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The Cryosphere
Discussions

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.

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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.

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Discussion paper

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.

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.

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. 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.

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. 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). 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.

Some miscellaneous comments: Page line 13 extra space before

Don't capitalize Grounding line or Basal friction

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?

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?

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

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