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The Cryosphere Discussions

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

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This is a very interesting paper that uses a simple or 'minimal' numerical model to assess the past, current and future dynamic behaviour and stability of Hansbreen, a the small tidewater glacier in Svalbard. The used model is highly simplified and governed by parametrizations of surface mass balance and calving and because of its simplicity and efficiency allows a robust adjustment/tuning of the model (ELA-history) to the observed retreat record of Hansbreen and an extensive exploration of equilibrium states. Although the 'minimal' is not new, this application to Hansbreen is the first 'real-world' application of it and the good fit with the observed length record indicates the value of such a simple model and its potential to be applied on a larger number of tidewater glaciers in order to estimate future contribution of tidewater glaciers to sea-

level, which is still a major limitation of current estimates from non-ice sheet glaciers. The limitation of such a wider application may be the reliance of the model on some basic knowledge of basal topography.

With this very simple model the expected strongly non-linear behaviour of tidewater glaciers in relation to climate is reproduced (ELA changes) and is a result of a basal overdeepening and the related two feedback mechanisms of (i) increased calving with water depth and (ii) surface elevation dependent surface mass balance. Two stable branches of equilibrium states (here given by glacier length) are found as expected for tidewater glaciers. The current extent of Hansbreen is found to be still refering to the upper (larger-glacier) branch, but the current ELA is far above the value of the stable equilibrium branch and as the climate is not expected to cool significantly for the near future, Hansbreen should according to the modelling retreat rapidly and completely disappear even under current climatic conditions. This is because the ELA is above the existence of the lower equilibrium branch. Re-advance into the upper and large glacier branch would require lowering of the ELA by more than 300m which is very high and so the authors present an interesting investigation of the effect of modified bed geometry through sedimentation on equilibrium states in order to explain re-advance to the LIA-extent from the Holocene Climatic Optimum. The found high sensitivity and bounds for existence of Hansbreen and its predicted disappearance even for current ELAs are crucial results regarding future sea-level rise, as they may similarly apply for many other smaller tidewater in Svalbard.

The paper is of high quality and well written and the authors reach clear and important conclusions. I can see no major issues with it, despite my rather long list of specific and a few more general comments (see below). These issues are minor though, and they should not be seen as criticism on the quality of the paper, but rather as constructive comments to further improve this already excellent document.

Regarding the role of tidewater glaciers in contributing to future sea level rise, this paper is important in the development and application of such simple tidewater models and assessing existence and future behaviour of tidewater glaciers and I strongly recommend to accept this paper for publication after considering the rather minor comments below.

Future glacier behaviour

In the calibration to the length record (p. 958-959) an a priori assumption on the future behaviour of the ELA $E(t)$ has been done by the assumed function for $E(t)$ e.g. a linear increase for the future (plus a bump in the early 20th century). This assumption therefore strongly affects the forcing for the future (obtained ELA for future), which should in my view be pointed out more explicitely. The ELA is only optimised/fitted to data for the period up to the present, the future is pure extrapolation based on the assumed function/scenario (and in reality could potentially be rather different, but i agree a further increase of the ELA seems reasonable given the ongoing warming). A potential different behaviour of the future ELA (stronger warming) is actually explored in this paper, i think it just would be useful to say that the future ELA is based on a scenario. To be fair, the main results and conclusions of this paper (Hansbreen to disappear, non-linear behviour and equilibrium states) are not really affected by the scenario/function of the future ELA much as the ELA is currently well above the critical value (and Hansbreen to disappear anyway).

Pre-LIA bed topography

I can follow the reasoning for the filling up/ removal of the overdeepening of (through sedimentation in the ocean) but I do not quite follow the argument for lowering the morainal shoal of the pre-LIA bed. Before the glacier reached its pre-LIA configuration, it is likely that it had advanced in an earlier colder period to a position similar as today and therefore build up a similar configuration as today (similar morainal shoal).

After retreating and several thousand years of sedimentation, the overdeepenning may have been filled up but the top level of the morainal shoal would not really be lowered (how would it have been removed, what process?). Of course changing sea-level may complicate things a bit further. i guess a higher shoal would not significantly change the dynamic behaviour and just shift or squeeze the top branch of the red curve a bit to the right (towards the blue curve) and it would probably ease an advance a bit. So the conclusions of the paper will not be much affected but still the lower morainal shoal needs some explanation.

Specific comments

p. 950, l. 4: not quite clear what "*global* dynamics of Hansbreen" means here, i guess it refers to the overall dynamics, *global* just sounds to me a bit irritating.

p. 952, l. 4: where does this mean mass balance come from (Reference, or how measured...)

p. 953-954, section **The model**, equation 7, 8,...: I find it slightly confusing that the small letter b is used both for mass balance quantities (balance rate $\dot{b}$) and at the same time for the glacier bed $b(x)$ and $\bar{b}$. Using capital B for the balance rate $\dot{B}$ as done for the total surface balance B_s may solve the issue.

p. 955, equation (3) and line 22: should it not be a $\bar{s}$ instead of the s in the formula. if not and s has the meaning of the sloping bed as in equation (9) then this should be explained or made clear earlier.

