Interactive response to reviewer comments on "Simulating ice thickness and velocity evolution of Upernavik Isstrøm 1849–2012 by forcing prescribed terminus positions in ISSM"

by Konstanze Haubner and co-authors

We thank both reviewers for their constructive comments on our manuscript. We feel the requested changes have improved the clarity of the paper and appreciate their feedback. The author response to reviewers is structured as follows:

– Reviewers' comments in blue

– Authors' response in black

We significantly rewrote some sections of the manuscript, to improve clarity. The major changes include:

– Model evaluation includes now comparisons of observed and simulated ice thickness instead of surface elevation

– Additional figures in the supplementary showing the basal friction coefficient and spatial comparisons for simulated and observed ice thickness, velocity

– New table visualising model initialisation steps

– Including the co-authors Eric Rignot and Todd K. Dupont (TC editorial support (Svenja Lange) is informed)

Our conclusions remain unchanged. The reviewed manuscript with tracked changes is attached.

Referee #2 (J. Bassis)

General comments

The goal of this study is to simulate the dynamic response of the Upervnavik Isstrom glacier system from 1849-2012 to prescribed terminus changes combined with changes in surface mass balance. The authors use the ISSM model approximation to the well known shallow shelf approximation (SSA) equations. The authors find that prescribed changes in terminus position have a large effect on dynamic discharge, reinforcing many prior studies that came to similar findings. Overall, one of the strengths of this study is that it is able to simulate dynamic drawdown over a relatively long period of time. I did, however, have some questions.

1. I am a bit confused by the geometry of the glacier system. It says in several places that the grounding line is computed automatically by the model. I guess this means that the groundling line (transition from grounded ice to floating ice) is computed automatically by the model, but the terminus position or calving front position is floating and this position is specified? The

existence of an ice tongue or shelf is not obvious at all from the discussion or figures: does the glacier always have a floating tongue or is the floating tongue only there part of the time. After reading through a few more times, I started to think that there is no ice tongue and the terminus is grounded, consistent with this system being a tidewater glacier system, but then there isn't a grounding line. Overall, I would like the authors to be a bit more careful with their explanation of whether the glacier has an actual grounding line and if the grounding line evolves separate from the calving front or whether there is really a calving front, which may sometimes approach floatation or something else.

We agree that the text was not very clear: the ice front position is prescribed, and ice is allowed to float depending on a hydrostatic criterion (Seroussi et al., 2014). Most of the time, the calving front is grounded (see video01, supplementary). Though, UI-3 shows an evolving floating tongue between 1900 and 1951.

In order to improve the understanding of the grounding line integration, we changed the manuscript in the following way: We removed grounding line in the abstract to take away to focus of it being a product of our method. We added the following sentences in section 3 and 3.2:

"The ice is allowed to float depending on a hydrostatic criterion (Seroussi et al., 2014)."

"Within the prescribed ice area, the grounding line is evolving freely and floating tongue formation is thereby allowed."

2. The model description could use a bit more detail. As far as I can tell the authors are using the SSA approximation as implemented in ISSM and inverting for basal friction to best match observed velocities. This is acceptable, but the authors should also tell us which sliding law was used. Back in the old days, models used to use a sliding law with friction proportional to velocity (Newtonian) because it was easy to implement. Now we know that the sliding law is rarely Newtonian, but some prefer a plastic bed, Coulomb plastic, Weertman or some combination of the three. For reasons related to my next point, it is important to provide the sliding law. The authors should also probably provide a map of the inferred basal friction parameter.

To clarify the used sliding law we include the tex below in section 3 and Figure 1 into the supplementary:

A Coulomb-like friction law is applied on grounded ice:

$$\tau_b = -C^2 N v_b \qquad (1)$$

where v_b is the basal velocity, N the effective pressure on the glacier base and C is the friction coefficient (Fig 1, supplementary). Friction is not applied on floating ice.

