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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

Anonymous Referee #2

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General comments

In this paper, inversions for a spatially variable basal friction coefficient along 3 flow-lines of outlet glaciers on the Academy of Sciences Ice Cap are performed. Assuming constant present-day boundary conditions, the results are then used to integrate the model 500 years forward in time. This effectively corresponds to a relaxation towards

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the steady state that would result from present-day conditions, although the authors do not state this, and steady states are not yet attained at the end of their calculations. The authors find significant reductions in the volume and extent of the grounded ice at the end of the model period.

Inversions for basal shear stress and projections of glacier/ice sheet evolution have been performed with more sophisticated models in other publications (Winkelmann et al., 2012; Sergienko and Hindmarsh, 2013; Clarke et al., 2015), and the main novelty of this publication is the application to the Academy of Sciences Ice Cap. Due to its specific nature, this study seems more appropriate for the Cryosphere than for ESD. However, there are major issues with the model setup and in the presentation of the paper, which have to be addressed before publication in either journal can be considered, and I recommend rejection of the paper in its current form. A significantly revised version of this article could be resubmitted to the Cryosphere. The most important issues are listed below, and some more specific comments follow under ‘specific comments’. Given the need for a significant revision of the manuscript, I have refrained from including technical corrections.

- The model as stated is incomplete and some of the symbols that are used are not defined. The worst example of this is that the authors invert for a friction coefficient that is not defined anywhere in the paper, nor is there an equation that describes how the basal friction coefficient enters into the ice flow model. Instead, the reader is referred to Konovalov (2012), but even from this paper I could not extract the exact form of the friction law used in the paper under consideration, nor the calving law, or the grounding line boundary conditions. On page 2216, line 14 it is stated that ‘a linear (viscous) friction law’ is used, which presumably is supposed to mean that a friction law of the form

$$\sigma_{xz,b} = \gamma u_b \quad (1)$$

is used, with γ the friction parameter, u the basal value of the along-flow velocity

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and $\sigma_{xz,b}$ the basal shear stress?

It is essential to state the full forward model in a complete but succinct manner in order to fully review the scientific merit of the manuscript.

- The ice flow model is a 2-dimensional flow line model, which is not really appropriate for a small outlet glacier, in which all stress components are potentially important. Zhang et al. (2015) find that these kinds of higher order flow line models show high discrepancies to full Stokes models when sliding is important and that these differences increase over long time integrations. This casts doubts on the applicability of the model used here, which introduces the additional complexity of grounding line migration.
- Grounding line dynamics: the retreat of the outlet glaciers' (I prefer this terminology over ice streams, which are generally associated with ice sheets) grounding lines by several kilometres is an important finding of the authors, but no details of how this grounding line motion is modelled are presented. In recent years publications have emerged which considered the performance of different models and implementations for modelling grounding line motion (e.g., Pattyn et al., 2012), but these are insufficiently referenced and it is not clear whether any of these findings are taken into account.
- The authors say they include a rectangular ice shelf geometry, presumably this is 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 be excluded 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

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- 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.
- The model does not take any inflow of mass from the regions surrounding the ice streams into account, which makes it unsurprising that the authors find that their equilibrium configuration is much smaller than their current state: most ice streams drain an area larger than just their surface area, consider for instance Jacobshavn Isbræ (Joughin et al., 2008). If inflow of ice from these regions is not taken into account, one would naturally end up with an overly negative mass balance. A full understanding of the evolution of the ice cap and its outlet glaciers would hence require modelling the entire ice cap.
 - The authors use a constant present-day mass balance, while state-of-the-art modelling involves the use of projected changes in climatic conditions (e.g., Winkelmann et al., 2012; Clarke et al., 2015). This would give the paper significantly more substance and allow to interpret the results in context of the presently observed changes in climate.
 - 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.

Specific comments

- 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.

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- The authors appear to put a lot of emphasis on the fact that they applied a Tikhonov regularization, but this appears to be fairly common in inversions for basal shear stress (e.g., Sergienko and Hindmarsh, 2013), and this related work should be cited.
- It is assumed that the basal conditions do not change over time, although the geometry of the outlet glaciers change substantially, which likely affects the temperatures in the ice and at the bed and also the sliding law. The authors should discuss their assumption and which changes in basal shear stress they would anticipate in a more realistic model.
- 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°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.
- page 2220, line 21: *Every peak reflects ice calving at the ice-shelf terminus. [...] For convenience, the periodic calving of equal-size debris is considered herein, i.e., δ -function is considered as the frequency distribution function.* This is the first time that calving is mentioned in this manuscript – at the end of the results section! If the model includes calving (for concerns about this ‘if’ see above),

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then its mathematical description should be given in section 2. The mentioned calving law is also inconsistent with the statement on p. 2217, line 23/24: [...] *ice thicknesses in the ice shelf along the flow line attain a constant value at the terminus*. Lastly, one would also hope that the choice of calving law is guided by physics rather than convenience!

- page 2222, line 26-28: *The lower values of the friction coefficient at $2 \text{ km} < x < 40 \text{ km}$ 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.
- page 2222, line 24/25: *the large values of the friction coefficient at $0 \text{ km} < x < 20 \text{ km}$ 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 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.

The statement should be rephrased and not just repeated.

- Fig. 6b and 6c show an increase in basal friction after 30 km – what do the authors think is the origin of this peak?
- Fig. 11 suggests that the model is not evolved towards a steady state and that there are still significant changes in the glacier geometries. I would like the authors to comment on why they chose a 500 year time frame for the forwards integration.

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