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Geoscientific
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Discussions

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

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We wish to thank the referee for a careful and considered review.

1 Arakawa C-grid

1.1 Variable arrangement

The referee suggests positioning the streamfunction and z-component of vorticity on the corners of a scalar cell rather than in the centre. This has the advantages of grid points coinciding with rigid boundaries and allowing compact central differencing of

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some derivatives. We agree with the referee that this is a more natural grid choice and we have altered the model accordingly. This includes implementing zero normal gradient on the streamfunction at the open boundary rather than a fixed value. The streamfunction-vorticity equation, Eq. (20), is now solved directly rather solving Eq.s (17) and (18) iteratively.

These changes have not noticeably affected the results presented in the paper.

1.2 Coriolis term

The referee suggests that the way we discretized $\nabla.\overline{\boldsymbol{u}}$ in Eq. (17) is incorrect because we cannot justify removing the p_B term in Eq. (10). I am afraid I do not understand the referee's point. Clearly this is important and we would welcome further clarification. We do not attempt to discretize Eq. (10) in our model, only the vorticity equation, Eq. (17), where the pressure term has already been removed by taking the curl of the continuous momentum equation. Further, our choice of discretization of $\nabla.\overline{\boldsymbol{u}}$ directly on a scalar cell is more compact than discretizing indirectly on u and v cells. By following the suggestion by the referee to change the positioning of streamfunction to the corners of the scalar cells, is this problem resolved? Perhaps the referee could suggest a reference where this problem is set out in more detail.

The referee suggests that (excessive) averaging is masking noisy velocity fields due to discretization of the Coriolis term on a C-grid. Changing the grid arrangement of the streamfunction has not changed the results. Noise is not a problem for simple geometry with reasonable flow parameters for a small time step. There is noise observed at the grounding for high entrainment and low plume drag coefficients which maybe due to this problem. However, the melt rates in the high entrainment experiments (mean values greater than 38 m/yr) are higher than observations (10 to 28 m/yr) suggesting this area of parameter space is unrealistic.

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2 Comparison with other models

2.1 GCMs

The referee would like us to compare our model to GCMs and has suggested using reference experiments from ISOMIP. It would be difficult to compare the speed of our model with a GCM fairly. Efforts have been made over a number of years by numerous coders to improve efficiency of GCMs. This is the first version of our code and our are aim was to get a functioning model which generates a spatial pattern of basal melt rates that include the effects of ice shelf topography and ocean properties. We do not claim that the code is efficient. We would expect a two layer model to be faster than a GCM because it uses less layers and plume velocity is calculated using an algebraic equation rather than by solving a partial differential equation.

We have looked at the ISOMIP website, which is an attempt at model intercomparsion. We are not aware of another intercomparison. The site is not complete and the publications list on the site are limited to a conference in 2003. One experiment has been completed with results from just three GCMs available. There is no clear guidance on what the results should be or comparisons with observation. We could approximate the experimental set up but we cannot replicate it, for example we use Cartesian rather than geographical coordinates. The only comparison we could make would be to say that the pattern of melt rates looked similar.

The referee suggested using the ISOMIP domain rather than setting up new geometry. We do not think using ISOMIP is appropriate for the reasons given in the last paragraph. We chose a domain based on Pine Island because is currently the most dynamic glacier in Antarctica and of most interest to the community. Repeating all our experiments with a different domain would take too much time and would not improve the paper.

2.2 One layer models

We felt is was important to compare our model with one layer models because we could use the same domain and parameter values for direct comparisons. We wanted to show that the reduced physics momentum equations produced reasonable results compared to the full model.

The motivation for the two-layer model was to address the criticism of the one-layer model that ocean temperature and salinity are not advected around the cavity. A finding of this work is that the velocity of the ambient layer is very weak and the assumption of a stationary ambient is not unrealistic. The two layer model had to be created to find this out. In the future development section we comment on this and suggest how the finding might be used to improve the model.

2.3 Simple models

We can compare our results with Olbers and Hellmer (2010)'s box model. They obtain melt rates for Pine Island glacier, which take the form of a mean value for the whole ice shelf and a mean value for a third of the shelf in contact with the grounding line. We can use our data to calculate the mean melt rates close to the ground and show that we can obtain similar values with certain flow parameters. Unlike our model, the box model can not produce a spatial pattern of melting, it is limited to very idealized cavity shapes and the topography of the underside of the ice shelf is not included, only its dimensions. Also, the box model parametrizes circulation rather than solve the momentum equation.

