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Interactive comment on "Subsurface Initiation of Deep Convection near Maud Rise" *by* René M. van Westen and Henk A. Dijkstra

Anonymous Referee #2

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General Comments:

This study investigates the initiation of deep convection over Maud Rise seamount, as well as the resultant open-ocean polynyas that appear in the region during austral winter. The authors highlight key oceanographic processes underlying the recently observed 2016-2017 polynya event, comparing high-resolution model output with observation-based data. This research topic certainly deserves attention; the seemingly rare modern occurrence of Southern Ocean deep convection, as well as its role in the high-latitude climate system, is still not fully understood. This study argues that deep convection is partially initiated by subsurface static instabilities, an explanation that contrasts from the surface-based triggering mechanisms identified

in prior high-resolution modeling studies (Kurtakoti et al., 2018) and observations (Cheon and Gordon, 2019; Campbell et al., 2019). To support this novel interpretation, the authors show that polynya formation is preceded by a weakening subsurface stratification in both CESM and Mercator model output. I found these analyses interesting, but I have two primary concerns with the study's conclusions as currently presented. First, it is not clear from the results that subsurface instabilities are a robust cause of deep convection. Concurrent changes in near-surface salinity and temperature, driven by atmospheric variability, may be the more dominant causal mechanism. Second, more attention should be given to temporal variability in the subsurface heat reservoir, given the outsized Weddell Deep Water (WDW) warming trends seen in some climate models. Acknowledging this potential model bias is especially important when considering the preconditioning mechanisms for Maud Rise polynya formation. I further detail each of these topics in my specific comments and suggestions below. If these issues are sufficiently addressed with additional analyses, I believe the model-to-observation comparison conducted in this study can provide substantial insights. Accordingly, I recommend this paper be published after major revisions are made.

Specific Comments:

1. The growing subsurface instabilities that precede the polynya in CESM model year 231 (Fig. 3a, 3b) are intriguing. It would be great if the authors could discuss in further detail what mechanism is behind the growing instability. Is it related to the multidecadal buildup of subsurface heat, mentioned in pg. 12, L24-25? Or is a shorter-timescale process more relevant here? Pg. 8 would be a good place to discuss this.

2. The aforementioned subsurface instabilities also appear to occur in the model years preceding year 231 (Fig. 3c). If these instabilities play a role in initiating

convection, why are there no polynyas during this previous time period? Are unfavorable near-surface conditions inhibiting deep convection from above? The time series of wind stress curl, as shown in Fig. 3a, could be useful if it is extended to include this earlier time period.

3. Pg. 8, L28-29 and pg. 9, L1-2: Wind stress curl is associated with upwelling, turbulent mixing, and sea-ice divergences. The manuscript would benefit from the authors explicitly quantifying the upwelling magnitude associated with wind stress curl anomalies. For instance, the horizontal and/or vertical Ekman velocities can be inferred from wind stress curl (e.g. Campbell et al., 2019, Methods, salinity fluxes from upwelling). This quantity could help contextualize the near-surface destratification shown in Fig. 3.

4. When comparing subsurface convection in CESM and Mercator output (Pg. 11, L29-35), it is important to acknowledge the magnitude of ocean heat content variability in the models used. Climate models are known to be prone to excessive subsurface heat accumulation, which has been attributed to freshwater forcing biases (Stössel et al., 2015) and weak parameterized mixing under sea ice (Heuzé et al., 2013). For instance, are subsurface warming trends between the simulated polynya events in CESM (Pg. 2, L24-25) consistent with the observed .032 K/decade trend in Weddell Deep Water temperature between 1977-2001 (Smedsrud, 2005)? What about the Mercator output? If not, the model-to-observation comparison could still be useful, but the difference must be acknowledged to properly inform the interpretation of the data.

References:

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