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

Anonymous Referee #2

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Summary

The manuscript uses a simplified glacier model to estimate the evolution of Monacobreen, a calving glacier in northern Spitsbergen. The model includes surging and calving parameterizations but neglects spatially resolved ice dynamics and asserts a simplified bed topography. Mass balance and calving parameters are asserted from previous studies in Svalbard. The main stream of Monacobreen is fed by a number of side glaciers, 10 of which are modelled by assuming simplified geometries. The model response time is explored with step changes in the ELA. The model is forced with an estimated past-ELA record derived from lake sediments and the conversion

of future air temperature estimates into ELAs. The mid-Holocene to 2099 CE length evolution of Monacobreen is explored. The sensitivity of the model to calving, surging, and the presence of the largest tributaries is explored. The modelling framework is supported by a number of papers and books by the author. There are a few typos in the manuscript and it is generally well written, though there are some portions of the text where more explanation is needed.

This manuscript is novel in that it seeks to address the difficult problem of modelling large tidewater glaciers, with numerous branches, and no mass balance, ice thickness, or velocity data. But when little data is available to constrain parameters, uncertainties and the number of assumptions grow. I think the approach is interesting and useful but because of the large number of assumptions and asserted parameters (based on very limited local data) I find it hard to believe that the results actually represent the evolution of Monacobreen. In my mind this work is an exploration of the Minimal Glacier Model with the best possible parameters asserted in a remote region. It follows that some of the inferences are overstated based on the analyses performed in the manuscript. I think the overall motivation for the study needs to be improved and refined. And the model explanation needs to be much more clear about the assumptions made in the derivation of the model. The way the manuscript is written it is hard for me to tell what those assumptions were at first. I had to look a several other papers to get a sense for the assumptions. I think some of the model variables and parameters need further explanation. Where possible quantitative sensitivity analyses should be supplied to the reader instead of assertions that certain the model is not sensitive to certain choices. I would like the sensitivity and parameter analysis to be greatly expanded. The evaluation should be quantitative and relevant specifically to this study, and should also isolate single parameters. The low computational cost of the model means these analyses can be done rapidly. A table with the full list of parameters (asserted and tuned) would be vary helpful. The effect of changing the parameter on length say from a reference state would also be helpful. For example, how sensitive is the model to changes to the asserted mass balance gradient? The term is essential to the model sensitivity through the B_tot term in equation 4 but there are no measurements from the actual glacier. The discussion should be expanded to highlight the implications of the assumptions made in the modelling approach. Items that should be discussed in more detail: 1) the neglect of bed topography/variability on calving rates; 2) the sensitivity of the model to changes in the mass balance gradient (which controls the sensitivity of the glacier to changes in the ELA (and seems to be most important climate parameter)); 4) quantified sensitivity of the model to the asserted bed profile; and 5) quantified uncertainty to the estimated side basin geometries (the geometry is simplified so there must be significant uncertainty).

A couple specific items of concern:

Calving glacier length change is strongly controlled by bathymetry and bed geometry. I point to Vieli et al., 2001 (from the abstract):

"Length changes of tidewater glaciers, i.e. especially rapid changes, are dominantly controlled by the bed topography and are to a minor degree a direct reaction to a mass-balance change. Thus, accurate information on the near-terminus bed topography is required for reliable prediction of the terminus changes due to climate changes."

Vieli et al., 2001 show that small bed fluctuations on order of 100 m can pin the calving front where the bed shallows. You note that there are known fluctuations on order of 100m in the bathymetry in front of the modern calving front. It is a major assumption that the bed monotonically declines. This should be discussed in the paper and is a major caveat to the current approach. The lack of bed data makes me skeptical of the length fluctuations outside of the historical record.

I am also concerned about the Holocene temperature history that is implied by the ELA sensitivities (35 m/ K and -2.25 m /% (page 8 line 4)) and lake-derived ELA record. If we assume that the ELA change ($\sim$250 m; Figure 6) was accommodated only by temperature changes then 4 thousand years before 1900 the air temperature would have been 7.1 degrees warmer. The manuscript points to temperature as being the

primary control of the Holocene ELA decline: "Due to the declining summer isolation, at the end of the Holocene Climatic Optimum the Arctic climate cools and the equilibrium line drops." (page 8 line 16-7). If we assume that half of the ELA change is accommodated by changes in temperature then the air temperature perturbation at the start of the ELA forcing would be 3.6 degrees and precipitation would have had to have been 56% lower. This implies that precipitation would have had to decrease when air temperatures were warmer. This is opposite of the often assumed increase in precipitation with temperature as warm air holds more moisture. These climate scenarios are extreme and highly unlikely (Kaufman et al., 2004 Holocene thermal maximum in the western Arctic (0-180 deg. W). This is a concerning because the future climate scenarios are based on these low dELA/dT numbers which in turn imply outlandish Holocene air temperatures.

