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 #1 (anonymous)

General comments

This study simulates the dynamic response of the 3 main Upernavik Isstrom glaciers to prescribed changes in terminus position and surface mass balance, both from observations, over the period 1849–2012. The authors model ice dynamics using SSA in ISSM and make use of inverse methods to initialise basal conditions. The results of the simulation indicate the importance of terminus position change in driving dynamic change in this glacier system. Overall this paper presents interesting results, but I have significant concerns with regards to the interpretation of the results and the presentation of the methods which lead me to believe it is not yet ready for publication.

This study's approach allows the authors to investigate dynamic response to calving, while circumventing the issue of calving law uncertainty. However, the nature of this approach is such that a large amount of the mass balance change comes from prescribed model inputs. This is not a problem in itself, but the authors have not untangled this model input from model output (i.e. dynamic response) when presenting comparisons with observations. For example, in Section 4.2 and in the supplementary material, the authors show comparisons between modelled and observed mass loss. However, much of this mass loss is actually prescribed through terminus retreat and surface mass balance. It should be trivial to subtract these prescribed components from both the simulated and observed MB. Without this correction the comparison is somewhat misleading, and makes it impossible to assess the performance of the model.

In our manuscript, we divide the simulated mass changes into mass change due to prescribed SMB and dynamic component DIL. We agree that a division of ΔDIL into mass change prescribed by terminus position change and the simulated dynamical mass change improves assessing model performance. Figure 2b is adapted accordingly. We gain knowledge about the contributions to mass loss from prescribed SMB, terminus change and resulting thinning and acceleration: "[...] while 30 % of total ice mass loss simulated by $\mathrm{ISSM_{PT}}$ was prescribed, with ΔSMB accounting for 9 % (-50 Gt) and prescribed terminus position change contributed 21 % (-121 Gt). Thus, 70 % of by $\mathrm{ISSM_{PT}}$ simulated mass loss is caused by thinning and acceleration."

However, the choice of dividing the mass loss into ΔSMB and ΔDIL is motivated by the cited publications Khan et al. (2013) and Larsen et al. (2016). They compute mass change by comparing Upernavik's ice surface elevation in different periods, compute ΔSMB between the dates of the surface elevations and compute ΔDIL as residual of computed mass change and SMB. By comparing our simulated ΔDIL with those observations, we find it important to use the same measure and compute prescribed ΔSMB and simulated mass changes on the domains observed in Khan et al. (2013) and Larsen et al. (2016). We also use the same term: dynamic ice loss.

We include explanations about the background of the mass comparison with Khan et al. (2013) and Larsen et al. (2016) and re-write the subsection 4.2.

My most serious concern with this manuscript is the claim made in the abstract and in the text that the model matches observations within 20%. In the abstract, the claim applies to the surface elevation and velocity over the period 1990–2012, while in the conclusions, the authors seem to claim that the entire 164 year simulation matches observations within 20%. From the data provided in Figures 4 and 5, and in Sections 4.3 and 4.4, neither claim appears to be accurate. This might be simply fixed by qualifying the statements somewhat, but it leads me to question the accuracy of the other (currently unverifiable) claims about the match between model and observation. I would like to see additional figures showing the mismatch in elevation and velocity across the domain to back up the claims made in Sections 4.3 and 4.4.

We remove claims similar to simulation matching $\pm$20% observations and change all evaluations of simulated ice surface elevation to ice thickness. We add figures to the supplementary, showing spatial absolute and relative differenced between simulated and observed ice thickness.

Comparing simulated and observed ice thickness changes the percentage of accuracy. We adapt the text accordingly.

The description of the model setup, physics, boundary conditions and initialisation is somewhat unclear and significantly lacking in detail. What does the model domain look like? Is it defined by the ice catchment? Does it extend to the ice divide?

The model domain is defined by the ice catchment and extends to the ice domain, marked as a red area Figure 1. The following sentence is now included in the model description: "The model domain is set to the Upernavik catchment, which is defined by the flow direction given by the 2008/09 surface velocity from Rignot and Mouginot (2012) (red area in Fig. **??**)."

What velocity data are used to invert for basal friction? What happens to the basal friction condition when flotation is achieved?

