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Interactive comment on "Sea ice and the ocean mixed layer over the Antarctic shelf seas" *by* A. A. Petty et al.

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This paper describes development and analyses of a mixed layer model for Antarctica, coupled to a sophisticated sea ice model (CICE). The authors describe the model outcomes for four regions (Ross, Amundsen, Bellingshausen, Weddell) in terms of mixed layer properties and depth, and sea ice growth, decay and advection.

Overall, the goals and approach to this problem are laudable. The results suggest that the model is doing a good job of representing sea ice processes and mixed layer response, despite the "passive" nature of the deep ocean which is held relatively constant by nudging to climatology. It appears the model would provide a valuable framework for further sensitivity studies.

However, I found the technical development section to be too confusing to understand sufficiently, and some sections of analysis to be too convoluted. With this in mind, I've limited comments here to "major" comments only, in no specific order.

1) Problems begin early (in the Abstract) where the authors talk about "surface power". In the Abstract, this term is not defined. Later, it is defined mathematically, but not in practical terms. It comes across as "potential to deepen or shoal the mixed layer", but it is never clearly explained how wind stress, cooling and brine rejection are put on a level playing field. Presumably wind stress loses out in terms of deep water production because it does not penetrate far, even in a homogeneous ocean because of planetary rotation and low vertical viscosity. But, in layman's terms, how is this constraint relative to buoyancy forcing terms enforced?

2) Many sentences are too rambly. Rule of thumb: if a sentence runs longer than 3 lines, it probably needs to be broken down.

3) Explanation of model is insufficient. I recommend the authors include a table of *all* variables in the paper. This may identify constants and variables that have not been adequately explained. Figure 3 is key; however, the caption assumes that everything on this figure needs to be obtained from the main text. A more detailed caption would help, along with including fixed parameters like h_s.

4) I couldn't find explanations of T_f, h_s (although I finally found it in Table 1), and it looks like 'w' might the same as '\omega' ?? This should be solved by ensuring that every symbol is explained at first appearance. Not much mention of model vertical resolution: I first became aware of this in Table 1.

5) Text on many figures is too small, at least as figures print out in "printer-friendly version". E.g., it is almost impossible to read text on Fig. 2 (also, to see black direction vectors on a generally dark background in 2f).

6) The model fit with expectations, primarily in sea-ice characteristics, production, melt

and advection, but also mixed layer response, is very good. The maybe naive question I have is: How much does this represent good physics, and how much comes from relaxation schemes to keep deep T,S, close to climatology? E.g., I imagine that the SW Weddell Sea is easy to deep-convect in, because its climatological stratification arises from deep convection, while the A and B seas don't allow this because the clim. stratification is too strong. (In those cases, surface forcing can eat into the remnant WW layer, but not more unless deep diapycnal mixing is really high.) That is, if you specified poor deep-ocean climatology, how much would your results change? Does this even feed back into sea ice evolution so that sea ice must match reality at some level?

7) Staying on the same "problem": If the deep climatology is fixed with some fairly short nudging time-scale ($\sim$1 y), then interannual variability of upper-ocean and sea-ice might be constrained. E.g., in the Ross Sea, Comiso et al. (2011) and Drucker et al. (2012) found quite strong trends (and interannual variability) in sea ice production and export, and you already comment that perhaps increasing freshwater from the A. Sea is driving trends in Ross Sea shelf water masses. But, by nudging these back to climatology, you set an artificial deep stratification that, I suspectk, would damp the modeled interannual ocean and ice variability forced from atmospheric variability.

8) I got a little lost in sea ice forcing. Eq. 21 appears to constrain ice/ocean stress to be 15 degrees off the ocean velocity. That obviously doesn't apply where internal stresses get too high, e.g., against coasts like the western Ross where advection is forced northward regardless. There is insufficient information about CICE *dynamics* to decide what the impact of the assumed rotation angle is, nor did I get a sense of whether it matters or whether ocean currents *not* associated with the wind stress and thermodynamics might be important. It is good to use CICE, but one question some of us have is whether simpler models might be good enough? Maybe codes with a single ice thickness class are adequate?

9) Figure 7 caption needs to explain the dots on 7a.

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10) On page 4339 you claim that "the regionally varying surface fluxes can directly explain the bimodal distribution in shelf seabed temperature" {with caveats}. Not really. There are large-scale reasons for distribution of seabed T that have very little to do with surface fluxes over the continental shelves. Furthermore, as I've suggested above, it feels (to me, at least) like nudging to deep climatology forces the model to set the upper ocean to be in balance (over a time scale of a year or so) with the originally specified climatological T and S fields.

11) Minor. Sometimes uncertainties are cited at too high accuracy. e.g., on page 4344, you have 578 +/- 39.8 km^3. Perhaps "580 +/- 40" would be sufficient?

12) A lot of your comparisons of specific numbers (total ice production and import/export etc) with previous studies would be clearer and more concisely explained in Tables.

13) The method for defining polynyas so that you can compare polynya production with previous estimates seems somewhat ad hoc so that, when you cite fraction of total production in specific polynyas relative to the total, the numbers don't carry much weight. I would prefer that you just state the importance of polynyas and that your model is too coarse to really capture them.

14) I do not include typo/grammar and minor comment suggestions here. Will do this later or offline via the Editor.

Interactive comment on The Cryosphere Discuss., 7, 4321, 2013.