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Interactive comment on "A two-layer flow model to represent ice-ocean interactions beneath Antarctic ice shelves" *by* V. Lee et al.

V. Lee et al.

v.lee@bristol.ac.uk

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We wish to thank referee 2 for their comments.

1 Mix of linear and quadratic drag laws

The linear drag law for the cavity is a model assumption made for convenience rather than motivated by physical reasons. It is necessary so that a tractable streamfunction-vorticity equation can be derived. Taking the curl of the linear term, $c_D\overline{u}(x,y)$, and discretizing the result is considerably easier than doing the same to the quadratic term, $c_D|\overline{u}(x,y)|\overline{u}(x,y)/H(x,y)$.

In an earlier version of the model we also applied a linear law to plume drag. This meant that the momentum equations for each layer and the cavity were consistent, i.e. H*Eq. (10) - D_p*Eq. (25) with linear law $\equiv$ vertically integrating Eq. (1) across the ambient. The problem with the linear law was that the calculated melt rates remained less than 5 m/yr no matter the value of the entrainment coefficient and drag coefficient. They could not reach values inferred from observations because the linear law retarded the plume speed too much.

We use the quadratic drag law for the plume because it can produce realistic melt rates. The referee is correct, the momentum equations are not consistent. The quadratic law is a model assumption made for physical reasons. It has the advantage of increasing drag towards the underside of the ice shelf, which is a rough surface, without excessively retarding the whole plume. We feel this is justified because a quadratic law is more appropriate for turbulent flow, such as at a boundary. A linear law only really applies to slow flows. Our results show that the cavity mainly consists of slowly circulating ocean water. Fast flowing meltwater is confined to a narrow layer close to the underside of the ice shelf. Vertically integrating velocity across the whole cavity produces a slow average flow. The layers represent two different physical systems, so different drag laws is not unreasonable while $D_p \ll D_a$.

2 Not convinced the model is a good candidate to couple to an ice sheet model

The referee does not specify why they are not convinced, which makes a response difficult. A good candidate needs to

1. provide melt rates to the ice shelf that have spatial variation which reflect observations of high melt rates near the grounding line.

2. generate melt rates that can respond to changes in the shape of the underside of

the ice shelf calculated by the ice sheet model.

3. generate melt rates that can respond to changes in ocean temperature and salinity at the ice shelf front.

4. handle complex geometries, such realistic grounding line positions.

5. handle grounding line migration. The land/sea mask for the cavity model needs to be updated every time the grounding line moves. Sea cells created during grounding line retreat need to be initialized.

6. be computationally affordable to tackle the whole of Antarctica.

From existing candidates, one has the option of choosing a one-layer plume model, a box model or a GCM. The one-layer plume model fulfills most of these criteria except items 3 and 4. The scalar properties of the ambient water are prescribed using values at the ice shelf front. This means that changes at the front would be felt instantaneously near the grounding line. The one-layer model takes no account of the cavity's bedrock. Our model addresses these deficiencies. The one-layer has a wetting and drying scheme (Jungclaus and Backhaus, 1994) that can be used to initialize new cavity cells. Our model has inherited this scheme which goes a long way to fulfilling item 5. Olbers and Hellmer (2010)'s box model fulfill items 3 and 6 well, but cannot do item 4. It calculates mean rates for the whole ice shelf and for a portion of the shelf from the grounding line. It only takes into account the dimensions of the shelf and not its curvature. So items 1 and 2 are addressed in a limited way. Item 5 is not a problem given the simple way the cavity is divided up. It is important to note that circulation is parametrized in the box model, whereas it is calculated from momentum equations in our model. GCMs certainly satisfy items 1 to 4. However, they currently do not satisfy item 6 and it is not an obstacle anyone can avoid. It is not clear how they can handle item 5.

Referee 2 would like our model to be validated against GCMs. We have addressed this in our response to referee 1. (Please see sections "GCMs" and "Motivation".) Referee 2 is right in saying that using GCMs to predict future changes in ice shelves would be worthwhile, but is not achievable for more than one ice shelf at present. It is also beyond the scope of the paper.

3 Specific comments

3.1 p76 line 6:

We shall make the change.

3.2 p78 line 5:

We disagree with the referee that the derivation of plume velocity is not clear. We also give an explanation of our Picard iteration, but we can add a citation too.

3.3 p82 section 2.4.2:

Eq.s (44) and (45) describing melt rates are formed using turbulent boundary layer theory, where the water temperature, salinity and velocity at the edge of a narrow layer along the ice-cavity interface is relevant. These values in the plume are the best approximations. Values in the far field would provide poor approximations for the boundary layer. The ambient is at least 1 °C warmer and speed is an order of magnitude smaller so using these values would give very different melt rates. Ocean values, especially velocity, at the ice shelf front would have little or no relation to values close to the grounding line. We would need a different set of equations to model melt rates.

3.4 p108 lines 10-15:

The cavity is modelled as two-layers of different density moving at different velocities. This is a stably stratified shear flow with piecewise constant density and velocity profiles. The interface between the layers of such flows may be subject to instabilities depending on the ratio of the shear to buoyancy across the interface. The instabilities may develop into waves such Kelvin Helmholtz, if rotation is not important, or Rossby waves. At high entrainment rates, there is near geostrophic flow near the grounding line and a gentile increase in layer thickness, i.e. a few tens of metres per 2-3 km of the grounding line, in the y-direction. Both of which are suitable conditions for Rossby waves for a constant Coriolis parameter (Pedlosky, 1987). Our governing equations do not have the vertical dynamics to fully model such waves, but they do contain shear through the entrainment parametrization and buoyancy in conservation of momentum. We have added more explanation to the manuscript.

3.5 p108 lines 20-25:

Figures (7c) and (10) are discussed in the text. The results for high values of the entrainment coefficient have been included in the figures even though the model output is considered to be unphysical. We talk about varying parameters to find the limits of the model. They are included to show that the mean values look plausible even though they are not and to highlight possible numerical problems of the model at extreme parameter values. We believe that describing a model's limitations is one of the criteria of the GMD.

A rise in ambient temperature above the initial conditions is not physically possible without a source of heat. We have changed our choice of interface temperature (see

section "p80 line 10" in the response to referee 1) and rising ambient temperature is no longer a problem.

Interactive comment on Geosci. Model Dev. Discuss., 4, 65, 2011.