

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

Anonymous Referee #1

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General comments:

The manuscript describes the equations and the implementation of a numerical model of ocean circulation in ice-shelf cavities and elaborates on a number of sensitivity experiments with this model in an idealized geometry. The model is an extension to previous plume models by adding a layer of active ambient flow underneath the plume layer. This innovation makes this model attractive as a potentially inexpensive (or at least affordable) model of intermediate complexity to be coupled to ice-sheet and ocean models. Unfortunately, I think that there are several issues with the manuscript (and probably with the model) that require a major revision, before the manuscript is interesting for the community.

Specific comments:

C2

The model is formulated on an Arakawa C-grid. I do not understand, why streamfunction and vorticity are represented on scalar (or C-) points. The natural choice for these variables are the corner points of the scalar control volumes that in C-grids are often even termed vorticity (or Z/zeta-)points. With this choice $\zeta(i,j) = (v(i,j)-v(i-1,j))/dx - (u(i,j)-u(i,j-1))/dy$ and $Hu(i,j) = -(\psi(i,j+1)-\psi(i,j))/dy$, $Hv(i,j) = (\psi(i+1,j)-\psi(i,j))/dx$, without any additional averaging. Boundary conditions for zeta (no/free slip, e.g. for free slip $\zeta=0$ on the boundary) and ψ also follow naturally. Further the divergence of velocity in eq.17 is a consequence of the Coriolis term in eq.10, which is a notorious problem on C-grids: it requires averaging u to v -points and vice versa. For consistency, when you take the curl of the discretized eq.10, the resulting divergence in eq.17 has to be evaluated using the velocities averaged to the v/u -points. Otherwise you cannot justify dropping the ρb term in its discretized form and you violate conservation properties. I can only speculate, but I assume that either part of your numerical issues arise for this choice of discretization (the averaging introduces computational modes), or, even worse, the excessive averaging smoothes the fields and hence masks other problems: C-grids tend to produce noisy velocity solutions, mostly because the discretization/approximation of the Coriolis term changes the dispersion relations for fast waves so that energy is transported towards the shortest wavelengths, leading to grid scale noise. Usually one requires some form of (lateral) dissipation to remove this energy at short scales. Possibly the smoothing in your discretization takes care of that.

The model is presented as filling the gap between very simple and very complex models. If the reader should believe that this is a gap that requires filling then there should be some evidence for that. For example, it should be shown/argued that

(1) the new model is faster than full GCMs of the cited literature, but still produces the main features required from a sub-ice-shelf model. This could be tested/shown with reference experiments from previous publications (e.g. ISOMP: http://efd1.cims.nyu.edu/project_oisi/isomip/overview.html), and according to the acknowledgements, the authors interact with scientists who can provide such reference

C3

runs), rather than setting up a new idealized geometry. In the absence of observations, it is probably necessary to rely on GCM simulations;

(2) the new model is more realistic than simpler models justifying the extra (computational) cost of the new features. A comparison to the simple plume models conveyed (to me) that the differences between the models is not very large, or at least the authors failed to point out the benefit of having a more complicated model. Comparing to results of even simpler models would also be appropriate (Hellmer + Olbers, 1989, Olbers + Hellmer 2010, etc.): Are the melt rates different (more realistic)?

If this evaluation in the context of the different model options is missing, the reader does not know the purpose, the motivation of introducing yet another model. Just because there hasn't been one yet, is not enough.

The manuscript is far too long. The language is not always concise but tends to become narrative. Especially the description of the circulation and the various sensitivity experiments is lengthy and overly detailed to the extent that I quickly lost interest in reading and understanding this part. I think that section 5 can (and should) be easily reduced to one third of its current length without losing its essentials. Further, there are many places where the text could be shortened. E.g. most page 115 describes a coupling algorithm, before all physical problems are settled (some of which are described on page 116). I think that this description does not belong in this manuscript. (Further examples below.)

