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Discussions

Interactive comment on "Antarctic sub-shelf melt rates via PICO" *by* Ronja Reese et al.

F. Pattyn (Referee)

fpattyn@ulb.ac.be

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General comments

This is a great paper that fills in the gap that is currently existing in linking large scale ocean to ice-sheet models. At this time, it is probably the best alternative to fully coupled ice-shelf - ocean cavity circulation in order to determine basal melt rates underneath ice shelves. The method is based on the Olbers and Hellmer box model (OH10), but extended to two plan-view dimensions. While it encompasses a series of approximation to this simple model, it is superior to current parametrizations used in large-scale ice-sheet modelling relating melt rates to ice draft. The paper is well written and gives sufficient details on how the model is derived from OH10 and implemented numerically.

The basic premise of the PICO ocean-coupler (if I may call it so) is that circulation in ice shelf cavities is based on vertical overturning. Parameters are therefore chosen in such a way that overturning is applied to all sub-shelf cavities around Antarctica, leading to sub-shelf melting close to the grounding line and accretion (if conditions apply) away from it. Results of the model applied to present-day Antarctic ice shelves gives sub-shelf melt rates close in agreement with observed values. A brief sensitivity analysis demonstrates the effect of ocean temperatures on sub-shelf melt rates.

While the model is definitely interesting for use as an ocean coupler (in absence of fully-coupled solutions), some care should be taken in its future use: it is based on stable vertical stratification, it only considers overturning circulation under ice shelves, it neglects Coriolis effects, and it relates ocean temperature (not circulation or intrusion of CDW underneath ice shelves) to sub-shelf melt. However, major advantages are that it considers the physics of th overturning circulation and that ice shelf size (given by the number of sub-shelf boxes) and distance to grounding line and ice shelf front matters.

I have only two major comments on the paper:

1. Why using basins and not individual ice shelves to link to mean values of T_0 and S_0? It seems to me that ocean circulation (and temperature/salinity) is related to individual ice shelves and not to drainage basins, which are governed by inland ice flow. Furthermore, during prognostic simulations, these drainage boundaries may change over time, making the initial setup invalid. By treating individual ice shelves, it would also give greater detail in the coupling with ocean-model results. Furthermore, it is not complicated to implement this in a dynamical fashion.

2. While details on the implementation in PISM are given, the presented material doesn't go further than applying it to the BEDMAP2 geometry (at a given spatial resolution). Basically, the link with PISM is non-existent. It would therefore be appropriate to see how the model behaves when really applied to PISM, i.e., for an

initial state (for instance spinup) close to the present day, where ice shelves are actually evolving. The only experiments shown are diagnostic, but a prognostic run would really demonstrate the capacity of the PICO coupler. Furthermore, a short run forward in time would reveal how sub-shelf conditions adapt to changing grounding-line position.

Detailed comments

- P2, L1: A reference to Thoma et al (2008) 'Modelling Circumpolar Deep Water intrusions on the Amundsen Sea continental shelf, Antarctica - GRL' would also be in place.
- P2, footnote: http://www...
- P5, L20: form stress
- P5, Eq (4): I guess there is an error in this equation, since the value of ρ will be a fractional value and not a local density. In fact, the original equation from OH10 reads

$$\rho = \rho_* \left(1 - \alpha(T - T_*) + \beta(S - S_*)\right)$$

  where T_* = 0C and S_* = 34 PSU. In combination with Eq (3), this then leads automatically to Eq (A9), where these two *-values are cancelled out.
- P6, L3: Neglecting heat flux into the ice, ...
- P6, L19: see major remark: why not using ice shelves instead of basins?
- P6; 2.3: What happens if the shelf is really thin or absent? Is the box model still applied for these contacts with the ocean?

- P7, 2.4: This section is irrelevant as long as the behaviour in PISM is not shown. I wold therefore like to see such a simulation with the coupled system that evaluates the coupler for ice-sheet modelling beyond the diagnostic case. It would also be useful to see how it behaves when the grounding line retreats.

- P8, L4: Even when the average of grid cells of the adjacent box is used to connect with the next box, a sharp transition (maybe less sharp) exists, as can been seen from Fig. 5. I agree that the box model can be applied locally to the shelf geometry through local variation of the ice pressure p that changes the local temperatures and salinities. However, the boundary condition to a box is given by the conditions of the adjacent box, not the conditions of a series of elements that are closest (physically this makes no sense). So, why not using just the adjacent box properties (which are mean values anyway) as a boundary condition to the local values with a box? Wouldn't this also make the model more conservative (see discussion on P15)?

- P8, 3: See remark on basins versus ice shelves.

- P11, L1-4: I would not consider criterion 3 and 4 criteria. 1 and 2 definitely are the basis of the overturning model; 3 and 4 are limits obtained from tuning (or validation with respect to two ice shelves), not criteria.

- P13, L2: See previous remark. I would't state that generally the melt rates are highest in the vicinity of the grounding line; that is an assumption made by the model and should be stated as such.

- P13, L5: Ice shelf thickness, hence pressure p is a factor that has a relatively large impact on melt rates. This is also the reason why highest melt rates within box 1 are found nearest to the grounding line. To me this is a more important observation.

- P15, Discussion: Some discussion on the limits of the model should be given. Coupled ocean-ice sheet/shelf models show that not always the maximum melt is reached at the grounding line (e.g., De Rydt and Gudmundsson (2016) Coupled ice shelf-ocean modeling and complex grounding line retreat from a seabed ridge, JGR) . Also, what are the consequences of considering overturning circulation for all ice shelves; the assumption of always having melt in box 1 and decreased melt/accretion towards the front, stable vertical stratification, and relating ocean temperature (not circulation or intrusion of CDW underneath ice shelves) to sub-shelf melt?

- P17, L2: This has not been shown in this paper and remains 'potential'. Although I recognize the potential of it.

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