The Cryosphere Discuss., 3, C374–C378, 2009
www.the-cryosphere-discuss.net/3/C374/2009/

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

Anonymous Referee #1

Received and published: 3 December 2009

This paper describes the evolution of a Norwegian ice cap over the last 100 years and for the 21st century based on a state-of-the-art modelling approach and using a comprehensive set of field data for validation. The authors directly couple glacier surface mass balance calculations based on an energy balance formulation and the description of ice flow dynamics. The paper is well written and covers a wide range of different aspects of sensitivity and processes in glacier change modelling. It therefore deserves publication in 'The Cryosphere'.
However, I still see some important points that should be addressed by the authors.

- The description of one very important data source is completely missing. Only

when reading the Acknowledgements I learned that actually measurements of the ice thickness distribution exist. As the bedrock elevation and, consequently, the ice volume directly impact on (i) the ice flow velocities, (ii) the simulated glacier retreat, and (iii) the projected disappearance of the glacier, the origin of this basic initial condition for glacier evolution modelling and the related uncertainties should be carefully described.

- The validation of the coupled model over the 20th century is a weak point. The authors initialize their validation run using a *modelled* glacier geometry. It is not completely clear how this geometry was obtained, but it seems to originate from a 'calibration'. So the model is validated with a initial geometry that was first calibrated applying the same model? It is thus not surprising that the current day glacier geometry is matched when running the model forward in time. Errors in (i) the ice flow dynamics, (ii) the spatial variation in mass balance affecting the distributed elevation changes, or (iii) the bedrock elevation would not be revealed in this validation experiment. Therefore, I am not sure what can be learned from this validation of the coupled model over the last 100 years. Anyway, the authors do their best given the available field data, but the implications of the shortcomings in their validation procedure should be more carefully discussed.

- The authors apply a physically based energy balance model and therefore conclude that "projected changes in the meteorological variables can be directly incorporated in the model" (page 970, line 4-5). I appreciate the way their model is described and applied, however, I have the impression that they might be a bit more critical. Many parameterizations were established for climate conditions typical for recent yearss using one single locations on the ice cap. Calculated energy fluxes were validated using variables observed at the same weather station. I guess there is a considerable uncertainty in the variation of the energy balance components in space due to the extrapolation of the input data, such as temperature, precipitation and cloudiness. Furthermore, parameterizations established based on a single point might not be representative for (i) the entire ice cap, and (ii) for the entire 21st century (with strong climate warming changes in the calibrated parameterizations are likely). This should be discussed more critically. Is it even possible to attach error bars to the projections?

Detailed comments:

- page 949, line 13: A short review of literature might be of benefit. What was already done in the field of coupled mass balance - ice flow modelling on mountain glaciers over comparable time spans?
- page 950, line 22: Isn't this a rate? In my opinion mass balances should be given in m w.e. a^{-1}. Also elsewhere in the text.
- page 951, line 11-29: This is quite a long summary of glacier changes in the Holocene. It could be significantly shortened in the context of this paper.
- page 954, line 16: Does this mean that superimposed ice formation at the base of the snowpack in early summer is not accounted for? The authors just state that the potential error due to this assumption is small. How small?
- page 954, line 20: A reference to "measured" air temperature is made before mentioning any field data. The available meteorological records should first be described. Here, the authors could also define the meaning of "local" meteorological data (see below).
- page 954, line 21: What is variability in the seasonal lapse rate, and how does the assumption of an annually constant number affect the calculations?
- page 954, line 24: 'observed' cloud fraction - observed where?

- page 955, line 2-4: The prescribed accumulation pattern is based on measurements. Winter accumulation or annual balance? Is the west-east gradient superimposed in the modelling also seen in some in-situ field data?
- page 955, line 13: The authors should state here which 'measurements' are used, over which time period, how many locations.
- page 959, line 18: Here and elsewhere: With "local" meteorological data the authors refer to the combined measured time series of several stations around the glacier. The term "local" seems a bit imprecise to me. Its meaning should at least be clearly defined somewhere in the paper.
- page 960, line 18-26: Also after reading this paragraph three times I did not really get what the authors mean by 'meteo record'. What is, for example, the meaning of "...varying the set with other meteorological variables." This paragraph should by clarified.
- page 961, line 1: What is the mean difference between measured and modelled snow depth? When assuming the same bias over the entire modelling period, what would be the impact on the calculated mass budget? The modelling of precipitation, and in particular, its spatial distribution seems to be the most important uncertainty and should be addressed more critically.
- page 962, line 8: Is this ± 15 m?
- page 963, line 1-13: This paragraph is a discussion of the spatial mass balance distribution given by the model. However, due to the effect of wind redistribution, for example, or other processes not incorporated in the model, it could significantly differ from the simulated pattern. As long as there are no direct field data that would confirm this spatial distribution, this discussion is too long and invalidated in my opinion.

- Section 5.5 Mass balance sensitivity: Although I find this section interesting it is not directly related to the rest of the paper. The values for the mass balance sensitivity are not the basis for further interpretation. Therefore, this section could be shortened. The paper is already quite long in its present form.
- page 967, line 23 / page 968, line 3: Changes in winter balance are given relative to the location of the maximum in temperature change, and not relative to present day, am I correct? This should be clarified.
- page 972, line 12: Have these meteorological measurements been homogenized / tested and corrected for biases? If not this could be stated here.
- page 972, line 17: Although shown on a map, the distance of the weather stations to the glacier should be given.
- page 973, line 3: What is the correlation coefficient r^2?
- page 974, line 10: Is the determination of the seasonal lapse rates sound from a statistical point of view? The relation is based on only five data points.

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