Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-25-AC1, 2020

Interactive comment on "Multidecadal Preconditioning of the Maud Rise Polynya Region" *by* René M. van Westen and Henk A. Dijkstra

René M. van Westen and Henk A. Dijkstra

r.m.vanwesten@uu.nl

Received and published: 6 July 2020

MS-No.: os-2020-25
Version: Revision
Title: Multidecadal Preconditioning of the Maud Rise Polynya Region
Author(s): René M. van Westen and Henk A. Dijkstra

Point-by-point reply to reviewer #1

July 6, 2020

We thank the reviewer for his/her careful reading and for the useful comments on the manuscript.

Major comments:

1. *Link with the Southern Ocean mode (SOM): - Please explain what is the SOM. It's not the NAO or the SAM. As far I can see it has been investigated in handful of papers, so most readers will have no idea what this is. - The connection between the MRP and the SOM is unclear. What the figures suggest (Fig.6b, Fig. 7) is a connection between the MRP and the area in the red box (50S-60S, centered on 30W). Then, the OHC in the red box is correlated with the SOM. It seems like a 2 step connection. Perhaps this reflects me not understanding what is the SOM, but what does the link with the SOM provides here?*

   Author's reply:
   To provide more information on the relation between the SOM and SAM, we have already performed additional analysis. First, we analysed the SOM using the stand-alone POP simulation which was first used to detect the SOM (Le Bars et al. 2016) using a Principal Component Analysis (PCA) of the upper ocean

temperature fields in the Southern Ocean. A large-scale pattern of variability is found then with a period of about 40 – 50 years. In the stand-alone POP model, the associated PC displays the same multidecadal variability as the SOM index in Le Bars et al. (2016). This multidecadal variability is not related to any atmospheric variability since the atmospheric forcing is seasonally varying in the stand-alone POP.

Secondly, the same PCA (as above) is done for the CESM (fully coupled climate model). The first principal component displays a 25-year period and this period is significant against red noise in the CESM. The SOM index time series contains much more noise compared to the first principal component. The noise in the SOM index is likely related to atmospheric variability (which is absent in the stand-alone POP). Therefore, we do not use the SOM index in the revision anymore but the first principal component time series of the PCA.

Finally, we determined the SAM index in the stand-alone POP and in the CESM. The SAM index is constant in the stand-alone POP. For the CESM, the SAM index displays inter-annual variability over the 101-year period. In both the stand-alone POP and CESM results, we find no relation between the SOM and SAM.

Changes in manuscript:
We will include a new section in the revision: The Southern Ocean Mode. In this section, the PCA analysis is described and we motivate why the SOM is different from the SAM. In the revision, we will use the first principal component of the PCA to measure the SOM instead of the SOM index. All figures and results will be changed accordingly.

2. *The computation and interpretation of Pbrine are unclear. I understand why Pbrine is in Sv psu for comparison with the other fluxes, but could it be also given in m of sea-ice (thickness). This would be a more useful measure.*

Pbrine contributes to increase the salinity of the upper layer (during non-polynia years). From Fig. 3, salinity in the upper layer decreases during non-polynya years. So what is the point of developing so much the Pbrine diagnostics when it cannot explain the behavior observed in Fig. 3 and cannot be a major contributor to the balance? More generally, I do not get what is the main outcome of the salinity analysis. The salinity stratification delays the destratification over Maud Rise by working against the temperature changes?

Author's reply:
In the simple 1D-box model in Martinson et al. (1981), the water column overturns due to brine rejection. The salt budget analysis in CESM shows that brine rejection is not a major contributor to the balance and, from this, it is not likely that brine rejection leads to the overturning of the water column in the CESM.

Changes in manuscript:
We will rewrite and clarify this section in the revision. The Pbrine time series will be replaced by the sea-ice thickness time series. In addition, we will determine the surface salinity averaged over the Polynya region and include this in the revision. The results and figures will be changed accordingly.

3. *Page 14: "Hence, the frequency of occurrence of the MRP events is related to the SOM variability through the advection of the subsurface OHC anomalies in the Weddell Sea. In this case, a preferred frequency of convective events is induced through preconditioning, which is around 25 years in the CESM."*

This is probably the main point of the paper, but this needs clarification. Are you suggesting that the 25 years periodicity of the SOM set the 25 year period of the MRP (with a 10-year advective delay)?

