The Cryosphere Discuss., 3, C142–C145, 2009
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The Cryosphere Discussions

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3, C142–C145, 2009

Interactive Comment

Interactive comment on "Diagnostic and prognostic simulations with a full Stokes model accounting for superimposed ice of Midtre Lovénbreen, Svalbard" *by* T. Zwinger and J. C. Moore

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Received and published: 20 August 2009

General comments

In this discussion paper, authors apply a thermo-mechanically coupled full Stokes model to one of the polythermal glaciers in Svalbard region. They discuss both steady-state and transient-state behavior of the glacier in details. The strength of the employed model is that it accounts for the latent heat released during the formation of superimposed ice. However the sole impact of superimposed ice on the glacier's thermody-

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Interactive Discussion

Discussion Paper

namics, and hence on its overall dynamics (e.g. velocity field and surface evolution) is not distinguished. This could have been easily done by comparing corresponding results from the models with or without superimposed ice. The constraint of the model is that it does not include basal sliding, although the presence of warm basal ice (Fig. 5) is likely to cause sliding (p. 480, line 1-4).

To me, the only major issue to be commented on is unnecessarily long explanation of dip angle. Authors spend about two full pages (p. 489-491) describing the dip angle, which certainly diverts readers' mind (at least mine) from the main theme of the paper. In the context of this paper, a brief description of how well dip angles of modeled isochrones match with the ones obtained from GPR data (Fig. 9), and why this is useful on explaining the age of ice would have been sufficient. Note that, in the abstract of the paper, authors do not include even a single sentence about such a long explanation of dip angle, which reflects its worthlessness. Apart from that, the flow of writing is excellent (although, I encounter some long sentences every here and there, a few of them are noted below) and the paper is self contained. I strongly recommend this paper for publication subject to addressing following specific comments.

Specific comments

Local convention: Every comment begins with page number followed by line number, dot being in between. For example, page 480 line 5 is written as 480.5.

478.9-10: Which year of data is used as a constant mass balance (accumulation/ablation) to run the prognostic simulations?

479.3-5: Please mention the corresponding year/s (AD) for these figures (glacier dimensions and ELA). How many years of data are used to arrive at the average ELA of 395 m a.s.l.?

479.13-16: Rewrite this sentence (A surface energy mass balance...) more clearly.

479.22-27: This sentence (In common with the model...) is too long, please break it down into a few. Are annual surface temperature (line 23) and air temperature on the glacier (line 26) the same? Shouldn't the lapse rate (line 27) be negative?

479.29: Offset by what °C? When was the temperature -4.5°C measured at stake #7? Is that a single year measurement or an average of a few years?

480.10: The superimposed ice is included in the model, but its impact on the energy balance (and on overall glacier dynamics) is not exclusively analyzed.

480/481: Looking at Fig. 1, the elevation ranges (480.22, 481.7&14) for three distinct regions seem to be approximate ones. Please clarify them by writing, for example (481.7), approx. 400-450 m a.s.l. or so.

482.5-11: Is the constant value of $d_{ice} = 5$ m representative for both the winter and summer temperature distribution? The term d_{ice} is penetration depth of the surface temperature signal, which does not necessarily be the same in summer (the ice surface is partly/fully exposed) and in winter (the surface is covered by a few meters of fresh snow). I am just curious whether it matters significantly.

483.26: It's worth mentioning a couple of sentences about element type and basis (shape) function employed.

484.10: Make p (pressure) *italic* as it appears elsewhere.

484.17: Mention that this equation is for Glen's flow law exponent $n = 3$, although it's a general convention in ice rheology.

485.16-19: How much does $h_{min} = 5$ m (at domain outline) alter the overall dynamics of glacier, compared to having a conventional Dirichlet boundary condition $s_{outline} = b$? What happens if we consider a very small non zero h_{min} (say 0.1 m) instead of $h_{min} = 5$ m? What sort of effect will it have on numerical stability? I think it's worth explaining this issue briefly.

489.10-11: How the age of ice (for diagnostic run) scaled to the order of thousands of years? What do you mean by "a physically feasible range"?

493.21: 'Adopt' would be appropriate term here (We adopt a simple time-averaged...).

Fig.1: Mention that the solution of surface temperature is obtained from the diagnostic run. Clarify whether the contours represent for surface or bedrock elevation.

Fig.2: Remove 'iso' from the last line. It should be- The contours indicate...

Fig.4: I doubt about this result, especially around stake #5. What causes such an abrupt switch in age from few hundreds to few thousands year? The solution for the age of ice comes from the advective equation (Eq. 12). Peclet number is well above 1, indicating the dominance of advection. Velocity field and bedrock topography are pretty uniform. With all these, I would expect a smooth transition in age of ice.

Fig.8: Annotate the iso-lines of surface age distribution.

Fig.11: Annotate the ice thickness iso lines in each of the four figures.

Final note: Please be consistent with the unit of temperature. I can see temperature both in Kelvin (e.g. 480.26, 482.12) and in degree Centigrade (e.g. 479.25, 482.10).

Interactive comment on The Cryosphere Discuss., 3, 477, 2009.