2.4 Motivation

Our primary aim in developing the current model is to couple it to an ice sheet model to study the interaction between ocean temperatures, ice melt and grounding-line migration. In this context, the model offers several advantages over existing complex OGCMs and simple box models. The first is that, unlike box models, it is able to simulate the spatial pattern of melt experienced by an ice shelf. The details of this pattern (in particular, high melt rates close to the grounding line) are thought to be important in controlling grounding-line dynamics (Walker et al., 2008). Second, the model is likely to have far lower computational requirements than a full 3D OGCM, which are likely to become important at the high spatial resolutions ($\sim$1 km) and long time spans ($\sim$1,000 yr) required to study grounding-line migration. Finally, changing the spatial domain of our model in response to grounding-line migration is likely to be easier in the current model compared to an OGCM. This is due to the relatively simple formulation of our lateral boundary conditions which have been implemented within a wetting-drying framework (Jungclaus and Backhaus, 1994), and the two-layer structure which is likely to be more stable in newly-defined ocean grid cells than a full vertical discretization.

3 Length of the manuscript

We agree with the referee that section 5 needs to be cut down.

Removing the coupling description is important to the motivation behind the paper and vital for future developments. The paper is incomplete without such a discussion.

4 Smaller problems

There is no requirement of the model to use the linear equation of state. We have stated the linear equation because that is what we use.

4.1 p73 line 13:

The model requires that volume is conserved in the vertical so that a streamfunction can be defined. This means that changes in volume due to melting and/or freezing must be neglected and the plume grows only through entrainment. We use the results from Payne et al (2007) for Pine Island as an example of where entrainment rates are much larger than melting rates to indicated that assumption is reasonable. Holland et al (2008) also find that entrainment is much larger than melt rates for generic simplified cavities using a GCM. We have not cited Holland et al. because volume changes due to melt rates are not included their GCM (personal communication). In Payne et al's model, plume thickness is allowed to vary with melt rates so the relative size of the two rates can be compared.

4.2 p75 line 1:

We have included the vector identities because some readers, such as glaciologists, may not be familiar with vector calculus commonly used in oceanography. Also, how to handled pressure in cavity model beneath ice shelves is a source of confusion in our experience, so we wish to give a clear and complete explanation of our approach.

4.3 p79:

We assume that the referee is saying that the derivation of the third term on the r.h.s of Eq. (36) is unclear. We can change the order of the subsection to introduce the relevant melt rate equations before Eq. (36).

4.4 p80 line 5:

We can add a couple of sentences about how γ_T and γ_S are determined.

4.5 p80 line 10:

The temperature and salinity at the interface between the two layers is modelled using depth weighted averages of temperature and salinity in the plume and ambient, Eq. (37). This means that the interface has values influenced by the deeper layer. It allows the scalar properties in each layer to feel the effects of entrainment without excessively heating the ambient.

The referee suggests using $\chi_I = \chi_p$. This would not work because when it is substituted into Eq. (36) the contribution from entrainment to the plume disappears. Crucially, the plume's scalar properties could not be influenced by those of the ambient and ultimately those of the ocean at the ice shelf boundary.

Another possibility, which we rejected, is to use the mean value, $\chi_I = (\chi_p + \chi_a)/2$. The entrainment terms become $\dot{e}(\chi_a - \chi_p)/(2D_p)$ in Eq. (36) and $\dot{e}(\chi_a - \chi_p)/(2D_a)$ in Eq. (38). Both these terms are sources of heat and salt in our experiments where $\dot{e} \geq 0$ and $\chi_a > \chi_p$. Looking at the terms, the source is stronger for the plume than the ambient, because $D_p < D_a$ in our experiments. It seems reasonable that entrainment will affect the thinner layer more. However, we found that the ambient heated up to 2 °C in 75 days near the grounding line where entrainment is strong, which is wrong. Note, vertically integrating the 3D Eq.s (4) and (5) across each layer we obtain $d(D_pT_p)/dt \propto \dot{e}T_I$ and $d(D_aT_a)/dt \propto -\dot{e}T_I$, i.e. the entrainment terms are equal and opposite. The two source terms mentioned above arise when Eq. (30) and $\partial D_a/\partial t + \nabla.(D_p\bar{\boldsymbol{u}}_a) = -\dot{e}$ are substituted into the integrated equations. The rising ambient temperature is accompanied by a decreasing ambient thickness to satisfy the vertically integrated equation.

The depth weighted average, Eq. (37), does not have this problem. The ambient cools over time and tends towards temperatures that lie within the range of values at the open boundary. The entrainment terms are now $\dot{e}(\chi_a - \chi_p)D_a/(2HD_p)$ in Eq. (36) and $\dot{e}(\chi_a - \chi_p)D_p/(2HD_a)$ in Eq. (38). Both are sources, but the later is much smaller (by about four orders of magnitude) than the former in our experiments. The interface temperature is warmer using the depth weighted average rather than the straight mean. The plume is warmer and melt rates are higher, unsurprisingly. However, the source term for the ambient is about two orders of magnitude smaller, using mean values from the standard experiment, for the weighted average compared with the straight mean, which means the ambient does not heat up.

