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The Cryosphere Discussions

Interactive comment on "Response of the ice cap Hardangerjøkulen in southern Norway to the 20th and 21st century climates" *by* R. H. Giesen and J. Oerlemans

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Overall Impression:

The authors have assembled a thorough look at a relatively small icecap in a region of great climatological interest for its proximity to direct impacts from the North Atlantic Oscillation, North Atlantic SSTs, and high latitude climate change. Their approach couples a moderately sophisticated mass balance model with simple ice dynamics to demonstrate the need for incorporating ice dynamics when assessing glacier change over decadal to centennial time scales. The paper is very well written and concise, which is good because it covers a lot of ground! It suffers from the overall simplicity

of the approach which fails to account for important aspects of climate as it influences glaciers. Overall, I suggest publishing this paper with revisions.

Major Concerns:

The authors' approach to incorporating a climate change scenario is very simplistic (which they acknowledge) and has the major fault of ignoring climate variability that will very likely play a large part in dictating the fate of all glaciers, especially those in scandanavia, which show decadal scale variability. The authors note that Hardangerjøkulen has undergone decadal-scale positive mass balance anomalies as recently as the early 1990s. Because studies of cryospheric change can be very useful to policy makers for whom such time-scales are most critical, I regard failure to incorporate such variability a major drawback of the research presented here. The lack of variability, even in a statistically imposed manner, renders the timing of the projection for disappearance of the ice cap questionable although I don't doubt the result that the fate of Hardangerjøkulen is sealed.

I'm very curious about your decision to allow for a lapse rate of 6.5 K km-1 across the ice cap especially because many researchers in your institute and elsewhere have shown that, over a melting ice surface, such a lapse rate is very rarely observed. The authors note that the summit of the ice cap yields meters (w.e.) of ablation so clearly the ice cap is melting everywhere for much of the summer. What are your justifications for choosing this lapse rate? What do you think the impacts are? My impression is that your need for a high turbulent exchange coefficient near the summit is offsetting the overly cold temperatures that would arise in prescribing such a lapse rate – you are getting the right result for the wrong reasons.

In addition to neglecting the importance of interannual variability for glacier mass balance as described above, you implicitly neglect the importance of day-to-day variability by retaining only climatological means of individual days. I allow that it is very difficult to impose such variability without more sophisticated regional climate modelling or a

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more clever use of the data at-hand. I would at least expect to see the importance of such variability mentioned and how you expect that failing to include it influences your results. With regard to temperature, because the freezing point loosely clips the capability for melting to occur, failing to include variability will result in too-little melt. I think this is apparent in your results (fig 5c) where the ctrl/ctrl experiment yields a too-positive mass balance.

I think that an error estimate is becoming necessary for studies such as these if these results are to be taken seriously by the increasingly engaged public. The surface energy balance model is extolled as physically accurate, but is so highly parameterized that I'm unsure that it lends any greater certainty to your results than a simplified surface mass balance model would. This is too much to ask as a revision, but should be strongly considered in your future applications of this approach.

Finally, give more detail about how the precipitation gradient was imposed and either show a map of the enhancement factor or give an equation defining the plane. Was this gradient imposed in the course of tuning to data? Was it randomly selected? The vertical distribution is also mentioned but not numerically described. Please describe this.

Minor Comments:

948-1: The first sentence should be rewritten. It imples equal weight of forcing by geometry and climate, but geometry driving mass balance is rarely the case. This is better described later in the paper.

949-0: "relatively" and "rather" can be deleted from this line.

950-27: "is operating on" should read "has been in operation"

951-11-29: Although interesting to some readers, I'm not sure the discussion of the Holocene history of the ice cap is warranted. Consider shortening.

954-23: What kind of exponential decrease for water vapour? Scale height for water

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vapour?

955, section 3.3: It doesn't appear that you describe your source for the basal topography. How confident are you in the accuracy of the basal topography? How does this affect the results?

There are a few instances where Giesen refers to his Ph.D. thesis especially with regard to model parameters. These should be included in this paper either as a table or as an appendix for both mass balance and ice dynamics.

Section 4.3: For reference which emission scenario, tied to which GCM is your 3 degree, 10% precip change derived from?

A 10% change in the turbulent flux coefficients seems arbitrary to reflect a change in wind speed especially because wind speed does not scale linearly with turbulent heat fluxes. Please justify this.

959-14: Not clear what this sentence is saying.

960-24: I think you mean "meteorological" instead of "meteo" in this case.

968-10-12: The differences may look small because the final volume is compared to a large initial volume. Compare between final volumes (i.e. 50% smaller than the control run). Also, are the trajectories the same for the various experiemnts?

Figure 9: For reference, these figures should show the location of the actual ice cap margin at the various years either from mapped moraines, or aerial photos (mentioned at 951-8-10). Otherwise, this figure does little to demonstrate the performance of the model and should be discarded.

Figure 7: Based on the caption, I have no idea what this figure is presenting especially panel b. A little more description is needed here.

Interactive comment on The Cryosphere Discuss., 3, 947, 2009.