Velocity gained from relaxation is used to invert for the basal friction coefficient. Now, Table 2 provides more information about the model initialization steps. The section describing model initialisation is re-written to improve clarity.

Friction is not applied on floating areas.

In comparing surface elevations, the authors state that the surface lies within 20% of observations, and similar percentage comparisons of surface elevation are made throughout the results section (e.g. 84% surface lowering at UI-2 2012 terminus). This should be restated in terms of ice thickness, which is altitude-agnostic and which, after all, is the variable of interest from an ice dynamics perspective. A 20% error/change in surface elevation translates into quite different thickness errors/changes depending on whether the ice is floating or resting on bedrock at 500 m.a.s.l.

As explained above, we change surface elevation comparisons to thickness comparisons and adapt all numbers accordingly.

I found quite a few grammar/language errors, some of which I have highlighted in ‘technical corrections’ below.

We improved the study regarding grammar and spelling.

Specific Comments

P1 L3: make it clearer that you prescribe changing terminus position. “Observed glacier terminus changes” could be e.g. oceanic or atmospheric conditions.

Sentence has changed during revision.

P1 L5: I think you used 2012 velocities to invert for basal drag (though I’m not sure), and terminus positions (and SMB) are prescribed. As such, I don’t think a <20% error in elevation and velocity at the end of your simulation would necessarily imply that your model is realistic from 1849-2012. It would tell you that your basal inversion worked properly. But more importantly, this is not accurate! For example, Fig 4 shows UI-1 observed surface elevation in 2009 at 5–10km of over 500 m.a.s.l., but modelled is less than 400 m.a.s.l. Fig 5 shows UI-2 0-5km 08/09 observed velocity is just under 2500m/a, but modelled is over 3000 m/a. You explicitly state in the text that simulated 2012 upstream surface elevation is 56-62% of that observed. And these are data averaged over a large area. In the shear margins, you mismatch by 100%. This in itself is not a problem – shear margins are tricky, but you cannot claim that you match elevation and velocity within 20%.

We agree, the statement is too strong and formulated misleadingly. Conclusions and abstract are revised and focus now on different mass loss periods, the different contributors and how PT could be used to improve ice sheet models.

P2 L12: What is a “dynamic ice loss event”? In the context of Kjaer et al (2012), it seems to be a multi-year period of sustained accelerated calving. You should clarify this.

We re-phrase to "periods of increased dynamically driven ice loss".

P2 L14: The final sentence of this paragraph feels out of place. Perhaps move it to the start of the next paragraph. “Hence” here implies that the focus of previous studies is a result of the two dynamic mass loss events.

We moved the sentence to the next paragraph and start the sentence with "Previous studies [...]"

P3 Fig 1: This is a good figure, but the poor contrast in the landsat image between rock and ocean makes it slightly tricky to pick out the historic positions of the individual glaciers. Perhaps you could tweak the bands a little?

We changed the color bands and contrast aiming for better rock-ocean contrast.

P3 L4,5: This sentence is quite unclear. It starts by describing SSA (approximation for stokes, long. stress), but “neglecting lateral drag” is not a fundamental part of SSA. I guess you mean that you choose to neglect lateral drag on the sides of your domain? Given the width of the domain, this is quite justifiable, but explain it better and give this justification. I also think you could give a more technical and less clunky description of longitudinal stress gradients.

We re-write the paragraph to: "Ice flow is calculated applying the Shelfy Stream Approximation (SSA; MacAyeal, 1989), that integrates vertically averaged ice properties (e.g. ice rheology, thickness, velocity) and neglects vertical shear stresses. The SSA is well suited for fast-flowing glaciers like Upernavik, where the ice flow is primarily driven by basal sliding."

P3 L10: Can you show, or at least properly describe, the domain somewhere?

The model domain is the UI catchment and shown in Figure 1. The figure caption now contains information about the model domain and the following sentence is added to the model description:

"The model domain is set to the Upernavik catchment, which is defined by the flow direction given by the 2008/09 surface velocity from Rignot and Mouginot (2012) (red area in Fig. 1)."

P3 L6: Why use surface air temperature for depth integrated viscosity? Is there any reason to think that surface air temp is equal to, or even correlates with, internal temperature?

We divided the explanation for ice viscosity, and 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)."

