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Interactive comment on "A data-constrained model for compatibility check of remotely sensed basal melting with the hydrography in front of Antarctic ice shelves" *by* D. Olbers et al.

Anonymous Referee #1

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Summary:

In this manuscript, the authors formulate an idealized (multi) box model of ice shelf cavity circulation and ice shelf-ocean interaction to investigate whether such a model can reproduce published mean ice shelf basal melt rate estimates for a set of hydrographic properties at the cavity's inflow and outflow lateral open boundaries.

The idealized model has several free parameters [e.g., inflow intensity + the pattern or shape of the upwelling of the cavity circulation from a "deep box" to a shallower "sub-ice box"] which, together with an idealized representation of cavity geometry, yield a range of possible ice shelf melt rates for a given specified ocean inflow temperature and salinity. The simplicity of the model affords a search over a wide range of the parameter space to identify whether a subset of model parameters can be found that reproduces the observed mean basal ice shelf melt rate and ocean outflow temperature and salinity.

General comments:

Showing that a wide range of simple cavity circulation structures can yield observed mean basal ice shelf melt rates for a given set of ocean properties entering an ice shelf cavity is a potentially useful contribution. Few ocean circulation models are currently being used to explicitly simulate ice shelf cavity circulation and the subsequent interactions between the circulation and the ice shelf at fine resolutions. One problem is that the full three-dimensional cavity shapes are generally unavailable; another problem is that the proper form of the ocean-ice shelf melt parameterization is still debated. Therefore, if we know that mean basal ice shelf melt rates can be reproduced over a wide range of idealized cavity circulation patterns and with a relatively simple ocean-ice melt parameterization, we could more easily evaluate different numerical ocean models that explicitly simulate cavity circulation and ice-ocean interaction.

Simple box models can be extremely useful for developing a conceptual understanding of complex phenomena. However, it is not obvious how conceptual understanding is being advanced here. The authors show that canonical cavity circulation patterns can be made to yield realistic mean basal melt rate but this finding is not particularly surprising given that (1) their melt rate parameterization is a function of circulation speed in the sub-ice box and (2) the speed of the along-cavity flow in the sub-ice boxes is the free tuning parameter [total inflow volume plus the upwelling pattern can give either high or low average velocities in the sub-ice boxes]. The utility of the work would be much greater if the authors could compare their solutions against existing hypothesized cavity circulation patterns for particular ice shelves. Perhaps they could show an example where the only circulation pattern that yields a realistic mean basal melt rate in a particular cavity is different than what the community believes exists there.

In such a case, our basic conceptual understanding of the circulation in at least that particular cavity would be improved.

Estimates of the magnitudes and spatial patterns of ice shelf basal melt rates have been produced from observations and ocean general circulation models [e.g., Rignot + Jacobs, 2002, Science. Rapid Bottom Melting Widespread near Antarctic Ice Sheet Grounding Lines, Khazendar et. al. 2013 Nature Comm.], yet information about the spatial patterns of the basal melt rates are not used to constrain this idealized model, nor are they used for a posteriori evaluation of the solution parameter space. The authors should compare their results against existing melt rate spatial patterns or at least argue why they do not need to evaluate their model solutions against those observations (or simulations).

There is no comparison of cavity circulation solutions found in their idealized model with those found by more complex ocean general circulation models. It would be very helpful to demonstrate that simulations of a cavity circulation and/or ocean-ice shelf melt rates from ocean general circulation models do indeed fall within the idealized model's solution space. [see Heimbach + Losch, 2012. Annals of Glac. plus references therein].

There could be more discussion about the implications of their choice of the hydrographic properties of the cavity inflow. Inflow hydrographic properties certainly may vary significantly over a range of time and spatial scales but are typically only available along single synoptic sections during ice-free conditions in summer. On the other hand, published estimates of basal melt rates from remote sensing are typically presented as being long-term [annual or longer] averages. There is an obvious and serious inconsistency here – can one really expect to reliably match [annual or longer] mean basal melt rates using synoptic snapshots of potentially highly variable cavity inflow properties?

I am not convinced that the investigation of a very small subset of their model's parameter space significantly clarifies our current conceptual understanding of cavity circulation or ice shelf basal melt in general, or the circulation or basal melt patterns for any one ice shelf in particular.

Specific comments

1) why are hydrographic inflow properties not part of the parameter space? They are imperfectly known and it is likely that the solution parameter space is highly sensitive to the choice of inflow properties.

2) p.921: lines 4-9. The point of these sentences is not clear. Models are always approximations of reality, and it would indeed be surprising if the first attempts to simulate ice-shelf cavity circulation and ocean-ice shelf interaction would yield circulations and melt rates that were consistent with observations. The iterative improvement of models [in whatever form] by comparing their output against observations is how science advances. Yet, the authors suggest that complex models are too “elaborate” to refine, which is a bit of an exaggeration.

3) P.921 lines 10-14: the authors claim that their approach has the advantage that it allows one to potentially ‘scan the complete parameter space’. Yet the size of the parameter space considered here is not a fundamental feature of the problem but actually defined by the authors a priori. One could trivially expand the parameter space by also varying (1) hydrographic inflow T and S properties, e.g., +/- an uncertainty, (2) upwelling (w) decay parameter, \alpha: w(x) \approx w_o eˆ{-\alpha x}. Indeed, the dimension of the true parameter space of even this simple box model is much greater than the authors imply.

4) p.921 lines 25-end, p.922 lines 1-3: the authors claim that if they cannot find a subset of the parameter space that yields a mean basal melt rate that matches observations, then the “data . . . must be judged as incompatible”. In other words, the authors claim that their model can discover problems with reported mean basal melt rates. P. 930 line 9 and P.931 lines 23-end make the same point. The authors have not at all convincingly demonstrated that their idealized model can be used to identify problems

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8, C834–C838, 2014

in observational data. At this point, persistent discrepancies between this model and observations are much more likely to reveal limitations in the model's representation of cavity circulation than problems in mean basal melt rate inferred from remote-sensing observations.

Technical corrections

1) p.920, lines 1-5: provide reference 2) lines 15: constraint not constrains 3) lines 23: to measure not and measure 4) p.924 line 18: misuse of the word 'anachronistic' 5) P.923 equation (2): around which (T,S, p) state are the freezing temperatures being linearized around?

The manuscript requires language editing.

Interactive comment on The Cryosphere Discuss., 8, 919, 2014.