Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-63-AC1, 2020

Interactive comment on "Multidecadal Polynya Formation in a Conceptual (Box) Model" *by* Daan Boot et al.

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Received and published: 22 September 2020

MS-No.: OS-2020-63
Title: Multidecadal Polynya Formation in a Conceptual (Box) Model
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Point-by-point reply to reviewer #1

September 22, 2020

We thank David Bailey for his careful reading and for the useful comments on the manuscript.

Specific concerns:

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?*

   Author's reply:
   In the revision we will include more information on the simulation of the Community Earth System Model (CESM). The results in van Westen and Dijkstra (2017) only cover the first 200 years of the simulation, while we use a period between model years 150 – 250. The complete CESM simulation (300 years) and full details can be found in van Westen et al. (2020) (https://doi.org/10.1038/s41598-020-71563-0).

A present-day forcing (of the year 2000) is closer to current observations compared to pre-industrial control simulations (such as in CMIP). The author is right that the model is not in equilibrium, but the model has a spin-up period of 150 years. The upper ocean temperatures (1000 m) are fairly in equilibrium (see Figure S2 in van Westen et al. (2020)). Any drift can be removed by subtracting a linear or quadratic trend. Note that pre-industrial control simulations of CMIP also are not in equilibrium, as the deep ocean fields take much longer time (millennial timescales) to equilibrate.

Changes in manuscript:
We will include a brief summary of the CESM simulation and equilibration and motivate the present-day configuration. For a complete overview of the full CESM results, we will refer to van Westen et al (2020).

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 realise 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.

Author's reply:
Thank you for the references. In van Westen and Dijkstra (2020a) they analyse a companion CESM simulation at a 1° resolution for 1300 years. One major difference between a low-resolution CESM (LR-CESM) and high-resolution CESM (HR-CESM) is the background stratification. The background stratification is much stronger in the LR-CESM, hence no deep convection developed over Maud Rise. This does not imply that no convection events occur in a low-resolution model. Dufour et al. (2017) demonstrate a different background stratification between a high- and low-resolution climate model which alters the periodicity of deep convection in the Weddell Sea.

Regarding the sea-ice thickness bias, van Westen and Dijkstra (2020b, https://os.copernicus.org/preprints/os-2020-33/) show the climatology of the sea-ice thickness over Maud Rise (their Figure 1d). The August sea-ice thickness is varying between 30 – 80 cm (90% range) with a time mean of about 53 cm for non-polynya years. Such values are also reported in an observation-model study in this part of the Weddell Sea (Holland et al. (2014), https://doi.org/10.1175/JCLI-D-13-00301.1). These daily-averaged (and also monthly-averaged) sea-ice fields clearly show that the polynya appears within the sea-ice and not by embayment as suggested by the reviewer.

Changes in manuscript:
We will discuss the results of the references provided by the reviewer regarding the potential biases. We will include an analysis of the daily-averaged sea-ice fields for model year 231 to show how these polynyas form in the CESM.

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 h1 and h2 here. Similarly Regime I and III only have T, S, and rho instead of T1, S1, rho1, and T2, S2, and rho2. The Tb1, Sb1, and Tb2, Sb2 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?*

   Author's reply:
   The discussion of the equations was indeed quite minimal. The different terms in the equations will be discussed more elaborately. We will pay special attention to the sea-ice equation (following reviewer comment 2), and the horizontal advective fluxes related to T_{b1}, T_{b2}, S_{b1} and S_{b2}.

   The convention of h, and $H - h$ is indeed from Martinson et al. (1981). The suggestion to change this to h_1 and h_2 for the two layers is followed with a depth H of the total layer.

   The extensions to the Martinson model are: a dynamic subsurface layer, the horizontal advective fluxes, the forcing, and some parameter values have been

changed. This issue is addressed in section 3.1. We will include this also in section 2.1.

Changes in manuscript:
The equations will be discussed more elaborately. The convention for the layer depths will be changed. The extensions/changes made with respect to the Martinson model will (also) be addressed in section 2.1.

Minor comments:

1. *What about Qio? I think this is more important when there is ice present in terms of forcing the ocean rather than Qia. Or does Qoa include Qio somehow? You have Qio from the CESM simulations already.*

   Author's reply:
   Q_{io} is modelled via a heat transfer flux given by the term: $\rho_0 \times C_p \times K(T - T_f)$ in equations 4a, 4c, 5a and 5c (as was done in Martinson et al., 1981). This means Q_{oa} does not include Q_{io}.

   Changes in manuscript:
   In the model description (2.1) the equations will be discussed more elaborately. There it will also be made clear that this term represents the heat flux between the sea ice and the ocean.

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

   Author's reply:
   There are several observations, see for example in Trenberth et al. (2007) where they show evaporation minus precipitation in their Figure 3. The value seen there, corresponds to the value used in this paper. This is based on ERA-40 data. This includes reanalyzed data for open ocean and ice-ocean.

Changes in manuscript:
No changes necessary.

