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Earth System Dynamics Discussions

ESDD
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Interactive Comment

Interactive comment on "Two-dimensional prognostic experiments for fast-flowing ice streams from the Academy of Sciences Ice Cap: future modeled histories obtained for the reference surface mass balance" *by* Y. V. Konovalov and O. V. Nagornov

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This paper presents simulations of the future evolution of 3 major outlet glaciers of the Academy of Sciences Ice Cap using a 2D thermodynamically coupled flowline model. The present study builds on Konovalov et al., 2012, deriving basal friction coefficients from inversion of observed ice-surface velocities (1995). Given a constant, elevation dependent surface mass balance, both glacier thickness and extent is found to decline.

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The model is able to reproduce the phenomenon known as tidewater glacier instability – although the authors do not acknowledge this – i.e. rapid retreat of the grounding line in case of a retreat into deeper water and, vice versa, a stabilizing effect upon retreat into shallow water.

The topic of the paper is foremost of interest to the glaciological community and a submission to a journal such as "The Cryosphere" seems more natural to me. However, it is not irrelevant for ESSD. I have a list of major issues that I suggest to be addressed before the manuscript can be accepted. First of all, I have concerns about the use of a constant basal friction coefficient field for prognostic runs over a large timescale of 500 years, where other authors report on significant variability in the dynamics of the studied outlet glaciers (Moholdt et al., 2012). Secondly, the modeled temperature field indicates basal temperatures in the fast flowing regions well below freezing. Velocities up to 200m yr-1 are than explained by submelt-sliding at rates in agreement with Echelmeyer and Zhongxiang, 1987. However, the cited study obtained flow rates of 0.5 mm day-1 (180 mm day-1) and are thus 3 orders of magnitude smaller. The use of constant friction coefficients needs to be better justified and the related uncertainties assessed. The modeled temperature field should also be reconsidered and the discussion of glaciological processes at the bed adjusted accordingly. How do ice temperatures and the basal thermal regime, in particular, affect the ice flow? The manuscript is on the whole free of typos, however, the construction of sentences and the choice of words needs to be improved.

GENERAL COMMENTS

1 Use of constant basal friction coefficients

Moholdt et al., 2012 showed that the dynamics of the studied outlet glaciers varied significantly on short timescales of years to decades. Basal friction coefficients calculated from 1995 velocity fields therefore represent a snapshot in time and are unlikely to be representative over long time scales of 500 years. The authors should investigate the

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sensitivity of their model results to significant variations in friction coefficients as they would be obtained based on significantly different velocity fields. The associated uncertainties should be discussed. How do the model results compare to the observations from Moholdt et al., 2012?

2 Initial and simulated ice temperature and effect on ice dynamics

The authors admit that latent heat release by meltwater refreezing within the snow and firn is not considered. However, in an earlier study, Nagornov et al., 2005 (Ann. Glac.) point out the importance of subsurface melting for the temperature profile. Firn-warming may have been negligible in the Little ice age, and consequently, present ice temperatures in the lower part of the glacier may not be affected by it. However, firn-warming is important today, and will also affect the basal thermal regime over a long timescale of 500 years. The temperature fields displayed in fig.5 do therefore not show the expected temperature distribution with warmer near-surface-ice temperatures in the accumulation area (above 400m elevation; where firnwarming operates) and colder near-surface-ice temperatures in the ablation area, despite of warmer surface air temperatures. What processes are accounted for in the temperature calculation? What is the effect of ice temperature and the basal thermal regime, in particular, on ice flow and basal motion? The simulated basal temperatures in the fast flowing regions are well below freezing. Velocities up to 200m yr-1 are explained by submelt-sliding at rates in agreement with Echelmeyer and Zhongxiang, 1987. However, the cited study obtained flow rates of 0.5 mm day-1 (180 mm day-1) and are thus 3 orders of magnitude smaller. This reference provides evidence that basal motion can operate below freezing point, however, velocities such as the ones reported by the authors (up to 200 m yr-1) require a temperate bed at pressure melting point (e.g. Clarke, 1987, JGR). So there is a contradiction between the simulated basal temperature at the terminus of -4 to -9 deg C and the observation of high velocities associated with basal motion. Significant changes in glacier geometry and the spatial extent of the firn area will eventually influence the basal thermal regime and may switch on or off significant basal motion,

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i.e. significantly change basal friction. This should be discussed.

3 Literature

The authors should consider the results from Moholdt et al., 2012 and discuss the implications for their study. The authors acknowledge Dowdeswell, et al., 2002 for data and reporoduction of figures. However, it is not clear to me if they have actually acquired permisson of reprint from the author and publisher.

4 Model descriptions

The model description is insufficient. The authors do for example not mention that their employed model is a higher order model – or is it full stokes/SIA? Additional components, such as the calving model are barely described at all.

5 Presentation of results

The main results are poorly quantified/presented. The authors better describe their results and not just refer to some result figures, e.g. as for fig. 13 on page 2221, line 1-2.

6 Language

The manuscript is on the whole free of typos, however, the construction of sentences and the choice of words needs to be improved. My list of specific language comments is not exhaustive.

7 Figures

Do the authors have acquired permission of reprint for figs. 1 and 2? The figures are generally clear, however the font size for figs. 3-6 and 11-12 are too small.

Please see the Supplements for further specific comments.

Please also note the supplement to this comment:

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