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The Cryosphere
Discussions

Interactive comment on "The Potsdam Parallel Ice Sheet Model (PISM-PIK) – Part 2: Dynamic equilibrium simulation of the Antarctic ice sheet" *by* M. A. Martin et al.

Anonymous Referee #1

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A nice piece of work. The authors have taken a well-written model (PISM) and extended it in ways that are absolutely necessary to study fast modes of change in the Antarctic Ice Sheet. This manuscript represents the next logical step after to model development - a whole-continent steady state experiment and comparison with observed fields. The use of simplified parameterizations of complicated process provides a useful way to assess the "minimum" of what is needed to capture a "snapshot" of Antarctica as it is today. (I would caution, though, that this is not necessarily the same as the "minimum" needed to project future change, as e.g. it presupposes where subglacial water may exist.) Many of the most relevant values have been compared between model and observations, I feel, and I look forward to seeing results from future experiments

performed with the model.

I have a few comments and questions which I would like to see addressed.

1. Regarding ocean melting, it was nice to see that some attempt at realistic melt rates was made, but I have some questions about that:

i. What was the basis for the choice of \gamma in the melt parameterization? In HJ99 this is related to mixed layer speed (and possibly a few other things) - a rationalization in the text would be nice.

ii. The amount of heat available for melting at the ice shelf base depends on the heat flux into the ice shelf. This does not seem to be accounted for. How did k dT/dz|z=b (heat flux into the shelf) compare with the flux from the ocean?

iii. Why would the basal dirichlet temperature condition not be the freezing point? Temperature should be continuous at the interface. Seems especially strange because this would make the ice temperature even warmer than the water, which should not be true anywhere in the ice shelf (given that the air temp. at the upper surface is even colder).

iv. I was not a referee on Bueler and Brown 2009 or the companion to this paper, so I must ask this here. I scanned those papers and could not get a clear answer - is the temperature equation solved in z-coordinates or sigma (terrain-following) coordinates? If it is the former, I think your scheme for temperature evolution needs to be explained in detail, either in this paper or its companion. Keeping a dirichlet condition on a moving (?) boundary in a z-coordinate frame is not straightforward.

v. Did you calculate the Peclet numbers related to the melt rates? Was the resolution you used sufficient to capture the associated boundary layer?

2. page 1314, line 12: how similar is your P_eff distribution to the thermodynamically-defined one? can you show a figure?

3. page 1316, line 10: is this volume of fluctuation of floating ice due to variability in floating front extent or in ice shelf thickness? Or something else?

4. page 1318, line 11-12: how does this happen if a CFBC and subgrid ice front migration is not implemented for ice cliffs and marine fronts (as you say later)? If ice that moves past a cliff or a front is "calving off in the same timestep", then within that timestep you presumably assess the principal stresses and add or remove mass from those floating partial-cells. How/where are the principal stresses evaluated? This process needs to be addressed in this manuscript or one of its companions.

5. Has the "dynamical core" (i.e. the isothermal velocity solver) or PISM-PIK (or PISM) been tested against the results of the ISMIP-HOM intercomparison? I am curious to see how it compares with Blatter/Pattyn. Looking at Fig. 12, it looks like velocity is underestimated in a few ice streams where, as far as i know, there is a nonnegligible vertical shearing component. One difference in the velocity solve between PISM-PIK and other "hybrid" solvers, e.g. Pollard and DeConto 2009, Schoof and Hindmarsh 2010, and Goldberg 2010 (in press) is that in PISM-PIK, the strain rates from one lower-order balance are not accounted for in the viscosity of the other. I don't know how large a factor this is.. could be that the SIA enhancement factor more than compensates.

I wouldn't expect this final comment to be accounted for in the manuscript, but I would just like to open it up for discussion.

Interactive comment on The Cryosphere Discuss., 4, 1307, 2010.