With hindsight, using a weighted average is not an intuitive choice. A better choice and one that has been implemented in the model is

$$\chi_I = \{\, \chi_a if \dot{e} \geq 0 \chi_p if \dot{e} < 0, \tag{1}$$

which means that the scalar properties at the interface take values of the entrained layer. By considering the entrainment terms in Eq.s (36) and (38), the properties of layer receiving entrained water are affected, but those of the other layer are not. We find that results of the experiments using this definition of interface properties are very similar to those using Eq. (37), except there is no spurious warming of the ambient.

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4.6 p83 Fig 2:

Fig. 2 is not strictly necessary, but it makes the spatial discetization clearer for the reader unfamiliar with a C-grid. In particular, it helps explain the indexing of the discrete derivatives.

4.7 p86 Eq. (55):

In our new version of the model this equation no longer used.

4.7.1 p88 line 15:

The threshold for small flow speed is 1×10^{-15} ms^{-1}. For smaller values we assume that the two layers are not moving relative to each other. Therefore there cannot be any entrainment and so we set $\dot{e} = 0$ at that point.

4.8 p89/90:

Good idea, thank you.

4.8.1 p93 line 4:

The referee suggests setting $f = 0$ to see what the flow field looks like. We cannot evolve the plume velocity from rest using Eq. (28) if we set $f = 0$. Instead we use the the one-layer model with full momentum. The flow travels from the grounding line to the open boundary (see figure) and the plume grows in the direction of flow. There is some vorticity generated from the corners of the glacial inflow which weakens and

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spreads towards the open boundary. The flow is approximately irrotational. We do not observe vortex stretching or topographic steering as the plume thickens. It is worth noting that potential vorticity is not conserved in the plume near the grounding line for the standard experiment. To answer the referee's question, we do think Coriolis is the only factor.

4.9 p98 line 21:

Comparing two different open boundary conditions is probably silly. We wanted to point out to the reader the effects of different conditions on the flow and to demonstrate that our extrapolated method was an improvement on the no normal gradient condition.

4.10 p109 and conclusions:

Our choice of eddy diffusivity was based on values used in previous models rather than on grid resolution. We did not find that the stability of the code depended on the eddy diffusivity. Wall-flow oscillations developed for timesteps within the diffusive stability criterion. The referee expressed concern that our conclusion that the ambient is mainly diffusive is dependent on parameter choice. In response, we have conducted extra experiments with diffusivities down to 1×10^{-2} m^{2}s^{-1}. Changes to the interface temperature and salinity (see section 4.5) mean that there is no unphysical warming in the ambient noted in the original manuscript. Advection is very noticeable in the ambient for low diffusivity. The ambient near the grounding line is still cooled and the end state of the ambient's temperature lying in the range of temperatures at the open boundary is unlikely to be different. The process is advection rather than diffusion. However, it is extremely slow and mean, extrema and percentile values for the variable fields of both layers do not change below 1 m^{2}s^{-1}. The time to steady state in these experiments is much greater the 50 days run. This behaviour is consistent with our

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original conclusion. Contrary to the referee's suspicion, assuming 1×10^{-2} m^{2}s^{-1} is small enough, we observed no noise in these experiments.

4.11 p111 line 27:

When we talk about improving the model we are referring to the regression model. Improvement is measured by an increase in the R-squared value. Perhaps this needs clarifying.

4.12 p114 first paragraph and earlier in section:

We agree with the referee that it is not intuitive that the plume drag should influence melt rates when the coefficient is varied alone and as one of the variables in a bivariate regression model when all four parameters are varied, but not in a simple regression model. We can not offer an explanation.

4.13 p114 second paragraph:

It seems reasonable that the wall-flow oscillations are a consequence of an unsolved boundary layer. It also explains why we observe a narrower wall-flow in the models with momentum equations with reduced physics compared to that of the model with full physics.

5 Technical corrections and suggestions

We are happy to make the changes the referee has suggested.

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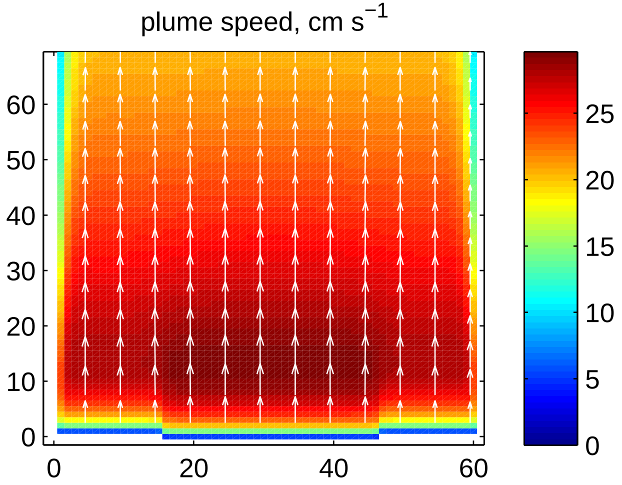

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