surface elevation. *Yes*

2.2*Geometry*:

Important regarding this section are the aspects discussed in general comment 2.

• P6, L144: Similarly, limitations regarding the fixed surface geometry of the tributaries discussed in general comment 4d should be mentioned here. *Yes*

2.3 *Calving rate*:

- P6, L150-153: I suggest changing "the dominant control" by "a dominant control", as other controls have been shown to exert an important role (e.g. meltwater filling the crevasses – or enlarging them when flowing downwards through crevasses –, effect of ice mélange), and add to the references listed De Andrés et al. (2018, 2021), doi: 10.1017/jog.2018.61 and 10.1017/jog.2021.27, respectively. *The text will be fully changed since a different calving law will be used (water-depth dependent)*

- P6, L155-156: limitations regarding the constant calving rate discussed in general comment 4f should be mentioned here (with greater detail than that used there). *Yes*

- P6, L162: some text is missing here.

2.4 *Imposing surges*:
Limitations regarding the prescription of surges discussed in general comment 4e should be mentioned here. *Not clear to us; we clearly state what we do.*

2.5 *Climate forcing*:
The bias discussed in general comment 3 should be discussed in this section.

- P8, L202: it was decided -> we decided *Yes, we will*
- P8, L202-203: the sentence "in line with the statement in the beginning of this section" is ambiguous (I imagine that it refers to that on lines 192-193, but the reader should understand it clearly). *Indeed*

3) BASIC EXPERIMENTS ON THE SENSITIVITY OF TUNABREEN TO CLIMATE CHANGE:

- P8, L226 and P9, Figure 5: It would be convenient to comment the case ΔE=+100 m in relation with Fig. 5 and the fact that, in this case, instead of a smooth curve (as in other cases shown in the figure) there is a sharp change some earlier than 1700 yr. We will consider this. It may look somewhat different with the water-depth dependent calving law.
- P8, L232: shows -> show *Yes*
- P9, Figure 6: It would be better to restrict Regime I to the section starting in year 1500 (and ending close to year 3000); Before year 1500 it dos not make much sense. *OK*
- P10, L249-257: The comments in regimes I and II regarding water depth are meaningless under the assumption that the model is using a simplified geometry with flat bed in its submerged part (see general comment 2).

4) SIMULATING THE EVOLUTION OF TUNABREEN DURING THE PAST 100 YEARS:

- P10, L260: Some text is missing here.
- P10, L263-275: The 2002-2004 surge is the only one not discussed.
- P10, L273: I guess that "large part" should be "last one". Additionally, is there an explanation for the statement in the sentence? *Yes, will change to to "last one". There is no obvious explanation why the surge came so fast.*

5) THE FUTURE EVOLUTION OF TUNABREEN:

- P13, L313-314: "a glacier lenght of 16.2 km and no calving anymore" is stated here, but in line 136 L_1=17250 m is mentioned. *There is no conflict here because e 16.2 < 17.25*
- P13, L316: I suspect that the last m^{-1} (at the end of the line) is a typo. *Yes*
- P13, Figure 10: Two surges have been imposed, starting in 2030 and 2065. Explain how these values were selected in the light of the dating/frequencies of the previous history of (observed) surges. *Well, it is just a possibility (ambiguity); nobody knows what will happen with the future surging regime*
- P14, L341: Again, the mention that "the front comes into shallow water" is meaningless in the context of a model with simplified geometry with constant thickness of submerged part (i.e., "shallow water" – versus deeper water – does not exist in the model). *Part of the inclined linear profile is still under sea level, so here water depth is variable*

6) DISCUSSION:

The main point here would be the need to summarize the main limitations of the model (as discussed earlier) and how the model results should be taken with caution.

• P15, L359: Add reference to Oerlemans at the end of the first sentence (although mentioned earlier – line 355 –, it will make it clearer). *Yes*