Smaller problems (and suggestions):

Is the linearized equation of state (eq.6) a requirement?

p73l13: If the model is meant to be a tool for more than just Pine Island Glacier, making restrictive choices for parameterizations (or model design) is not useful.

p75l11: a "smaller" example for making the text shorter: Do we really need to read the nabla x (X x Y) identity again? And that curl(grad scalar) = 0 is equally well known.

C4

pp79: strictly speaking, eq36 does not follow from eq4+5+12+29 but requires further assumptions about the boundary values that are stated only after eq36. I'd change the order.

p80l5: if I do not want to look up Holland and Feltham (2008), can you describe the method briefly (in a sentence or so)?

p80l10: what's the motivation for eq37? Why not use the plume value χ_p , since the plume abuts the boundary? p83: is Fig2 necessary?

p86 eq55, if this is the motivation for putting zeta and psi at C-points, then it's the wrong motivation, see above.

p88l15: what is the reason to this? what is the threshold for "small" flow speed?

p89/90: another example for shorter text: the first part of 3.4 could be shortened to "Beneath the ice shelf front we have an open boundary condition. Such conditions are notoriously difficult to implement numerically (Blayo and Debreu, 2005). The implementation needs to close the system of discrete equations while allowing flow to pass through the open boundary seemingly unhindered. We treat the outflow condition in similar manner to outflow in a supersonic, compressible simulation (Anderson, 1995) by extrapolating the variables in the interior of the domain to the open boundary. Simpler approaches (e.g. by Payne et al. (2007)) turned out to be inefficient in fluxing the plume out of the domain and were discarded. Etc.

p93l14: Is Coriolis the only factor? What about vortex stretching? The relevant conservative property is potential vorticity, in QG form $(\zeta + f)/D$ (D =thickness). If D increases (thicker plume) zeta has to increase (more counterclockwise, so flow the left boundary) for QGPV to be conserved. What does the flow field look like when $f=0$?

p98l21: if the open boundary conditions are very different then a comparison near this boundary does not make too much sense.

p109 and conclusions: Eddy diffusivities of $50-1000\text{m}^2/\text{s}$ appear awfully large

C5

for a 1km resolution. The corresponding stability criterion is approximately $\text{diff}K*dt/dx^2 < 0.25$ which is satisfied only because of very small time steps. Maybe the conclusion that the flow is mainly diffusive is a consequence of the choice of parameters. It also raised my suspicion that the observed noise might be masked by high diffusion (in both scheme and explicit diffusivity).

p111|27, suddenly there is talk of improving the model. But I cannot see how this improvement is measured?

p114 first paragraph and earlier in section 5: It is not intuitive that the plume drag coefficient has a large impact on the solution (Fig7) when varied alone, but not so when varied along with other parameters, and then the difference between a simple and a bivariate regression is not clear either. Something is missing in the discussion.

p114 second paragraph: is it possible that the grid scale noise is a consequence of unresolved boundary layers? From Munk-gyre theory, you need a minimum viscosity to produce large enough boundary layers that can be resolved by the grid spacing. The Munk layer width is $(Ah/\beta)^{1/3}$ and should be larger than $> dx$. Since you have no viscosity, it's zero. For $\beta=1e-11$ you'd need a viscosity $> 0.01 \text{ m}^2/\text{s}$.

Technical corrections/suggestions:

eq(23) there is probably a line break missing between dz and $\overline{\nabla\{p_b\}}$

p78|1: reference for Picard iterative scheme?

p82|24: This allows the(?) plume to . . .

p87|18: and -> or ?

p92|22: reference for "Latin hypercube"?

p93|20 and many other places: I learned that using "respectively" in this context is bad style (Strunk and White, Elements of Style, ISBN 0-205-30902-X), why not say, e.g., "Its spatial mean characteristics are a speed of 18.9 cm s^{-1} , a thickness of 8.1 m , and

C6

a density of $1029.39 \text{ kg m}^{-3}$."

p95|6: water cannot be "drawn" (from a physical point of view it is always "pushed" by a pressure gradient, or advected by the associated velocities)

p107|20 counterwise -> counterclockwise

p111|15: "due to" -> "to"?

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

C7