3. As I understand it, the authors apply a prescribed surface mass balance along with prescribed changes in terminus position to simulate changes to the dynamics of the glacier system. Moreover, the authors use an inverse method to invert for the basal friction coefficients in their sliding law. Now ice dynamics models are based on approximations to the Stokes equations. A consequence of this is that if the geometry is appropriately specified and the boundary conditions are all correctly specified the velocity is completely determined. Because the authors are using observed velocities to tune the friction coefficients and prescribing changes in terminus position, it isn't all that surprising that they can simulate the correct dynamic response. In fact, it would be surprising if the model failed given the tuning step.

We indeed do constrain the ice margin and assume that the friction coefficient C does not change during the simulation, but the surface is allowed to change freely. We do not constrain the driving stress, which controls the ice dynamics. The fit between

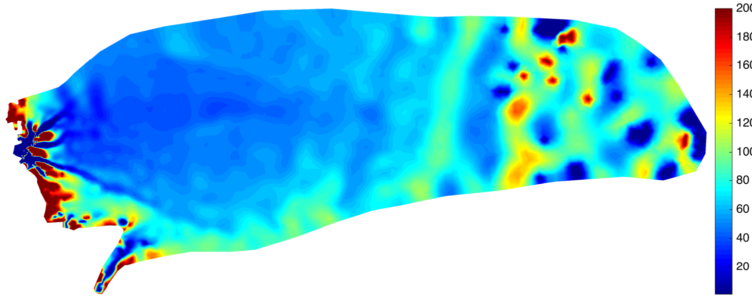

Figure 1. Inverted basal friction coefficient

the observations and the model was therefore not necessarily expected since we perform our inversion reconstructed velocities from a first relaxation.

We re-formulate the description in the manuscript: "Thus, in the first relaxation basal friction is based on the assumption that driving stress is equal to basal stress at any given point using the initial geometry." and add Table 1 to give a better overview about the model initialisation steps.

Table 1. Steps for model initialisation

Step	Input	Output
Relaxation 1	GIMP extended to 1849 terminus position, Ice viscosity (initial guess), Basal friction (initial guess)	Reconstructed 1849 ice thickness and velocity
Thermal	Ice thickness and velocity from relaxation 1	Improved ice viscosity
Inversion	Surface velocity from relaxation 1, Ice viscosity from thermal	Inverted basal friction
Relaxation 2	Ice thickness from relaxation 1, Ice viscosity from thermal, Basal friction from inversion	Steady state ice thickness and velocity

What is surprising and impressive is that authors are able to get the correct dynamic response over a fairly long time interval. This seems like it is probably dictated, at least in part, by the choice of sliding law - which is why I think it is important to specify the sliding law and show us its pattern of spatial variation. Also, are the model results sensitive to the form or magnitude of the sliding coefficient. For example, do you get similar results for plastic, Coulomb and Weertman type sliding laws? How sensitive are the results to the inversion? Is the good agreement a consequence of extensive model tuning or relatively insensitive to model tuning? Related to this, the authors need to be careful when comparing observations of velocity with simulated velocity. Good agreement means the inversion was able to match surface velocities, but tells us nothing about the models skill.

As explained above, we apply a Coulomb-like friction law and invert for steady state velocity, not for observed ice surface velocity.

The control run that applies only surface mass balance to the model does not show the same pattern of acceleration. The prescribed terminus position changes lead to increasing ice flow. Previous simulations to this study did not include ice viscosity retrieved from a thermal solution and others where initialised with a previous bed topography version (Morlighem et al., 2014). The resulting velocity changes were the same. We conclude that the simulation results are not too sensitive to the inverted friction.

4. Related to this, I'm a bit confused by the metrics for model success. It seems to me as though the authors are comparing observations of mass balance to simulations of mass balance. However, surface mass balance is prescribed and changes in terminus position are prescribed so the only part of this that can vary is the increased dynamic discharge. Why not just compare simulated change in dynamic discharge predicted by their model with that inferred from observations. For example, Figure 2 shows annual mass loss along with change in mass loss from prescribed changes in terminus position. What about also showing mass loss from prescribed surface mass balance? Then we would clearly see the component that is predicted (dynamic discharge) and what is specified.

