

Interactive comment on “Simulating ice thickness and velocity evolution of Upernavik Isstrøm 1849–2012 by forcing prescribed terminus positions in ISSM” by Konstanze Haubner et al.

Anonymous Referee #1

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General Comments

This study simulates the dynamic response of the 3 main Upernavik Isstrøm 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.

C1

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.

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.

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

In comparing surface elevations, the authors state that the surface lies within 20% of observations, and similar percentage comparisons of surface elevation are made

C2

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.

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

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.

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%.

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.

P2 L14: The final sentence of this paragraph feels out of place. Perhaps move it to the

C3

start of the next paragraph. "Hence" here implies that the focus of previous studies is a result of the two dynamic mass loss events.

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?

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.

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

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?

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?

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?

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

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

C4

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...

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

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

P7 L8: “hereafter anomalies Δ SMB and Δ DIL” - I see what you mean, but this isn't a sentence.

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.

C5

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

P10 L9: Source for these winter velocity maps?

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

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.

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.

Technical Corrections

P1 L6: “and are within”

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

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

P5 L21: “The basal friction”, ditto.

P6 L7: “away form”

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

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

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C6