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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "The Gregoriev Ice Cap evolution according to the 2-D ice flowline model for various climatic scenarios in the future" *by* Y. V. Konovalov and O. V. Nagornov

Anonymous Referee #1

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General appreciation

This paper investigates the climatic sensitivity of the Gregoriev Ice Cap using two different methods. The first method is a flowline model including higher-order stress gradients, similar to the Pattyn (2000) model. Perturbations are carried out for different amplitudes and periodicities in background temperature, comparable to changes taken from the literature, from which glacier front variations are derived. The second approach relies on the Oerlemans method to reconstruct air temperatures based on front variations that were obtained from the previous method. However, by using the modeled front variations as an input, this results in circular reasoning.

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Interactive Discussion
Discussion Paper

The paper describes in a rather lengthy way a model that was described in detail elsewhere. However, emphasis on the differences between the Pattyn and the Konovalov model, especially on behalf on the numerical solution could be more enhanced. Next to this, it is not clear what the main focus of the paper is. Not much information on the present/past glacier and its mass balance is available, which limits useful scenarios on future glacier behavior. It is a pity that not more geomorphologic evidence is available to constrain observed glacier lengths. Therefore, the model that is used is probably too complex for the experiments presented here. Since the bedrock profile is rather smooth and basal sliding limited, one may wonder whether a shallow-ice model would do the trick as well.

The paper lacks a clear hypothesis and the model experiments should be devised in such a way that they can verify the hypothesis proposed.

Finally, the paper is written in poor English, with many grammatical errors. It should therefore be corrected by a native English speaker prior to submission.

In view of the overall remarks, I propose to reject the paper in its present form. If the hypothesis is clearly defined, the results adequately described as a function of the hypothesis, the model description made less elaborate and the text reviewed by a native English speaker, a resubmission of the manuscript may be envisaged.

Detailed remarks

Introduction:

It should be clarified where exactly the glacier is situated. Central Asia is large and from Fig.1 it is impossible to derive this information. The glacier flowline should be draw accompagnied by the width of the glacier as used in the parameterization.

Modeling:

This section is written in too much detail. It is based on an already published model and its description could therefore be reduced considerably.

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Eq.(3): The authors use a transport equation for the solution of continuity. Such an advection equation is generally used for fast glacier motion, such as ice streams or ice shelves. However, in case of glaciers it is more common to use a diffusivity equation, which avoids to add artificial viscosity to stabilize the solution.

A geothermal heat flux of 0 W/m^2 is extremely low, and hard to imagine plausible in mountainous areas. Even for central Asia, geothermal heating is of the order of 40-80 mW/m^2. It is not very clear why it was necessary to evolve the temperature field, as only sliding occurred near the front of the glacier, which is quite interesting, since the ice here is the thinnest.

Basal sliding and longitudinal stress coupling:

The basal sliding law used is a linear (viscous) one. However, basal sliding is supposed to follow a non-linear relationship as a function of basal shear stress. A proper sliding law (Weertman type for instance) should be implemented. Furthermore, it is not clear what the amount of basal sliding compared to the deformational velocity is. Since sliding is limited at the front, I suspect that only a limited amount of basal sliding is present. This together with the simple geometry of the glacier makes one wonder why a higher-order model should be taken. The shallow-ice approximation is perfectly valid for conditions where ice is (mostly) frozen to the bedrock and where generally low surface slopes exist (or at least bedrock is rather smooth). Is there a benefit for using a higher-order model? Is longitudinal stress coupling an important factor in the evolution of the glacier? These questions can easily be addressed by comparing the results with a shallow-ice model. Mass balance variations will probably dominate.

Numerical solution

The details given here can easily be left out. However, it would be beneficial to include the difference in approach compared to other (published) higher-order models.

Steady-state experiments

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It is never stated whether the model successfully passed the ISMIP-HOM tests. In that paper it is shown that another model by the same author (a Full Stokes 3D model) participated in experiment A and E. These experiments are without basal sliding. The presented flowline model in this submitted manuscript does however include basal sliding, but it is not stated whether other ISMIP-HOM tests were done.

Experiments are done for m=0.15 and m=0.3. Both values are rather low, which means that the ice has a very low viscosity or is eventually colder than modeled. Could it be that the cold ice is a relict of a larger geometry in the past (when colder temperatures were prevalent), as is sometimes the case with polythermal glaciers in the Arctic?

Mass balance measurements are very sparse and the two balance years shown give a rather large spread in mass balance, from which the mean value is taken. A sensitivity on both mass balance curves would be interesting, especially in relation to the low values of m.

Interactive comment on The Cryosphere Discuss., 3, 77, 2009.