Good idea. We modify Figure 2b to visualize the portion of prescribed mass changes to simulated total mass change and add information about mass loss contributors to the text.

The more I think about it, it seems as though the observations probably give changes in trim line (or something like this) so maybe the right comparison is between predicted glacier surface elevation and observed trim lines (as opposed to ice thickness)? (I apologize to the authors if I misunderstood their data or comparisons).

Observations provide ice surface elevation from aerial photography and satellite. We only have trim line data for the little ice age extent. Following advice from Referee #1 we compare ice thickness results to observations instead of ice surface elevation.

As I said before, I also don't think that the authors can claim that the match between measured velocities and observed velocities provides any test of the model. Friction has been determined by tuning the model to match observed velocities so any match between observed and simulated velocities is partly a consequence of the tuning procedure. Here I think the authors might be able to narrate to readers a bit more thoroughly what they actually measured and what they predicted (without prescribing) and how the measurements can be used to test the things that the model predicted that weren't ingested in any tuning exercises.

As explained above, we invert for the relaxed velocity. Therefore, "tuning" is done for the initial state of the simulation, not for observed present day velocity. Furthermore, we compare observations to results after 160 years of simulation.

Some miscellaneous comments

Page line 13 extra space before

We could not find what you are referring to.

Don't capitalize Grounding line or Basal friction

Done.

Page 3 below 5: The ice temperature is determine by solving an advection-diffusion equation. The paper says the temperature field was initialized using surface air temperature. Does this mean the ice temperature was run to steady-state using the assumed surface air temperature?

We have divided up the explanation for ice viscosity, and have moved some parts of the explanation to section 3.1, Model Initialisation. The first part remains in the Introduction section in section 3:

"Ice viscosity follows Glen's Flow law (Glen, 1955). The initial viscosity is taken from Table 3.4 in Cuffey and Paterson (2010, p. 75), assuming ice temperature of $-5°$C and will be refined in section 3.1"

Further explanation is given in section 3.1 Model Initialisation:

"Given computed ice velocity and thickness from the first relaxation, ice viscosity and basal friction can be redefined. The ice viscosity is calculated by extruding the model with 15 layers and solving for the thermal steady state based on forcing the surface with 1854–1900 UI mean surface air temperature (Box, 2013)."

Page 3, line 10: The grounding line position is automatically calculated in each step implies that the terminus is at flotation. Why is this a good approximation and why is it forbidden for the terminus to have a thickness greater than flotation?

As explained above, the model includes a grounding line migration scheme. During the simulation, the glacier can evolve a terminus at flotation, but it is not forced to float. In most cases for our simulation, the grounding line is located directly at the terminus position.

I found a few other grammar mistakes throughout and I would urge the authors to give the manuscript one more proof read.

We improved the study regarding grammar and spelling.

References

Box, J. E.: Greenland ice sheet mass balance reconstruction. Part II: Surface mass balance (1840-2010), Journal of Climate, 26, doi:10.1175/JCLI-D-12-00518.1, 2013.

Cuffey, K. and Paterson, W.: The Physics of Glaciers, Elsevier Science, 2010.

Glen, J. W.: The creep of polycrystalline ice, Proceedings of the Royal Society of London A: Mathematical, Physical and Engineering Sciences, 228, 519–538, doi:10.1098/rspa.1955.0066, 1955.

Morlighem, M., Rignot, E., Mouginot, J., Seroussi, H., and Larour, E.: Deeply incised submarine glacial valleys beneath the Greenland ice sheet, Nature Geoscience, 7, 418–422, doi:10.1038/ngeo2167, 2014.

Seroussi, H., Morlighem, M., Larour, E., Rignot, E., and Khazendar, A.: Hydrostatic grounding line parameterization in ice sheet models, The Cryosphere Discussions, 8, 3335–3365, doi:10.5194/tcd-8-3335-2014, 2014.