



Interactive comment on “Multidecadal Polynya Formation in a Conceptual (Box) Model” by Amber Boot et al.

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Received and published: 11 August 2020

This is an interesting study where the authors have developed a box model formulation similar to Martinson et al. 1981 for upper ocean mixing in the region of Maud Rise. While this is a very nice study, I am concerned about the underlying assumption that is basing polynya formation on CESM high resolution model results. Here are some specific concerns that I have with the manuscript as is.

1. First off, it is extremely difficult to figure out exactly how these high resolution simulations were done. The authors refer to a manuscript in review (van Westen et al. 2020) for a description of the experiments. However, the model experiments and configuration are actually described in an earlier manuscript by van Westen and Dijkstra 2017.

Please add more detail here about these simulations so the reader does not have to sift through the rest of the literature. Can the authors also comment about using year 2000 forcing as a control run for 250 years which is not a balanced climate?

2. Here is my biggest concern. Based on the high resolution CESM simulations that I have seen (McClean et al. 2011; Kirtman et al. 2012; Small et al. 2014; Chang et al. (2020)) the mean state of the Antarctic sea ice is biased thin and not extensive enough. This I believe is one of the main reasons the polynyas do not show up in low resolution simulations, but do in the high resolution. That is, I believe that the polynyas are a result of a mean state bias. I realize this is more relevant to the van Westen et al. 2020 manuscript, but I think this should be addressed here as well. Also, this is a bit of semantic issue. Most of the “polynyas” that form in these high resolution simulations are sort of closed off embayments. As the ice grows in the SH, the Weddell gyre circulates sea ice to the East and eventually it meets up with the Maud Rise coastal area and encloses an open-ocean region. A polynya in my mind is when the area is completely sea ice covered in mid-winter and a hole opens up in the sea ice. Look at animations of daily sea ice concentration. The seasonality is the key here. At least stating the assumption that while polynya formation and the frequency of actual polynya events versus embayments in the CESM in these simulations may not be realistic, these are the processes behind this particular model simulation.

Chang et al. (2020) Under review iHESP project paper.

Kirtman, B. P., et al. (2012), Impact of ocean model resolution on CCSM climate simulations, *Clim. Dyn.*, 39, 1303–1328.

McClean, J., et al. (2011), A prototype two-decade fully-coupled fine-resolution CCSM simulation, *Ocean Model.*, 39, 10–30.

Small, R. J., et al. (2014), A new synoptic scale resolving global climate simulation using the Community Earth System Model, *J. Adv. Model. Earth Syst.*, 6, 1065–1094, doi:10.1002/2014MS000363.

3. The box model description is very confusing. Some of the terms in the equations are not described. While this might be in the Martinson et al. 1981 paper, some more detail should be repeated here. I guess the Martinson paper came up with the convention of h and $H-h$ for the layer thicknesses. I would prefer h_1 and h_2 here. Similarly Regime I and III only have T , S , and ρ instead of T_1 , S_1 , ρ_1 , and T_2 , S_2 , and ρ_2 . The T_{b1} , S_{b1} , and T_{b2} , S_{b2} variables are introduced in the equations but not explained. I see these are mentioned later on in section 3.1. I think it is also very important to highlight what is different in the model description section from the Martinson et al. 1981 paper. Is it just that you used basically the same model, but with different forcing?

The rest of my points are fairly minor.

4. What about Q_{io} ? I think this is more important when there is ice present in terms of forcing the ocean rather than Q_{ia} . Or does Q_{oa} include Q_{io} somehow? You have Q_{io} from the CESM simulations already.

5. Are there any other freshwater flux observations around Antarctica? Are these open ocean only or ice-ocean?

6. I'm curious why you used fitted background T and S . You have the data from the CESM run, so why not use that?

7. Figure 4 is missing labels on the density contours and the salinity axis.

8. Figure 5 (and others). Why did you plot thickness as a measure of polynya presence? The definition is concentration based. Panels b and e in Figure 5 are not that helpful. The thickness is the same every year. The "shading" indicates there is a constant polynya in panel e right? Where on the T - S curve is the "active" polynya. It should only be during Regime IV, i.e. while on the "straight" line between density 1027.8 and 1027.7? Actually, how can you say MKH is a polynya? It looks ice free the whole year? I'm very confused here.

9. Figure 6. Can you indicate the actual polynya years in CESM here? Is it every year?

Similarly for Figure 7.

10. In the summary and discussion, the authors mention that this box model is “slightly extended”. This needs to be expanded. You could not replicate the results from Martinson as I understand it. More detail here on what is new about your study!

11. Also, I sort of feel like it is missing a big “punchline”. What have you added to the body of literature on Maud Rise polynyas here? Is it just the enhanced role of subsurface heat accumulation? The results from the van Westen work were not simulated with the box model, but I think more needs to be added here to explain what the box model gives you and adds to the story.

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-63>, 2020.