P4 L8: Can you give more details on the extrapolation of velocity? Is this done using a mass conservation approach? I assume that is what is meant by “following fjord bathymetry”? If so, were changes in glacier width also accounted for?

We re-phrased the paragraph to clarify the initialisation steps and added Table 2 to give an overview, which steps are performed including their goals.

The initial velocity is derived from stress balance solution, given GIMP surface elevation extended to the 1849 terminus as described in section 3.1. For the initial ice velocity, we asume driving stress to be equal to basal stress at any given point.

P5 L7: It took me some time to figure out your strategy here, but now I see that your interpolated surface elevation and bathymetry tells you whether the ice should be floating or grounded, and therefore gives you a thickness. Maybe you could clarify this?

We reduce the explanation to "The ice thickness is set to floatation height or to the maximum thickness, defined through the initialised ice surface elevation and bed topography. "

P5 L19: What about floating regions? I guess driving stress is small (but non-zero) here.

Friction is not applied on floating regions. The driving stress is small.

P5 L23: Authors state “the first relaxation... provides ice thickness and velocity for the second relaxation. Given computed ice velocity from the first relaxation, basal friction can be redefined”. So, is the inversion done with respect to observed velocity or simulated velocity from the previous relaxation? I guess the former, in which case you should clarify the above statements; given the instantaneousness of the stokes equations, I don’t think the velocity from the first relaxation really feeds into the second relaxation at all, except perhaps to provide the initial guess for viscosity in your first iteration. If the latter, this feels questionable – using velocity from SIA basal drag in SSA model to invert for new basal drag...

We invert for previously relaxed surface velocity after improving the ice viscosity by obtaining a thermal steady state.

New inversion description: "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). The basal friction coefficient is constant in time, but varies in space, and is calculated by an adjoint-based inversion, following Morlighem et al. (2010) and MacAyeal (1993), given the updated ice viscosity from the thermal steady state simulation."

P6 L9: If you want to show relative changes, you should be looking at thickness, as mentioned above.

Done. (see answer above)

P7 L1: How much of this -585 Gt was prescribed?

We added "99 % of simulated $\mathrm{ISSM_{control}}$ mass loss was prescribed by ΔSMB while 30 % of total ice mass loss simulated by $\mathrm{ISSM_{PT}}$ was prescribed, with ΔSMB accounting for 9 % (-50 Gt) and prescribed terminus position change contributed 21 % (-121 Gt). Thus, 70 % of by $\mathrm{ISSM_{PT}}$ simulated mass loss is caused by thinning and acceleration."

P7 L8: “hereafter anomalies deltaSMB and deltaDIL” - I see what you mean, but this isn’t a sentence.

Now: "hereafter referred to as anomalies ΔSMB and ΔDIL"

P7 L16-21: This paragraph and associated table are not very intuitive and could be improved. “2002/05 – 2010” should be clarified in the text – it’s not clear what this range represents. The authors state that mass balance corresponds to three sets of cited observations, but only two are present in the table. It’s also somewhat confusing that you mix comparisons of observed and modelled mass balance with comparisons of DIL % - this is made even more confusing by the lack of these % DIL values in the table. I’d recommend adding some data on the % DIL and SMB from simulation and observations to Table 2. This would significantly clarify the last sentence, in which the authors state that % DIL agrees with Khan and Larsen – the reader is drawn to Table 2 for evidence of this agreement, but none is provided. Also, as mentioned in general comments, you need to untangle the prescribed and resultant mass loss before comparing with observations.

P8 Table 2: I guess the simulated changes don’t appear to correspond because Khan 2013 don’t measure changes in the whole domain of your model? It would be worth explaining this, otherwise readers might wonder how the 2002/05-2010 simulated mass loss is 32 Gt, but the 2000–2011 mass loss totals 133 Gt.

We added a paragraph and extended Table 3 to address both comments above:

"Khan et al. (2013) and Larsen et al. (2016) measure surface elevation changes from aerial photographs, satellites and digital elevation models between 1985 and 2010. These yield a total mass change during different time periods and congruent to our calculations ΔDIL is estimated as the residual of mass change and ΔSMB. Both studies refer to different areas within the UI catchment. Table 3 presents a comparison of the observed mass changes and our simulation results, recalculated for the particular areas. Due to sparse data coverage Khan et al. (2013) combine surface elevation measurements acquired between 2002 and 2005 to quantify elevation changes and refer to this period as 2002/2005. The average of simulated ice mass loss between 2002 and 2005 is taken for comparison with the 2002/2005 observations from Khan et al. (2013)."

