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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 #2

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Overall assessment

This discussion paper presents a simplified model of ice shelf-ocean interactions which, given background ocean temperature/salinity, basic along-flow ice shelf geometry, and the along-flow distribution of 'upwelling' (or entrainment) of background ocean characteristics into an ice-ocean boundary layer, allows for the determination of the spatial distribution of melt/freeze and the strength of the overturning circulation induced by the melt. The authors argue that (1) in their simple model, a large parameter space can be investigated, which is not the case for more complex models, allowing for a better understanding of the basic mechanisms at play; (2) a comparison between otherwise observed melt rates and the one obtained by their model can shed light on

(i) the processes leading to melt and (ii) the validity of the observations.

However, it is my opinion that the model they present is too simplified and contain too many tuning parameters to answer (1) and (2). To simplify greatly, the authors created a discretized version of a plume model, where melting is only dependent on in situ temperature at the interface, which itself depends on advection, entrainment and depth (due to the pressure dependence of the temperature freezing point). They allow themselves to tune the entrainment everywhere, and the mean mass flux (hence, although this is albeit more complex, the mean temperature advection). They acknowledge the fact that their choice of entrainment spatial distribution is crucial (last sentence, section 3). It is therefore difficult to believe that the parameter space they explore using three scenari of the entrainment spatial distribution allows for a unique, and better, understanding of reality. In addition, they find that their model is able to represent reality. But given the large degrees of freedom built in the system, this is hardly surprising. So, in general, it does not shed light on the processes leading to melt, and certainly is not engaging the validity of observations.

I share with the authors the idea that simpler, intermediate level models allow for a better understanding of their physics. Therefore, if such simplified models are able to roughly represent reality, one might hope that they contain the necessary and fundamental ingredients to understand a system. If however, a simple model is unable to represent reality, one necessary conclusion is that it is, at least partially, inadequate. If, on the other hand, it can represent reality but has a large number of solutions, it is not very constraining and, again, not very useful.

My feeling is that the author demonstrate that their model falls generally in the second and third categories. When it does seem able to capture the main observed features, it is not constraining the field of possibilities, and when it does not (in the case of Pine Island Glacier), it is because fundamental hypothesis of the model are broken.

The general scientific contribution of this work is therefore unclear to me.

Specific comments

Abstract, line 6: "the physics ... is relatively simple". Is it? Surely this is an hypothesis more than a statement. The physics of your model is simple indeed, but it might not reflect reality.

Abstract, lines 10-13 (and in general): mean overturning and mean melting over ice shelf in general are poor indicators and constraints. They are indeed very much dependent on the area/volume over which they are measured. It does not mean they are useless numbers to compute, but it does mean (to me) that they should be used with caution to constrain models. Couldn't you use less generic constraints? For example, could you use a rough distribution of melt to deduce an overturning strength and T/S distribution in the discretized plume?

Inversely, when you want to obtain a 'best' estimate of mean overturning and melt to fit the inflow/outflow characteristics, given the dependence of your result on the distribution of melt, you at least need to have an idea about it, don't you?

Finally, your model only contains 1 dimension, couldn't you discretize the same equations in 2 dimensions (over the horizontal)? If not, it might be useful to note why.

Section 1, page 922, lines 2-3: this might be just a poor choice of words, but you seem to imply that observations that cannot be fit by your model must be incorrect. This is clearly not true, as a more likely scenario is that your model is incorrect. Granted, observations are often indirect and also contains approximations, but they do generally converge within their margin of uncertainty!

Page 924, line 15: gamma_T and gamma_S are fixed, which means they also are tuning parameters of your model. Given the large uncertainty in these numbers (and forgetting the fact that they might not even be that relevat in reality), it would be useful to have an idea of their impact on the results.

General note: it would be VERY useful to summarize in one paragraph all the assump-

tions of the model. They are scattered all through the derivation of the model, and it is very easy to forget one this way.

Figure 3: Why is Qmax=0.9Sv for the slow decrease upwelling scenario, and not 1Sv, like the others?

Page 926, line 21-23: 'the latter demonstrate the importance of pressure' Given that your model setup is directly relating temperature to melt (which is far from correct in a real system, where flow velocity, at least, play a non-trivial role), it is not surprising that the impact of pressure is large. This sentence seem to imply that this is therefore also true for the real system, but you do not demonstrate this here.

Page 929, line 24: 'RON ...' Again, I do not think your simple model allows you to reach such a conclusion: could you get a possible solution by playing with other tuning parameters and with gamma_T/gamma_S. Is it possible to find a distribution with enhanced melt near the ice shel front that would fit the observations?

Page 930, line 1-10: You appear to reach an erroneous conclusion. To my knowledge, not one study used 2 different methods to deduce the mean melt over the same timeframe or the same ice shelf area. So comparing different methods is hazardous. Nevertheless, they do not differ as significantly as the authors suggest (for example, Dutrieux et al., 2014 reconciles results from Rignot et al., 2013 to that of Nakayama et al., 2013 and others). Beside, you acknowledge earlier that the model is not good for PIIS, so how can you use your results to infer anything in the case of PIIS?

Technical corrections

Page 922, line 12: remove 'at'

Page 925, line 15: aren't you forgetting a constant to your summation?

Page 926, line 4: conversion -> convergence? and 'becomes obvious' appear overstated to me.

Page 928, line 20: constraing -> constraining?

Page 930, line 10: secenario -> scenario?

Fig. 1: (h_bathy-h_draft)/3?? why?

Fig. 5: I do not understand the colour code.

Some americanisms (color->colour) might need to be corrected?

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