Detailed comments:

Abstract: I think the abstract could be revised to include a better explanation of the motivation for this study. It would also benefit from a synthesis of the model results currently presented in the abstract.

Page 2:

Line 19-21: A citation would be helpful here as I am not sure which boundary conditions you refer to.

Page 3:

Section 2 Glacier model: This model description does not present the model clearly and it does not explain what the model assumptions are. Does the model neglect ice dynamics? Does the model assume equilibrium to determine glacier length? How does the model represent its response time? Is ice thickness resolved throughout the domain? These questions should all be answered here. They are fundamental to assessing the viability of the model to this specific application. Oerlemans, 2011 is referenced but I am not sure which equations in the book are actually relevant to the form of the Minimal Glacier Model used in this manuscript. Overall, the manuscript assumes too much knowledge of previous papers published on Minimal Glacier Models.

Some of the basic conceptual framework for this manuscript is outlined in the discussion but it needs to be in the modelling section.

Line 5-11: I think it should be mentioned out right how the model treats ice dynamics and that you assume an idealized bed profile.

Page 4:

Line 10: Mass balance gradients vary considerably over short spatial scales depending on local precipitation and air temperature lapse rates

Line 21-24: How sensitive are your results to uncertainty in these geometric parameters?

Page 5:

Line 7: what is kappa?

Line 18: main stream?

Line 20: “Until today” It seems this should be revised as it is a bit confusing what you mean.

Line 22-23: The reason for this is not clear. Please explain or add a citation.

Page 6:

Line 8-9: It would be helpful to see the actual, known bathymetry along side the idealized version in figure 3.

Line 10-11: “The mean ice thickness for the present state of the glacier is about 300 m.” Is this for the actual glacier or in the model? Figure 3 shows that ice thickness is substantially smaller than 300m when the glacier surface is connected to the bed

profile used in the model. What is going on here?

Page 7:

Section 2.5 How is the response time determined?

Line 7-8: What is the justification for perturbing the ELA of basins 1-3? These are additional parameters that you are asserting without any local constraint or optimization.

Line 11: I think it is more fair to represent that it turns out the climate sensitivity of the model is large as opposed to the glacier.

Line 14: What equation is the e-folding response time based on?

Page 8:

Line 3-4: These asserted ELA sensitivities are vital to your modelling yet you do not explore the sensitivity of the model to ELA sensitivity or the mass balance gradient. van Pelt et al., 2012 should be cited for the ELA sensitivities as that is were the numbers come from originally.

Line 38: check spelling.

Page 10:

Line 27: check spelling.

Line 30: How similar are the results? It would be beneficial to the reader if you provide a quantitative evaluation here. Right now your assessment of the model uncertainty is not fully fleshed out as I would like to assess the degree to which the bed topography effects the resulting model evolution.

Line 31-32: You can quantify the uncertainty of the model parameters and tell us exactly how important the forcing and the parameters are though. As it stands now you are asserting that they have less uncertainty but not showing that it is true.

Page 11:

Line 1: Extra period.

Figures: Some figures have panels labelled as a and b others do not.

Figure 1. Which of these length constraints are used to verify the model? It is not clear from the figure.

Figure 2. It would be helpful if you delineated the side basins as you represent them with the parameters in table 1.

Figure 3. Please also include the modelled ice thickness in this figure for the same time period as the glacier surface elevation. Or clearly explanation at the start of the paper that this model does not spatially resolve ice thickness.

Figure 5. Please explain what the lines in the legend represent in the figure caption.

Figure 6. It would be helpful if you put the model constraints in the lower panel of this figure. Should be labelled as ‘yr CE’

Figure 8. This is a perfect figure to incorporate a sensitivity of the model to changes in as many parameters as possible, α, v, the mass balance gradient, and assumed surge parameters (period, magnitude) as well as to changes in the guessed bed topography.

Figure 9. The solid lines and dots need to be better explained in the caption. I am confused as to why the reference period is an important issue to explore here.

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