P8 L4: As mentioned in general comments, I think you should discuss thickness changes, or else stick to absolute values.

Answered above.

P10 L9: Source for these winter velocity maps?

The winter velocity maps are produced from data available from http://esa-icesheets-greenland-cci.org/ and described in Nagler et al 2017.

We added the information to Table 1 and the caption of Figure 5.

P10 L23: “ice surface elevation. . . velocity observations”. This doesn’t seem to make sense.

The sentence changed during revision.

P11 L15: “The simulation reproduces not only the retreat...” - I don’t think you can say that the model reproduces the observed retreat and advance. You prescribe these changes.

This sentence was changed to clarify its statement: "Although we primarily discuss prescribed ice margin retreat, it is worth mentioning that our method also includes advancing observed terminus position changes at UI-1 and UI-2 in summer 2012 and at UI-3 in the summers 2001, 2003 and 2007."

P11 L27: “matching observed velocity, surface elevation and mass changes within 20% of observations”. As stated in general comments above, I think your comparisons with observations are flawed at present. Furthermore, Table 2, Fig. 4, Fig. 5 demonstrate that this figure of 20% is not accurate.

Changed. (See general comments above).

Technical Corrections

P1 L6: “and are within”

We deleted "and".

P1 L7: “Increased ice flow acceleration”, surely its just “ice flow acceleration” or “increased ice velocity”?

Changed to "increased ice velocity".

P3 L10: “The grounding line”, no need to capitalize.

Done.

P5 L21: “The basal friction”, ditto.

Done.

P6 L7: “away form”

Done.

P6 L2: “that are causing numerical instabilities” is not good english here.

Changed to "The additional calving fronts aim to improve realistic simulation behavior by splitting large ice area changes induced by the prescribed terminus changes into smaller areas within shorter time periods."

P7 Fig 3: Caption refers to SID rather than DIL.

Caption was changed.

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.

Khan, S. A., KjæR, K. H., Korsgaard, N. J., Wahr, J., Joughin, I. R., Timm, L. H., Bamber, J. L., Broeke, M. R., Stearns, L. A., Hamilton, G. S., Csatho, B. M., Nielsen, K., Hurkmans, R., and Babonis, G.: Recurring dynamically induced thinning during 1985 to 2010 on Upernavik Isstrøm, West Greenland, Journal of Geophysical Research (Earth Surface), 118, 111–121, doi:10.1029/2012JF002481, 2013.

Larsen, S. H., Khan, S. A., Ahlstrøm, A. P., Hvidberg, C. S., Willis, M. J., and Andersen, S. B.: Increased mass loss and asynchronous behavior of marine-terminating outlet glaciers at Upernavik Isstrøm, NW Greenland, Journal of Geophysical Research (Earth Surface), 121, 241–256, doi:10.1002/2015JF003507, 2016.

MacAyeal, D. R.: Large-scale ice flow over a viscous basal sediment - Theory and application to ice stream B, Antarctica, Journal of Geophysical Research, 94, 4071–4087, doi:10.1029/JB094iB04p04071, 1989.

MacAyeal, D. R.: Binge/purge oscillations of the Laurentide Ice Sheet as a cause of the North Atlantic's Heinrich events, Paleoceanography, 8, 775–784, doi:10.1029/93PA02200, 1993.

Morlighem, M., Rignot, E., Seroussi, H., Larour, E., Ben Dhia, H., and Aubry, D.: Spatial patterns of basal drag inferred using control methods from a full-Stokes and simpler models for Pine Island Glacier, West Antarctica, Geophysical Research Letters, 37, L14 502, doi:10.1029/2010GL043853, 2010.

Rignot, E. and Mouginot, J.: Ice flow in Greenland for the International Polar Year 2008-2009, Geophysical Research Letters, 39, L11 501, doi:10.1029/2012GL051634, 2012.