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The Cryosphere
Discussions

Interactive comment on "Application of a minimal glacier model to Hansbreen, Spitsbergen" *by* J. Oerlemans et al.

J. Oerlemans et al.

j.oerlemans@uu.nl

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GENERAL

First of all we acknowledge the appreciation of the reviewers for the use of simple models to study glacier systems. We concur with the view that simple and comprehensive models play a complementary role in increasing our understanding of the response of glaciers to climate change. The reviewers stress that the response of tidewater glaciers is a large uncertain factor in the global assessment of the effect of global warming on sea-level change. We fully agree with this and we think that minimal models can help to study a relatively large number of glaciers with a limited amount of input data. We are aso happy to see that the reviewers like our style and basically agree with the level of detail with which are method is described and result are discussed. Overall,

the minor revisions requested by the reviewers can easily be incorporated in a revised manuscript and we intend to do so.

RESPONSE TO MORE SPECIFIC COMMENTS

BJÖRNSSON

(1) The reference to Pohjola et al. (2002) will be added.

(2) For clarity, we propose to use 'Svalbard' throughout the paper.

VIELI

(1) Future glacier behaviour and ELA. It is clear that a smooth linear increase in the ELA for the future is unlikely to occur, because there is a lot of variability in the weather/climate on annual and decadal time scales, especially at high latitudes. The linear increase is taken because it is the simplest and most straightforward way to impose a scenario of warming. We agree that our conclusion about the future of Hansbreen (its disappearance) does not depend critically on the choice of the future E(t), because already at the present time the ELA is above the critical value needed for a more or less stable state of the glacier.

(2) Pre-Little Ice Age bed topography. The reason that we used a slightly lower moraine shoal as a possible pre-LIA situation is simply that we assumed that the glacier must have dumped material on the shoal during the little ice age for at least some time. Note that the difference is only about 30 m. We agree that this is speculative. On the other hand, as stated by the reviewer, the assumption of a slightly lower moraine shoal is not critical to the result (the magnitude of the overdeepening is, however !). We will mention the motivation for the lower moraine shoal more explicitly in the manuscript.

(3) Global dynamics. Here the word 'global' is not meant to imply that we think we do something fantastic and should therefore not 'irritate' the reviewer. The idea is more to refer to the qualitative, macroscopic poperties. In the theory of dynamical system / topology the word 'global' is used in this sense. But we are happy to replace 'global' by

'overall'.

(4) Mean mass balance, p 952. Surface mass balance data have been obtained from classic stake readings, done at the end of accumulation and ablation seasons (11 stakes along the centreline). A Digital Terrain Model of the glacier was updated every 5 years by precise kinematic GPS profiling. High frequency GPR profiling and snow pits studies have been carried out to map the distribution of accumulation.

(5) Equation (3). 's' should indeed be 's bar ' (we admire the sharpness of the reviewer!)

(6) Model parameters. The model parameters were taken in such a way that the mean ice thickness as well as the ice thickness at the glacier front were reproduced by the model. The balance gradient was taken slightly smaller, namely 0.006, than in Vieli et al. (0.0066), because new measurements are available, and also because the value used by Vieli is somewhat too large to represent the upper part of the glacier where the balance gradient is smaller.

(7) Matching the measured calving flux. The calving flux was calculated from data on the mean annual glacier velocity measured at mass balance stakes by precise static GPS surveys, and then extrapolated toward the terminus. Two stakes located close to the ice-cliff (c. 300 m upstream from the terminus) were surveyed by precise GPS in period of 2 September 2008 – 14 September 2009 to obtain a typical value for the acceleration of the flow toward the glacier front. The transverse velocity profile on the ice cliff was obtained by repeated terrestrial laser scanning by Riegl LPM-321 on 29 August and 9 September 2009. The same survey, together with an examination of terrestrial photogrammetric images, was used to determine the elevation of the calving cliff. Mean ice thickness at the terminus was obtained from ice cliff elevation above mean sea level and water depth near the terminus obtained from echo sounding in a distance of about 100 m from the calving cliff. The mean annual rate of retreat was extracted from terrestrial photogrammetric surveys and from geocoded ASTER images. The accuracy of the estimated ice thickness is +/- 10 m. The accuracy of

the calculation of the mean annual velocity at the terminus is +/- 10 m/yr, while the mean annual retreat rate has an estimated error of +/- 5 m. The determination of the calving parameter c is done in such a way that the total flux for the period 2000-2008 is matched. Fluctuations on a shorter time scale therefore do not play significant role in the calibration procedure.

(8) Steady state in 1850? We do not know how good this assumption is. But in our view the best one can do. . .

(9) Weighing factors. This will be explained in more detail. In fact, they are introduced to avoid that the last part of the length record gets a larger weight simply because there are more data points.

(10) The relation between temperature and ELA will be discussed in some more detail.

(11) Reference will be made to the study of Motyka (2006) for a better perspective.

There are a number of other minor points which will be taken care of in a revised manuscript.

Interactive comment on The Cryosphere Discuss., 4, 949, 2010.