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Discussions

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

Anonymous Referee #1

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This paper explores the mechanism of the Maud Rise polynya in the CESM model. The MRP in CESM re-occurs quite regularly every ∼25 years. The authors argue that the MRP is linked to the Southern Ocean Mode, which provides the 25 years timescales. The connection between the 2 phenomena is achieved through advection of subsurface heat content anomalies from the southern Atlantic to the Maud rise region.

The study is overall interesting and well conducted. The obvious limitation is that this is a one model study, in a domain where models have shown little consistency. Nonetheless, it is worth of publication to provide a possible avenue to investigate other models or the real world.

The manuscript could be significantly strengthen by clarifying the link with the SOM and the mechanism setting the timescale.

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?

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?

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 25year 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 (about20-10 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.

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.

Minor points:

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

- 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 rho_o 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?

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

- 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)

- 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.

- page 13, line 1: " is A measure"

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