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The Cryosphere Discussions

Interactive comment on "Modelling historical and recent mass loss of McCall Glacier, Alaska, USA" *by* C. Delcourt et al.

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General:
The paper is interesting and well written and relevant to the study of the response of polythermal glaciers to past and future climate change. It points out the importance of internal accumulation for the dynamics of polythermal glaciers and includes a crude representation of this process in a dynamical model. I have comments about certain aspects of the model and the modelling setup that should be improved before publication.

Specific comments:

1. A longitudinally varying width of the glacier should be specified in the model as pointed out in an interactive comment by J. Oerlemans posted on 30 November 2007. The historical integration involves a time period over which the ELA changes by on the order of 500 m. This presumably implies a change in conditions in the rather large Lower Cirque (LC), which is likely to have been a part of the accumulation area in the past but is now a part of the ablation area according to the paper. Ignoring this and other complications involving longitudinal changes in the width of the ice flow channel by assuming a uniform width must have considerable consequences in a comparison of the model results with measurements and historical indications about terminus positions and ice thickness derived from lateral moraines.

2. The internal accumulation is specified as 50% of the total accumulation in 1970 and appears to be the same independent of any other assumptions about ELA changes or associated temperature changes. This may be OK for the historical integrations as argued in the paper but could be questionable for the future integration shown in Figure 4. Over a 1000 year period it seems likely that the thermal structure of the glacier will change with time for an 800 m vertical span of the different assumed ELA (see Fig. 4) so that the "ability" of the the cold ice at depth to continue to freeze percolating meltwater will change from the 1970 conditions. This problem should at least be mentioned in the text. Furthermore, I don't see heat released by the internal accumulation described explicitly as a heat source in the description of the heat transfer model on p. 388, although this heat source is mentioned in other contexts elsewhere in the paper. This should be clarified. Is the role of the heat released by the internal accumulation always dealt with by assuming the same -6.3°C 10-m temperature in the accumulation zone? If this heat source is not somehow included in future simulations with a large change in ELA then this should be justified quantitatively. Note that if 1 m water equivalent of meltwater freezes at depth annually, the released heat is equivalent to

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the heat required to raise the temperature of an ice layer at depth tens of metres in thickness by ∼(5–10)°C. Thus, if some change in the thermal balance at the surface takes place so that the temperature of cold ice that is advected into the deeper parts of the glacier changes, one may expect large changes in internal temperatures and internal accumulation. This problem should be discussed in the context of the heat transfer model.

3. The statement near the top on p. 392 that the model results can be an underestimation of the time over which the glacier would largely disappear because the volume response time of Jóhannesson et al. (1989) (50–75 years) is shorter than the modelled life span of the glacier (∼300 years) is misleading. The volume response time is intended for small relative changes in glacier geometry and does not consider the effect of changes in temperature for polythermal glaciers. The numerical model of the paper should provide the definitive result in case there is a disagreement between the results of the model and what can be concluded from the volume response time because the model should provide a much more realistic description of the dynamics of a disintegrating glacier. I suggest eliminating the comparison with the volume response time and the reference to Jóhannesson et al. (1989).

4. The abbreviation "HOM" for the model of the paper is given first on p. 393-17 without defining it earlier in the text.

5. There is no comparison of the results obtained by the HOM and SIA models made in the text of paper as pointed out by in the comment by Oerlemans although the authors show results from both models in Figure 6.

Technical or minor corrections:

1. IPCC (2007) should perhaps be referenced on p. 387 rather than IPCC (2001).

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2. The sentence starting with "This operation *somehow* softens the ice ..." on p. 391-3 is strange.

3. On p. 394-13 add "Figure" and remove the parentheses in "... as defined in Figure 2.".

4. The sentence starting with "Results of experiment C are also more *confident* when ..." on p. 395-11 is strange.

5. It is hard to distinguish the curves and symbols on Figure 6. Is also a bit confusing that experiment A is said to be shown with dotted and dashed curves which cannot be seen in the figures. This is explained in the figure caption but it is nevertheless confusing. Why not state "Exp. A/B" directly in the caption and drop the dotted and dashed curves from the legend box?

Interactive comment on The Cryosphere Discuss., 1, 385, 2007.