Review of discussion paper essd-6-2211-2015:
"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", submitted to Earth System Dynamics by Y. V. Konovalov and O. V. Nagornov

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. 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 modelled 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 modelled temperature field should also be reconsidered and the discussion of glaciological processes at the bed adjusted accordingly. How does ice temperature profile and the basal thermal regime, in particular, affect 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 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. Firnwarming may have been negliable in the Little ice age, and consequently, present ice temperatures in the lower part of the glacier may not be affected by it. However, firnwarming 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 oerates) 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, 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.

SPECIFIC COMMENTS

Title: consider removing the second half of the title ": future modeled histories obtained for the reference surface mass balance". In any case, replace "modeled histories" by "modeled evolution" or similar. "History" is not the right word, unless your simulations represent a 500 years spin-up up to the present day!?

P2212, L8-9: How were the ice core temperatures employed to initialize the temperature field. Were not eqs. 4 and 5 used? Where was the core drilled? For what region is the measured temperature profile representative for?

L11: "from the prior investigations" -> "from previous investigations"

L19-21: I do not understand how observations of sea ice can be in agreement with your model results!? They are two different components of the cryosphere that both indicate an imbalance with the current climate. But the changes in sea ice cannot be used to evaluate your model results.

“modelled history”- “modelled evolution” (change this here and at multiple occurrences throughout the text)

P2213, L12: Dowdeswell et al, 2002 report velocities of 70-140 m a^{-1} (not 70-100)

L18: is the employed model a “higher-order” model?

L29: should the friction coefficients not only be spatially variable, but also variable in time, when running prognostic runs over 500 years? See general comment 1.

P2214, L 2-5: are all references needed here? You may restrict yourself to the studies introducing the concept of basal friction inversion and those that report significant advancements in the methodology.

P2214, L 12-14: “Herein, we present… Tikhonov’s regularization method…“. Was this not already included in Konovalov, 2012, or is this new in this study? New to this study are the prognostic runs.

L 26-28: “assessment of maximal ice mass in the ice streams in the future” -> do you mean “a conservative estimate of future glacier retreat and mass loss”?
L 27: “obtained forecasts do not imply a future global warming” -> “do not include/account for future global warming”

..P2215, L 1-2: “results of the prognostic experiments are in agreement with the observations of sea ice…” -> Two different things, cannot be used for validation of one-another. See comment above.

Sec 2: The model generally needs a better description. Higher-order model?

L 16-17 briefly describe the boundary conditions considered in Konovalov, 2012

P2216 How is eq. 2 implemented in the computation of ice flow?

Eq. 3: associate terms of equation with physical process considered: e.g. heat conduction, ice advection and strain heating

P2217, Eq.4 does not reflect the present day situation with warm near-surface temperature in the firn area above 400m (firn warming through refreezing meltwater) and cold near-surface temperatures below

L10: boundary condition at the ice base: does the basal thermal regime affect basal motion? If not, is the thermodynamic coupling restricted to temperature dependency of the rate factor and hence, ice deformation?

L2218 Inversion of friction coefficient: In what way are the observations from the ice core used here? Where does the core originiate from?

L9-10: “difference between simulated and observed temperatures are … small (Fig. 4b)” -> result and fig. from Konovalov, 2012

L16: “temperature history Ts0(t)” -> are you referring to a timeseries of past surface temperatures? Over what timeperiod was the model run? Present-day to 500 years into the future or over the past 500 years to present day? “Modelled present temperature distributions” -> does that mean after a spin-up or steady-state giving the boundary conditions at the surface and base?

P 2219, L1-2: How is the modelled temperature considered? This is not clear to me from eq.2. Or is the modelled temperature changing ice deformations through the rate factor, and hence the residual contribution of basal motion to the overall ice flow, and hence basal friction coefficients?

L 4: If “the distinctions in the friction coefficient are insignificant”, does this mean that ice deformation does not change significantly when the modelled temperature is considered?

L19-20: I do not understand what is meant by “they do not suggest a future mass balance drift down into the ablation zone.”

L23: The present ice surface temperature at the summit of -7,2 deg C is partly due to firnwarming, I suppose. What is the mean annual air temperature at the summit? In the ablation area where firnwarming is absent, the near-surface ice temperature should be similar to the mean annual air temperature, so likely colder than at the summit, despite colder air temperatures.