If so, why is it just the 25 year period that is selected as the SOM exhibits *other peaks (at 34, 17, 5 yr in Fig. 4b, the 25 years peak is actually not outstanding). This suggests a selection mechanism. I could speculate that there must be a minimum threshold for the subsurface heat content build up in the MRP to trigger a convective event. So possibly 5 and 17 years are not long enough. If 25 years is enough to get to instability, the 34 years peak in SOM would never show up in the MRP. Let's assume 2C is the critical temp difference to get instability (Fig. 8c). Could we say that, at the rate of convergence seen in Fig. 8c (about 20-10 TW) , it takes $\sim$25 years to build on a 2C difference?*

My main point is that the argument cannot just be "a 25 year timescale in SOM translates into a 25 year timescale in MPR". There is another effect for selecting the timescale.

Going further, this selection mechanism may fully determine the timescale. That is, the SOM acts as a white noise providing variability on all timescale, no need for a peak in SOM. The build up of the subsurface OHC to instability requires 25 years, and only this frequency shows up in MRP.

Please clarify

Author's reply:
We agree that the 25-year period in the SOM index is not the dominant variability and that variability associated with the 'other peaks' in the SOM index could in principle also induce MRP formation (with a time delay) if the critical temperature difference is reached, as suggested by the reviewer. We have already conducted a PCA regarding the SOM (see also Major comment 1) in the CESM and we find that several of these 'other peaks' in the SOM index are likely related to atmospheric variability. The PC1 time series of the SOM clearly shows the dominant 25-year period which is significant against red noise (99%-CL).

Changes in manuscript:
We will include results of the PCA analysis of the SOM and use this PC1 instead of the SOM index (see also Major comment 1). We will update the results and figures accordingly. In addition, we will include more lag-correlation patterns between the SOM and the subsurface ocean heat content fields to demonstrate the propagation of heat anomalies towards Maud Rise.

4. *page 5, line 10: I do not understand why particles older than 10 years are removed. Please explain this choice. This is even more surprising that 10 years misses the most interesting peak seen for the red box in Fig. 7.*

   Author's reply:
   This is indeed a subjective choice. We have already backtracked the particles for a longer period of time. As suggested by the reviewer, we will extend the plots (to 15 years); the peak (9 – 12 years) for the red box is now visible for all three plots.

   Changes in manuscript:
   We will replace the earlier results by those in which the particles are backtracked for a longer period of time.

Minor comments:

1. *Fig. 2: what is the thick black line in panel b)? Same as in panel a)?*

   Author's reply:
   This is the boundary of the modelled MRP during September model year 181.

   Changes in manuscript:
   We will clarify this in the main text and in the caption of the figure.

2. *Page 5, line 5: "The temperature, salinity and pressure dependency are taken into account when calculating the local density and heat capacity (Millero et al.,*

1980; Sharqawy et al., 2010)."

Does it make sense to do this? Does the model account for variations of density and heat capacity? I don't know the details of CESM, but many models assume that the heat capacity is constant and also assume that, in the heat budget, the density is constant. Such that the heat content of a grid cell is ρ_0 Cp dz(k) T(k) (dz(k) might vary due to stretching of the vertical coordinate). So are you introducing an inconsistent and unnecessary complication?

Author's reply:
Due to storage limitations, the density was not written out in the first part of the simulation. Therefore, we have chosen to compute the density (using T,S,P) as mentioned in the manuscript. We have later verified these values of the density with the standard CESM density output and these values were similar. The density and heat capacity in the CESM are not constant but local variations in density and heat capacity are very small.

Changes in manuscript:
No changes in text.

3. *Fig. 6b: how long are the trajectories shown here? And how many of them are plotted?*

   Author's reply:
   The particles were backtracked for 10 years and the particles were initially released at a $1^\circ \times 1^\circ$ grid, which means a total of 30 particles per initial depth level.

   Changes in manuscript:
   We have revised the caption accordingly.

4. *Fig. 3: Panel b is quite cluttered and difficult to decipher. Possibly you could remove the dashed black line. The information about the anti correlation between the upper and lower-layer temperatures is quite obvious in panel c)*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   The SOM index is shown in the new Section 'The Southern Ocean Mode' and is removed from Fig. 3.

5. *Still Fig. 3: there is a shift between the panel a) and panel c). At first look, I thought there was a shit between the variables with lead-lag effects. Until I realize that panel c) is shifted relatively to panel a) because of the color bar. It would be convenient to line up the 2 panels.*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   The figures will be aligned.

6. *Page 13, line 1: " is A measure"*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   Corrected.