Author's final comments (continuation)

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T. Dunse: *"The authors should consider the results from Moholdt et al., 2012 and discuss the implications for their study."*

Anonymous Referee #2: *"page 2224, bottom: the modeled histories are in agreement with the observations of sea ice extent and thickness in indicating a continual ice decline in the Arctic. Several issues: (a) there should be references here, (b) since present-day climatic conditions are used in the forward integration, it is difficult to make clear predictions about the evolution of the ice streams and the ice cap in general, and the only statement the authors could possibly derive from their study is that the current geometries are not in equilibrium, (c) the same sentence is used 3 times in the manuscript: at the end of the abstract, here, and at the end of the paper."*

Corrected. Corresponding paragraphs that contain the references to the results from Moholdt et al., 2012 have been included to the text.

T. Dunse: *"Do the authors 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?"*

The authors acquired the permission to reprint the figures yet for publication of the manuscript in 2012 (Konovalov, 2012). Prof. J. A. Dowdeswell have kindly permitted me to use the figures of (Dowdeswell et al., 2002) in my manuscripts. I reprinted the figures that were available as open access supplementary materials to the manuscript Dowdeswell et al., 2002. Thus, the available quality/resolution was provided by the publisher.

Anonymous Referee #2: *"In the model description it is not stated where the basal topography comes from, in particular whether this is simultaneously inverted for or taken from somewhere else. This information is only given in the caption of figure 3, but it should be included in section 2."*

The basal topography was reconstructed from the figure 8 of Dowdeswell et al.(2002).

T. Dunse: *"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."*

Anonymous Referee #2: *"The authors say they include a rectangular ice shelf geometry, presumably thisis rectangular in the x–y? If buttressing is to be included in this flow line model – and it is not clear to me at all how this is done, as ice shelves can generally beexcluded in flow line models (e.g., Schoof, 2007) – then these shelves should be able to evolve according to a MacAyeal type model (MacAyeal, 1989) and both a calving law and the force balance at the calving front have to be considered. If buttressing is included through some parameterization, then those details need to be given as well. The case of buttressing also ties back to the argument made above about the applicability of the model: buttressing is an important factor in stabilizing grounding line motion and calculating the force balance of a tidewater glacier, hence those missing physics could substantially change the results of the paper."*

We suppose that the calving processes at long-time scales can be described by a stochastic model, which considers the size of the debris as a random value. And this value satisfies a probability distribution law, for instance, likewise the Gaussian distribution.

In fact, we considered the simplest probability distribution, i.e. when the debris of equal length occur at each calving. Thus, the length of ice debris is the parameter, which, in particular, corresponds the average length in a probability distribution law.

Moreover, in this model the both ice-shelf length and ice-shelf thickness at the terminus are considered as the variables that should satisfy a certain conditions. If the ice-shelf length exceeds a value l_{cr} (the parameter of the model) or the ice-shelf thickness beside the terminus becomes smaller than a value H_{cr}, then the calving of the appropriate part of ice occurs in the model.

To investigate the impact of the parameters on the results of the modeling, the parameters were varied in a series of the experiments. However, the simulation reveals that at long-time scales the mass balance, friction coefficient, ice temperature have the main impact to the assessment of the grounding line retreat derived by the modeling.

The corresponding paragraphs have been included into the text.

Anonymous Referee #2: *"Fig. 6b and 6c show an increase in basal friction after 30 km – what do the authors think is the origin of this peak?"*

Possibly, the water content in the bedrock/till layer varies with the bed elevation changes, and the enhancement of water content at lower elevations provides a decrease in the friction coefficient in the corresponding areas (i.e. viscosity of the till layer depends on the water content). Another suggestion is that the variations in basal friction are provided by the variations in the till layer thickness.

Anonymous Referee #2: *"There are a number of oddly constructed sentences and incorrectly used words, and the manuscript should be edited by a native speaker to be acceptable for publication."*

The manuscript has been edited by a native English speaker editor.

T. Dunse: *"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!?"*

Title has been corrected.

T. Dunse: *"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)"*

The paragraph has been replaced with the new one.

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

Corrected.

T. Dunse: *"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."*

The paragraph has been corrected.

T. Dunse: *"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" "

The paragraph has been corrected.

T. Dunse: *"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."*

Corrected.

T. Dunse: *"L 16-17 briefly describe the boundary conditions considered in Konovalov, 2012"*

Corrected.

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

Done.

T. Dunse: *"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"*

Corrected. (The comment has been added to the text)

T. Dunse: *"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"*

Corrected. (The comment has been added to the text)

T. Dunse: *"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?"*

Essentially, in this model the basal thermal regime affects basal motion and basal ice deformation due to temperature dependency of the rate factor.

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

In the paragraphs at the page L2218 the authors imply that the ice temperature, which is considered in the friction coefficient inversions and which affects the ice flow velocities, was obtained employing the inverted ice surface temperature (and the elevational temperature gradient). Preliminarily, the inverted temperature was obtained from the borehole observations (in particular, from the borehole ice temperature measurements). The borehole was drilled beside the ice cap summit: 80.50 N, 94.83 E (Zagorodnov, 1988; Arkhipov,1999).

T. Dunse: *"L9-10: "difference between simulated and observed temperatures are ... small (Fig. 4b)" -> result and fig. from Konovalov, 2012"*

I have decided to include the figure in the manuscript for convenience (likewise the figures 1 and 2).