P2220, L9: please provide the full range of spatial resolution of the irregular grid from the terminus towards the summit

L13 “grounding line retreat…indicator of glacier evolution.” Figs. 8-10 show significant thinning, so the authors could use this as an additional indicator. Apropos significant thinning: I would expect that the terminus, currently fast flowing and therefore at pressure melting, becomes eventually frozen to the ground – ice thickness insufficient to insulate from cold athmosphere and reduced driving stress and strain heating. So basal friction coefficients could change drastically, given the simulated changes in glacier geometry.

L17-18: see last comment above – the changes in basal friction coefficient related to refreezing of the terminus may be larger than that related to retreat of the terminus given a constant basal friction coefficient map.

L21: “Every peak reflects ice calving…” The authors could point out here that there model is able to reproduce the phenomenon known as tidewater glacier instability: rapid retreat into deeper water and a stabilizing effect upon retreat into shallow water.

How does the calving model works? What parameters/variables are considered?

P 2221, L 5-6: “perturbed friction coefficients…horizontal surface velocity is weakly sensitive… ” -> in what range were the basal friction coefficients perturbed? Where they increased or reduced orders of magnitude or just by a few percent?

L23-25: “…inverted x distribution of the friction coefficient” -> Do you mean spatial distribution of coefficients with respect to the x-axis?

P2222 L 5-11: The submelt sliding rates reported by Echelmeyer and Zhongxiang are 3 orders of magnitude smaller than the ones observed/simulated here – see general comment 2.

L12-14: Here the authors state that basal temperatures at the terminus may reach pressure melting, whereas on page 2223, L 11 say state that basal temperatures vary between -4 and -9 deg C!?

L17: the presence of basal water would require a temperate bed

L19-20: “water in the basal layer provides the basal sliding…which with time increase the basal temperature” -> again, this requires a temperate bed. What is meant by basal layer, basal ice, sediment layer or bedrock layer? The temperature of an already temperate bed cannot further increase, only the liquid water content.

P2223, L1-2: “Specifically, the till layer provides the basal sliding” -> the formulation suggests there is evidence for this – is there?

L5 “modelled temperatures in the middle of each cross-section” -> in the vertical and/or horizontal dimension?

L10-14 “basal ice temperatures range -4 to -9 deg C…modeled basal ice temperature justify the sliding due to a layer of ice-laden subglacial drift (Echelmeyer and Zhongxiang, 1987)” -> 3 orders of magnitude lower, see comment above.

L18-19 “not account for the melt refreezing in the subsurface firn…” -> maybe the authors do account for it, in form of the history of surface ice temperature at the summit?

L25: “basal pressure” -> basal water pressure, ice overburden pressure or effective pressure?

P2224, L4 “water content in the basal layer” -> would require temperatures at pressure melting, not the modelled subfreezing tempertaures; what is meant by basal layer?

L8 “where basal ice is frozen to the bed…and where there is basal sliding” -> does the latter mean that it is NOT frozen to the bed, which in turn means it is at pressure melting?

L17 what is meant by steady-state environmental impact, a constant, elevation-dependent surface mass balance?

L19 “ice velocities …decrease …due to diminishing ice thickness” -> and thus deceasing driving stress?

L24-25 retreating outlets and observation of sea ice cannot “agree” - see comment above

P2225, L 2-7 Is the temperature distribution described here a main conclusion of this study? Could be dropped…

L10-11 “changing the physical properties of the bedrock aling the flowlines” -> does this include basal temperature?

L15-16 “The till layer provides the basal sliding.” This may be an explanation. But on what is this statement based on, only the reference to Echelmeyer and Zhongxiang, 1987 and the apparent agreement (actually a mismatch) of sliding rates at sub-freezing temperatures?

L28.. again, remove or reformulate comment about sea ice extent and thickness

Figures

Do the authros have acquired the permissions to reprint figures 1 and 2? The resolution of these figures could be better. Maybe the location of the ice core could be indicated in figure 1?

Fonts of figs. 3-6 and 11-12 are too small.

Fig. 5 The temperature distributions suggest warming at the surface, yet, no firn warming is considered. Do the plots show the initial temperature field obtained at steady-state given bc’s through eqts. 4 & 5?

Fig.7 The mass balance unit is m w.e. a^{-1}, i.e. “water equivalent”, I suppose?