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

   Author's reply:
   We are using a highly idealized model and it is not suitable to reproduce the CESM simulation accurately. We believe the model is suitable to test high level hypotheses: a subsurface accumulation of heat is important for polynya formation, and a periodic subsurface accumulation of heat results in periodic polynya formation. It is more suitable to use an idealized subsurface heat and salt flux for the model than the noisy CESM data.

   Changes in manuscript:
   The above reasoning will be included in the revised text.

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

   Author's reply:
   This was done purposefully to clearly show the different cycles in the T-S space. If we would have used the actual values for salinity and density, the plots would overlap, making the plot unclear (see for example Figure 9c which includes the three cycles also shown in Figure 4).

   We did include the temperature values since these are important for the onset of sea-ice growth. The salinity values are not that important for showing the general behavior. Since we do not use actual salinity values, we cannot compute the density with equation 1. Therefore, there are also no density values on the contour lines. We made sure that the scale of the salinity axis is the same for each cycle, so they can be compared.

   Changes in manuscript:
   We will make changes in the caption and the main text to clarify this issue.

5. *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.*

   Author's reply:
   The model does not determine sea-ice concentration. The sea-ice thickness is plotted in Figures 7-9 to show the difference between polynya and non-polynya periods. For consistency we also plotted the sea-ice thickness in Figure 5. Again for consistency, we included the shading. Furthermore, it also shows that the sea-ice thickness remains constant for these two cases (in Figure 5).

   The polynya definition we are using for this model is shown on pg. 10 l. 15. Following this definition, we can say that MKH is basically one long polynya period, with polynya formation every year. Every year there is a little bit of ice formation (about 10 cm), and due to the brine rejection of this sea-ice formation, the water column becomes unstable, mixing warm waters to the surface and melting the sea-ice. Case MKH can be compared with the 2-overturn cycle in Figure 4. At point D (in Figure 4) the polynya has formed. This point is not clearly visible in Fig. 5f, but it is located around (T, S) = (0.6, 34.7), around the 'kink' between the two straight lines.

   The results of case MKH are of course odd, but that is because we believe important physics are missing in this case. We do call it a polynya to be consistent with the other cases.

Changes in manuscript:
We will include a more extensive discussion of cases MKL and MKH where we will address the significance of Figures 5b and 5e, and why we do call it a polynya period.

6. *Figure 6. Can you indicate the actual polynya years in CESM here? Is it every year? Similarly for Figure 7.*

   Author's reply:
   The polynya years are addressed in the caption of Figure 6. The polynya years for Figures 7-9 are also clear from the shading in subfigures a and b.

   Changes in manuscript:
   The color codes corresponding to polynya years will be added in the captions for Figures 6-9.

7. *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!*

   Author's reply:
   The main extension of the model is the inclusion of subsurface advection of heat and salt for both layers. Although we couldn't exactly replicate results from Martinson, we considered a case in which there is no subsurface advection (e.g. reference case). This set-up was the original set-up of Martinson and we found similar (not identical) results as reported in Martinson et al. (1981). All the other experiments have subsurface advection in the layers.

   Changes in manuscript:
   We will discuss and highlight the extensions of the original Martinson model in the discussion.

8. *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.*

   Author's reply:
   Our starting point is the Martinson paper. In our view, this paper has a large influence on the general paradigm on Maud Rise polynya formation (i.e. deep convection induced by surface processes). What we have shown is that this model is not capable of simulating multiple polynya events as is seen in observations (e.g. the 1970s, 1980, 1994, and the 2016-17 event). When this model is extended, with most prominently (periodic) subsurface heat and salt accumulation, the model is capable of simulating multiple events. This is an improvement of the original model, which also sheds another light on the processes responsible for polynya formation.

   Our model is very simple and includes only a few basic physical processes compared to that of the high-resolution CESM. Nevertheless, our model is capable of qualitatively reproducing the CESM simulation. This suggests that the most important physical processes are included in our model. The results suggest that subsurface heat and salt accumulation play an important role in polynya formation. Processes which have not been discussed often in the 'polynya literature'. Most studies only investigate surface instabilities (e.g. brine rejection) rather than subsurface processes. Surface forcing, which is also incorporated in the box model, is random and not causing polynya formation. Surface related processes cannot completely explain polynya formation nor its periodicity (if such a multidecadal period exists in the Southern Ocean, see discussion van Westen and Dijkstra (2020a)).

Changes in manuscript:
An extra paragraph will be added to highlight these findings and that the subsurface related processes need to be investigated in future research of the Maud Rise polynya.

References:

– Holland et al. (2014), Modeled Trends in Antarctic Sea Ice Thickness, https://doi.org/10.1175/JCLI-D-13-00301.1

– Trenberth et al. (2007). Estimates of the global water budget and its annual cycle using observational and model data, https://doi.org/10.1175/JHM600.1

– van Westen et al. (2020), Ocean model resolution dependence of Caribbean sea-level projections, https://doi.org/10.1038/s41598-020-71563-0

– van Westen and Dijkstra (2020a), Multidecadal Preconditioning of the Maud Rise Polynya Region, https://doi.org/10.5194/os-2020-25

– van Westen and Dijkstra (2020b), Subsurface Initiation of Deep Convection near Maud Rise, https://doi.org/10.5194/os-2020-33