The Cryosphere – Comments by referee (2nd review)

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Title: Modelling the mass budget and future evolution of Tunabreen, central Spitsbergen

Referee: Francisco Navarro

GENERAL COMMENTS:

I acknowledge the efforts by the authors to address and clarify the main points suggested by the reviewers, which have led to a substantially improved version. Particular clarifications such as the one regarding the consideration of the tributary glaciers as buckets, which, when have a positive budget spill over and supply mass to the main glacier, are specially useful. However, in my view some aspects are not yet sufficiently discussed/stressed, justified or taken into account. I will focus on these, as general comments, because the paper, after its first review and subsequent revision, virtually does not need further specific comments. My comments follow a sequential order which does not imply any relative relevance.

1) CONTEXT: In my first review I asked the authors to add some comments to put the paper into context. I suggested them to address the following questions:
   - 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?

   The answer from the authors has been "*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.*"

   I was certainly not asking for an extensive description of the above-mentioned points but just a paragraph helping the readers not so familiar with Svalbard glaciers to understand the context of this study and its relevance. This should be not difficult for the authors, given their expertise on Svalbard glaciology. In any case, I have not found in the introduction those "*few lines to provide more context*".

2) SIMPLIFIED BED GEOMETRY AND CALVING LAW: In the previous review the implications of the simplified bed geometry with a flat submerged part (lack of effect on the advance/retreat rates, e.g. fast retreat on reverse-bed slopes, and stability conditions) were pointed out. It was also pointed out that a constant calving rate does not seem a suitable choice for a frequently surging glacier, as surges imply more intense frontal crevassing and therefore more frequent calving events. The authors

have addressed this point mainly by changing to a water-depth dependent calving law of the form $F = -c\ d\ WH_f$. It is clear, however, that a water-dependent calving law is not a suitable choice (in the sense that this particular property adds nothing) for a (simplified) flat submerged bed, as this implies a constant water depth. The change from a constant calving rate to a calving law as described, involves, however, a certain improvement, because the new law reflects the changing (in time) glacier cross-sectional area, which makes the calving flux nonconstant. But, as noted, the authors should not emphasize the "water-dependent" characteristic of the calving law, which has no practical effect given the flat submerged geometry.

Regarding the possible dynamical effects of the simplification of the submerged bed geometry (to a flat bed), the authors have added (among others) in the new version of the manuscript the sentence *"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… In view of the flowband geometry adapted here, the undulations are thus seen as irregularities with only a local effect on the calving process."* However, bed undulations of that size do not only have an a local effect on the calving rates, but can also have an influence on the stabilization of the glacier front position changes. Although numerical models used to illustrate such stabilizing effect usually employ larger undulation amplitudes (e.g. 50-100 m in Vieli et al. (2001)), recent stable front positions of glaciers such as Hansbreen (another Svalbard tidewater glacier) involve much lower amplitudes, of the order of those of Tunabreen (see e.g. Figure 2 of Otero et al. (2017), to get a sense of the scale of such undulations).

3) MODEL LIMITATIONS: After reading the arguments by the authors in their answer to the comments in my first review, I concur with them in that their tuning of the ELA history with the glacier length observations is the only thing that they could do given the available data. As the authors admit that their former statement "*the simulated glacier retreat is in good agreement with observations*" is "*a bit silly*", I have also to admit that my own statement "*which makes the model results of limited value*" was too strong. My intention was to remark that the mentioned agreement is to be expected, as it has been used for the tuning. But, obtained such an agreement, the model can (of course, with limitations) be applied in prognostic mode as done later in the paper.

   I am also sorry about the misunderstanding regarding my comment "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)". I should have clarified that the first part of the sentence applied to short-term predictions of front position (in particular, to prevent surges - this is crystal-clear). But the second part of the sentence makes clear that, given that the observed surges did not have long-term effects on the glacier front position, the model could be used for prognostic purposes, as done in the paper (once again, with limitations). So I basically agree in this with the authors.

   Lastly, I should emphasize that I acknowledge the use of simple models (such as the MGM used here) to analyze the interactions between glaciers and climate, especially for long-term simulations, and to analyze the sensitivity to the model parameters. But special care has to be put into the selection of the simplifications and the analysis of their possible effects. In other words, in not trying to reach not sufficiently sustained conclusions and in making always clear the model limitations (and this was the aim of my comments regarding the model limitations).

REFERENCES:

Otero, J., Navarro, F.J., Lapazaran, J.J., Welty, E., Puczko, D. & Finkelnburg, R. (2017). Modeling the Controls on the Front Position of a Tidewater Glacier in Svalbard. Front. Earth Sci., 5, 29, doi:10.3389/feart.2017.00029.

Vieli, A., Funk, M., & Blatter, H. (2001). Flow dynamics of tidewater glaciers: A numerical modelling approach. Journal of Glaciology, 47(159), 595-606, doi:10.3189/172756501781831747.