The Cryosphere Discuss., 3, C423–C428, 2009
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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

Anonymous Referee #2

Received and published: 8 December 2009

General comments

This paper describes an application of a coupled model, consisting of a surface energy balance model and vertically integrated SIA ice flow model, on Hardangerjøkulen ice cap in Norway. The evolution of the ice cap during the 20th and 21st centuries is simulated with input data from meteorological stations in the vicinity of the ice cap and for the 21st century a projection based on climate model runs is applied. All available data is used to calibrated and validate the model runs. This paper is well written and concise and the topic is relevant to the scope of TC. No model development is presented, but two previously developed models are coupled and applied for the first time on this

ice cap. My suggestion to editor is to accept the manuscript for publication in TC with minor changes. The paper would improve if authors consider and clarify the text in a few places as indicated below.

Model description could be more specific in abstract (line 5), page 949 (line 19) and page 970 (line 2) it should be stated that authors use surface energy balance model and a vertically integrated shallow ice approximation (SIA) model.

Model description in text is rather short and it would be beneficial for readers to get more details on how the different models have been calibrated and what the optimization parameters are (page 952). What are the deformation and sliding parameters on the flow model? is the sliding parameter spatially and temporally constant? Explain better the dynamic calibration of the ice-flow model.

The surface mass balance model similarly (suggest to change the titel 3.2 to include Surface). The calibrated parameters would be of interest and could be published in a table together with ice flow model parameters. Page 955, line 6-8 explain better the albedo parameterization (and values), add references to the 3 layer subsurface model (955, 11), page 955, line 14 How much larger coefficients are applied? Page 956 lines 5-10 Explain better how turbulent exchange coefficient is adjusted, is there a reference to this method? How sensitive is the result to this assumption? page 956, 17 How is this simple parameterization for air pressure and cloudiness, is there a reference?

There is missing validation of the coupled model that should be fairly easy to do, that is to compared the modeled 1995 ice thickness and extent to the 1995 DEM, or the 1961 DEM, that appear to be available (page 962, 7)

The discussion of the seasonally variable lapse rate is confusing(page 954, 19-22) Why is it only used in one case, not when extrapolation is done for the whole ice cap?. Is there data supporting this seasonal variation? How sensitive is the model result to this assumption?

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Suggest to rewrite the description of the Meteorological data, it is very confusing, "local" should be defined, much of the important information is in the appendix, which makes the reading of the paper harder, than if the necessary explanations were in the text. It is strange to start the section named Control climate with discussion of the future climate. It is not clear how the control climate is created, is it daily averages over the 30 years period? should be explained clearer.

Specific comments

page 949, line 27 rewrite "lower- order" is usually used to refer to the order of approximation in the stress balance equation, not the dimension of the model.

page 948, line 8 and page 956, line 14, suggest to use force, rather than drive models

page 953, line 24 not clear what "towards surface" is, upward or downward?

page 953, line 25, is only surface heated, not the snow/ice below?

page 954, line 8 "solid mass" do you mean ice or snow?

page 955, line 3, "flow" do you mean wind?

page 955, line 4, is there data to support this assumption?

page 957, line 1 Clarify how the control climate is created

page 957, line 8, how about comparing the meteo records from 2001-2005 to AWS data, how well does that compare?

page 958, line 19 How much larger than zero?

page 958, line 23 is the timing not available from the RegClim projections?

page 958, line 28, rewrite, air temperature change is varied .. from a mean change of 3decC?

page 959, line 3, rewrite, the model should simulate the present day surface energy

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and mass balance? not "the effect of present day climate on the surface energy and mass balance should be well represented in the model"

page 959, lines 13-24, see above, this paragraph should be rewritten to clarify how the calibration is done, define the local data etc.

page 960, titel 5.2 this is not clear chapter title, one could think that here you are comparing the model surface elevation (DEM) to observed (which should be done, see comment above), perhaps this should be accumulation/ablation at Midtdalsbreen?

page 960, line 20-26, this is not clear paragraph, rewrite it and clarify "data set", "meteo" record, add reference earlier, is this described better in that reference?

page 961, line 15, later than what?

page 961, line 16 what is threshold temperature for snow? is there a reference?

page 961, line 20 Is there a reference for the NVE measurements?

page 962, line 1, is it larger than measured every year? line 12, does this mean that the model is not sensitive to the input data? line 16, adapted, perhaps better to use scaled? line 19 specify here if control or "local" data is used line 21 Define what is meant by altitudinal mean, is that a good measure? line 21, Why not the control climate? page 963, line 1-8, Does this indicate that this effect is overestimated in the model? is there any way to test that?

age 963, line 21 DEM is topography, do you mean 1995 topography?

page 964, line 16 change -> sensitivity

page 965, line 5 and ice dynamics ?

page 965, line 10, The initial condition of the ice cap model appears to affect the result (simulated retreat of Midtdalsbreen) How is the initial state created? A short description here is appropriate, is it a steady state with changed climate input? how changed? how

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different from the 1995 DEM is this initial state?

page 966, line 3, how about comparing the simulated surface elevation to the 1961 DEM?

page 966, line 3-5 and 16-19, Do you have ideas of why this difference could be? Is it the geometry, precipitation distribution or something else?

page 967, line 2 omit "quickly" (relative to what?)

page 967, line 7 Not clear what is meant here, is the ice away from the the flow line?

page 967, line 13 omit "by far"

page 967, lines 16-19, Not clear what is meant here, clarify

page 968, line 1-3, Is this a real effect supported by measurements, or results of how precipitation is related to temperature in the model?

page 968, line 23, suggest "destined" rather than "bound"

page 968, line 23, add "further" (even without further....)

page 969, line 1, ice layer, do you mean glacier thickness?

page 970 line 1, suggest to change to Discussion and Conclusions

page 970, line 11, "will have" suggest to rewrite with "projected" or something like that

page 972 , line 17, necessary for what? line 18-19 is the data corrected in any way? clarify line 22 similar to what?

page 974, line 7, rewrite, define "local" see comments above

page 974, line 28, what is threshold temperature for snow?

Figure 4, caption, explain better similar runs, rewrite after clarifying in the text (local etc) Figure 5, Rewrite figure caption after clarifying text Figure 6, in figure caption state

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which 2 input data sets figure 7, Define altitudinal deviation, what does this figure tell us?

Figure 9, Suggest to add the dotted LIA extent as in Figure 2, how about plotting the difference between the modeled and measured 1995 DEM?

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