Interactive comment on "The Gregoriev Ice Cap length changes derived by 2-D ice flow line model for harmonic climate histories" *by* Y. V. Konovalov and O. V. Nagornov

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Summary

The authors investigate correlations between changes glacier terminus position and annual air temperature by means of ice flow modeling. The Finite Difference ice flow model solves diagnostic equations for the conservation of mass, momentum and energy along a flow line together with a prognostic equation for the evolution of the ice thickness. The momentum balance equation solved by the model is a second-order approximation to the full Stokes model. No-slip conditions at the glacier bed are assumed, except if a certain threshold of the basal shear stress is reached in an area close to the glacier tongue, and Weertman-type sliding is introduced. The model was driven by sinusoidal time series for temperature and mass balance as upper boundary conditions. The paper assumes a linear relationship between temperature, glacier length, and glacier length changes with three parameters. Values for these three parameters were obtained for a variety of different time series. Parameters were compared to the parameters derived by *Oerlemans* (2005), and good agreement is shown.

General comments

The paper under review was originally submitted to the journal "The Cryosphere" in October 2008 and received very detailed reviews by two referees who both suggested to rejected the paper in its original form. The paper in its current form addresses some of the concerns of the two original referees (e.g. to include a non-linear sliding law), and the English has improved considerably. However, most of the major concerns pointed out by the two previous reviewers are still valid, and include:

(1) The aim of the paper remains unclear and it is very difficult to follow the author's arguments. If the focus is on temperature reconstruction based on glacier length changes, then the modeling section is still too detailed. On the other hand, the numerical model itself does not contain enough new results to warrant publication. This has already been pointed out in detail.

(2) The conclusion that the geothermal heat flux at the glacier base must be close to zero because measured temperature do not vary substantially between 10m and 45m is unphysical, and neither does it prove that the glacier is frozen to the bed. There is no justification to extrapolate observed temperatures down to the bedrock. From Figure 2, I infer the glacier has an approximate thickness of 100m in the area of the borehole. The shape of the temperature gradient depends on the geothermal heat flux, deformational heating, and horizontal and vertical heat transport by both advection and diffusion. In general, temperatures in a column of ice increase with depth because (1) the geothermal heat flux entering the glacier at the base, and (2) energy production due strain heating increases due to increasing strain rates. Increasing temperatures with depth are thus highly likely, and a zero geothermal heat flux relatively unlikely.

(3) The numerical inversion approach to reconstruct the air temperature is indeed an interesting topic, however, this is still not clearly stated in the text and requires to read the manuscript several times to get the idea. To me, this seems to be the most promising idea of the paper, and I thus suggest to focus on this part.

Specific comments

I'm not familiar with the term "mechanical equilibrium equation", do you mean "stress balance" or, equivalently, "momentum balance"?

The reference to equation (3) may be a type error, I don't find that equation in the cited *MacAyeal* (1996) paper.

As pointed out in the previous reviews, mass balance will most likely dominate over ice dynamics, and thus a shallow-ice or first-order model may be equally appropriate. But, of course, there is nothing wrong with using a second-order ice flow model. Just of curiosity: Why is it assumed that the glacier can only slide in the lowermost (600m) part of the glacier, and only if a certain yield stress is reached? Is there some observational evidence? Anyway, this question is in the context of this paper relatively unimportant, and I would expect similar values for the investigated parameters α, β and γ even for no-slip conditions everywhere.

I'm wondering if the authors are aware of the papers by H. Blatter and colleagues, e.g. Blatter (1995); Colinge and Blatter (1998); Blatter et al. (1998); Colinge and Rappaz (1999). There is no need to cite these papers, but the authors might find valuable information about several points mentioned in Sections 3 and 4. Blatter (1995) presents the scale analysis underlying the stress balance equations used by, e.g., Pattyn (2002, 2003), while Colinge and Blatter (1998); Blatter et al. (1998) discuss the singularity arising at slip/no-slip transitions. And Colinge and Rappaz (1999) prove the

existence and uniqueness of a solution for the first order stress balance approximation, though I don't know if the prove can be extended to the second order approximation used in this study.

Concluding remarks

I therefore suggest to accept the paper only after major revision and I encourage the authors to resubmit a revised paper that carefully addresses the following points: (1) Shorten and simplify the section about ice flow modeling, make appropriate citations to already published work. (2) Focus on the temperature reconstruction part, explain in a clear and concise way the method, and thoroughly discuss the results. Make sure right from the beginning of the paper that you are doing an inverse approach. I was quite confused when I read the paper the first two or three times and I was wondering about circular reasoning. Only after reading the manuscript a few times, I figured out that inverse methods are being used. (3) Address more carefully the concerns raised by the previous two reviewers.

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