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

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I am very grateful to the referee for his/her thorough review, which acknowledges the originality of the approach taken to model a complex tidewater glacier system, but at the same time has significant criticism on, notably, (i) the model description (especially concerning the choice of model parameters and bed topography), (ii) the documentation on parameter sensitivity (too limited), (iii) response time and sensitivity, and (iv) the meteorological interpretation of the imposed ELA-history and future scenarios. I plan to revise the paper thoroughly on these point (apart from accommodating many small useful suggestions).

(i) Description of the Minimal Glacier Model (MGM) Although the MGM has been described in a number of publications where all the relevant equations can be found, the referees request a more extensive description with a better explanation of the basic assumptions. This will be accomodated in the revised version. Notably, a text along the following line will be included: In the MGM there is no explicit resolution in the sense that the ice thickness is calculated along a flowline. The mean ice thickness is related to glacier length and mean bed slope. It be noted that the mean bed slope changes when the glacier length changes. The relation used is based on extensive experimentation with a numerical flowline model (Oerlemans, 2001). The parameter alfa in fact is a measure of the bed resistance to the glacier flow. For glaciers on Svalbard this resistance is very low and the values of alfa are correspondingly small (as compared for instance to glaciers in the Alps). The use of the relation between length, slope and thickness implies that the height-mass balance feedback is included in the model. In fact, as has been demonstrated in Oerlemans (2011, Figure 5.8), the model even fairly accurately reproduces the hysteresis implied by an overdeepening. When the balance profile with height is linear, only the mean ice thickness enters the expression for the surface mass budget. So the fact that the ice thickness is not calculated as a spatial variable has no effect on the calculated climate-driven evolution of the glacier. This does not apply to the parameterization of calving, however. In the MGM the glacier front is always vertical and a fraction (kappa) of the mean ice thickness (or equal to the floatation thickness when this would be larger). Although this appears to be a rigorous approach, it allows a smooth transition from a land-based to calving glacier and vice versa [note: to my knowledge a full cycle of a land-based glacier into a calving glacier, and the way back, has never been simulated with an 'advanced' numerical model]. The method has been applied succesfully to Hansbreen (Oerlemans et al., 2005). Referee 2 notes that variations in the bed topography may have large effects on the calving rate and thereby on the position of the glacier front. According to the measured bathymetry in the Liefdefjorden, these variations with an amplitude of 10 - 50 m (not 100 m) are irregularly spaced and consist mainly of deposited moraines. It is unlikely that a similar bed would currently be present under Monacobreen with its very smooth surface. It is therefore not relevant to include a map of the bathymetry of the Liefdefjorden in the paper. I also don't think that the details of a bed profile matter for the glacier evolution on longer time scales, unless there are very marked jumps in bed or side geometry that could serve a pinning points. However, this does not seem to be the case.

(ii) Parameter values. To provide a better overview of the parameters used in the model a table will be included, in which references, when appropriate, will also be listed. New parameters for the bed profile will be determined by requiring consistency between ice thickness and observed surface profile, for the value of a taken from Kronebreen (which is based on observations; Hagen and Saetrang, 1991). Referee 2 mentions specifically that the effect of the balance gradient could be important and should be studied / presented. It has been demonstrated in many studies (with MGMs as well as numerical glacier models) that the effect of the balance gradient on the equilibrium length is small. At the same time the effect on the response time is significant. I will explain this in more detail in the revised version. The more extensive parameter sensitivity study requested by Referee 2 will imply that one more figure will be added to the paper. I suppose that is no problem.

(iii) Response time. The response time is an e-folding response time. It is calculated from the response curve in a straightforward way.This will be explained better in the revised paper. The large value (350 years) is mainly due to the very small slope of the glacier and the associated strong height-mass balance feedback, which is known to lead to larger response times (e.g. Oerlemans, 2001).

(iv) Climate forcing. It is clear that here a more extensive discussion is needed. Referee 2 calculates that, in order to explain the high ELA values 4000 YBP, a 7 C higher temperature would have prevailed. He/she refers to Kaufmal et al. (2004), who suggest that the Holocene thermal maximum would have been +1.5 K. Several remarks can be made concerning this apparent discrepancy. Later paleoclimatic studies have show the Kaufman et al. estimate is really too conservative (refs will be included). With respect to the apparent very high value of the ELA in the mid-Holocene: summer insolation was typically 30 W/mˆ2 higher than today, which no doubt had a strong effect on the summer melt rates and thereby on the ELA (apart from the temperature effect). So it is not necessary to attribute the entire rise in ELA from a higher temperature alone. In any case, it is the ELA variations that drive the glacier evolution, and the ELA reconstruction used seems to be very robust. With regard to the future projections: I think that the use of two different reference periods is useful and points to an aspect that is often not getting enough attention: the strong dependence of projections on uncertainty in the initial state. I am not quite sure why Referee 2 finds this unclear, but I will try to come up with a better explanation.

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