p. 955, line 18: a references to Fig. 4 would be helpful here.

p. 957, line 6-7: the model parameters for Oerlemans (2008) are taken here, for α_m ν and κ this makes sense, however it may be interesting to know how sensitive the model results are to these values. Is the balance gradient β also taken from Oerlemans (2008), what is the justification for it? i agree that 0.006 seems a reasonable value, but for Hansbreen there are actual mass balance measurements available and some knowledge on β exists, for example for the period 1991-1995 a value of 0.0066 has been estimated (Vieli et al. 2002) but by now more data should be available. Some reference/justification to why $\beta = 0.006$ is ok for Hansbreen would be useful.

p. 957, line 14: again some more information or a reference to this values for mass loss through calving should be given (how obtained?).

p. 958, first paragraph: the accumulation taken from Pohjola (2004) from Lomonosovfonna may not be that representative for Hansbreen as a long way away and therefore could be criticised, but in this paper this accumulation data is only used to get an idea on precipitation variation and trends with time (to evaluate results) and not used as any model input, so no real need to worry.

p. 958, line 21-22: how good is this assumption of steady state at 1850?

p. 959, line 3: to clarify all the variables and the equation below, i would add here something like "...a cost function that measures the misfit between observed L_k^{obs} and simulated L_k^{sim} glacier length for all available length observations k, defined by... (note also using superscript for refering to obs and sim).

p. 959, equation 21: what are the variable w_k? I assume some weighting variables. Is this standard in the random walk method?

p. 960, line 5: typo: proc*e*dure.

p. 960, line 12: any reference for the "other glaciers" in Svalbard that behave similar.

p. 960, line 19-20: any reference for this 25% precipitation increase to compensate 1 K warming, is this value also robust for the climatic conditions at Hansbreen (i guess a lot of studies are from more continental valley glaciers).

p. 960, line 27: i assume the values in the brackets refer to $E1$. could be clarified by adding in the brackets the variable $E1$ (i.e. $E1 = 2.16...$)

p. 961, line 3-4: i assume this temperature increase is derived from the ELA that has been reconstructed/projected but does it refer to the 2.16m/y increase of E1 or the original 0.72m/y, what is the assumed 'm ELA per degree warming' taken here (reference? i assume about 100m/1K).

p. 961, line 24: not sure if all readers are familiar with the term 'cusp catastrophe'.

p. 962, line 8-13: so in this case the glacier behaves very much like a 'normal' valley glacier .

p. 962, line 17-19: i think the surface altitude mass balance feedback may also play a role here with a flat or overdeepened bed (not just calving).

p. 962, line 19-21: i do not think 'identical' is the correct term here, rather 'similar'. here the instability comes from the calving parametrization which is taken linear with water depth whereas in the mentioned marine ice sheet instability the instability occurs as a result of a terminus steady state flux that depends not necessarily linearly on water depth and comes from the stress balance boundary condition at the terminus/groudingline.

p. 962, line 29: maybe interesting to remark here that the glacier does not even reach or stay on lower branch after retreating from upper branch (but completely disappears, which seems rather dramatic.

p. 964, line 10: i guess it would be interesting to know what this roughly would mean in terms of temperature change required to get the ELA to 46m (or precipitation change required).

p. 964, lines 18-20: Maybe some refernces to Taku Glacier(Motyka eta al 1996 and 2006) and Hubbard Glacier in Alaska maybe useful here as real examples for advancing glaciers through sedimentation/morainal shoal. At Taku Glacier basal sediment removal/erosion also has been estimated at about 2-4m/year (see Motyka et al 2006) relevant for p. 964 last paragraph and p. 965 line 1-4) also gives.

p. 966, lines 12-14: any references to support this wind affected accumulation pattern?

p. 967, line 3: one could add here that this may be the case for many other tidewater calving glaciers in Svalbard/Spitsbergen.

p. 967, line 17: Do the seasonal cycles refer to the Vieli et al 2002 paper? Further, not quite clear what is meant by the mini-surges, is this refering to the spring-speed-up events observed on Hansbreen and discussed in Vieli et al 2000 and Vieli et al 2004.

p. 968, line 1: what are these observations? surface mass balance measurements, net mass balance (geodetic), length changes,....?

p. 968, lines 5-6: strong and important conclusion! What would the 150m roughly mean in degrees temperature? about 1.5 degrees (ignoring precip?).

p. 976, Fig 6: in the caption, the variable B_{tot} should be explained, i assume it is simply the total mass balance ($B_{tot} = B_s + F$).

References that may be relevant

Vieli A., M. Funk, H. Blatter (2000) Tidewater glaciers: frontal flow acceleration and basal sliding, *Annals of Glaciology*, 31, 218.

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Motyka, R. J., and Beget, J. E.(1996). Taku Glacier, southeast Alaska, U.S.A.: Late Holocene history of a tide-water glacier. *Arctic and Alpine Research*, 28, 42ÃŘ51.

Motyka, R. J., M. Truffer, E. M. Kuriger and A. K. Bucki, 2006. Rapid erosion of soft sediments by tidewater glacier advance: Taku Glacier, Alaska, USA, Geophys. Res. Lett., 33, 24504.

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