T. Dunse: *"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?"*

Corrected. (The comment has been added to the text)

T. Dunse: *"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?"*

In fact, the modelled temperature affects ice deformations through the rate factor and, hence, affects the overall ice flow and the modelled ice flow surface velocities that are included in Eq. (2) and, hence, it affects to the friction coefficient inversions. The modeled temperature is defined from Eq. (3)..(5) (the text have been corrected). However, the temperature modelling does not account for the ice thickness changes in the past.

T. Dunse: *"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?"*

I believe that it would be better to say that the initial ice temperatures were "successfully" chosen so that the friction coefficients inverted for the initial and the modelled temperatures don't reveal significant distinctions… Nevertheless, the deformations can locally differ more significantly one from another. However, it seems that the depth-averaged deformations for the two temperatures in the ice streams are close one to another.

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

The paragraph has been corrected.

T. Dunse: *"P2220, L9: please provide the full range of spatial resolution of the irregular grid from the terminus towards the summit"*

Corrected.

T. Dunse: *"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."*

Corrected. The new paragraph have been included into the text.

T. Dunse: *"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?"*

The range of the perturbations is shown in Fig. 4 of Konovalov (2012). In particular, the perturbations with amplitudes of order of the coefficient magnitude were considered.

T. Dunse: *"L23-25: "…inverted x distribution of the friction coefficient" -> Do you mean spatial distribution of coefficients with respect to the x-axis?"*

Corrected.

T. Dunse: *"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."

Anonymous Referee #2: *"page 2222, line 7-11: Echelmeyer and Zhongxiang (1987) measured sliding rates of 0.5 mm day^{-1} over a solid rock surface at a temperature of -4.6^{o}C under a shear stress of approximately 60 kPa. This sliding rate can provide the ice surface velocity of about 180 - 200 m a^{-1}, and thus can explain the fast-flowing ice streams. This statement is wrong on multiple levels: first of all, 0.5 mm day^{-1} corresponds to 0.2 m year^{-1}, which is several orders of magnitude too small. Second, in general the fast flow of ice streams is associated with the existence of water at the bed (see for example Blankenship et al. (1986); Iken et al. (1993); Engelhardt and Kamb (1997) for ice streams in ice sheets and Meier and Post (1987) for fast-flowing tidewater glaciers). It would be very unusual to have such fast flows associated with a sub-temperate bed. If this finding is robust, it should be discussed in detail."*

The paragraphs relating to the explanation of the basal sliding in agreement with the report by Echelmeyer and Zhongxiang (1987) have been deleted.

Anonymous Referee #2: *"page 2222, line 24/25: the large values of the friction coefficient at 0 km < x <20 km justify the rock-type bottom where the ice is frozen to the bed. A rock type bottom seems unlikely given that most of the bed is below sea level (see figure 3), suggesting that marine sediments are more likely (see also Dowdeswell et al. (2002)).*
page 2222, line 26-28: The lower values of the friction coefficient at 2 km < x <40 km presumably indicate the existence of the till layer at the bottom. Several later studies than those referenced after this sentence have found till to behave plastically, rather than viscously (e.g., Iverson et al., 1998; Tulaczyk et al.,2000). Dowdeswell et al. (2002) also emphasize that basal motion of the fast outlet glaciers considered here is likely facilitated by marine sediments. Before this background the authors should include a comment on their choice of a viscous sliding law rather than a probably more realistic plastic sliding law. Better yet, they should consider using a plastic sliding law instead."

The paragraph has been corrected… The authors mostly relay on the mathematical considerations in the choice of the friction law. In particular, the modelled velocities are in a good agreement with the observed velocities for the linear friction law. Moreover, the linear friction law facilitates further simulations including the prognostic experiments. However, the authors agree that the non-linear friction law is more likely. And, respectively, the complementary modeling with the non-linear friction law was also carried out (I mean the modeling, which is still in the supplementary materials including the authors response).

T. Dunse: *"L5 "modelled temperatures in the middle of each cross-section" -> in the vertical and/or horizontal dimension?"*

Corrected.

T. Dunse: *"L25: "basal pressure" -> basal water pressure, ice overburden pressure or effective pressure?"*

Corrected.

T. Dunse: *"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?"*

Corrected.

T. Dunse: *"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?"*

Corrected.

T. Dunse: *"L17 what is meant by steady-state environmental impact, a constant, elevation-dependent surface mass balance?"*

Corrected.

T. Dunse: *"L19 "ice velocities …decrease …due to diminishing ice thickness" -> and thus deceasing driving stress?"*

Corrected.

T. Dunse: *"P2225, L 2-7 Is the temperature distribution described here a main conclusion of this study? Could be dropped…"*

The temperature distributions were obtained in this study (i.e. the results were not presented in the previous study (Konovalov, 2012)). Thus, the results are concisely reflected in the conclusions. However, the temperature distributions are not a main conclusion of this study.

T. Dunse: *"Fonts of figs. 3-6 and 11-12 are too small."*

Corrected.

T. Dunse: *"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?"*

The plots show the temperature field obtained at not steady-state conditions. In particular, Eq. (4) contains $T_{s0}(t)$, which is the inverted from the borehole measurements ice surface temperature. Thus, $T_{s0}(t)$ is the time-dependent temperature which reflects the past climatic changes.

T. Dunse: *"Fig.7 The mass balance unit is m w.e. a^{-1}, i.e. "water equivalent", I suppose?"*

Corrected.

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Thanks and all the best,

Yuri V. K.