Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-33-SC1, 2020

Ocean Science
Discussions

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

Ethan Campbell

ethancc@uw.edu

Received and published: 28 June 2020

I appreciate the authors’ submission of this interesting work to Ocean Science. The journal offers members of the broader scientific community the opportunity to comment on the preprint during the eight-week open discussion period. As the lead author of Campbell et al. (2019), a study also addressing the formation mechanisms of the 2016 and 2017 Maud Rise polynyas, which is cited (and challenged) throughout this work, I feel compelled to offer an observationally-grounded perspective on some key aspects of this study. My comments below are not meant to be an exhaustive review, but I hope that the authors and editor find them useful.

The primary conclusion of this study is that the 2016 Maud Rise polynya was initiated

by subsurface overturning, which the authors identify through the appearance of deep potential density (sigma-theta) inversions in a 1/10° CESM control run (with polynya years picked to represent an analogue for 2016-17 conditions) as well as Mercator ocean reanalysis fields.

Simply put, the simulated phenomenon that is the focus of this study has not been observed in the real ocean, to my knowledge, and is in direct conflict with decades of hydrographic observations from the Weddell Sea, including profiling float measurements collected before, during, and after the 2016-2017 polynyas.

First, the cause of the subsurface destratification seen in the authors’ CESM simulation is a multidecadal buildup of heat in mid-depth layers (200-1000 m) of the Weddell Sea, as detailed in their companion submission to Ocean Science Discussions (van Westen and Dijkstra, in review). Similar periodic behavior has been documented in other climate models on various timescales (Behrens et al. 2016; Reintges et al. 2017). While many in the modeling community regard this behavior as spurious and unrealistic (e.g., Heuzé et al. 2015; Held et al. 2019), I agree with the authors’ view that the underlying cause of these cyclical, heat-accumulation-induced polynya occurrences is worth understanding. These cycles certainly have an outsized impact on intrinsic global climate variability in some models, as the authors and others have demonstrated.

However, oceanographic observations do not support this behavior. Repeat hydrographic surveys from 1984-2008 along the Greenwich Meridian, crossing Maud Rise, show no sign of marked heat accumulation following ventilation during the massive 1974-1976 Weddell polynyas (Fahrbach et al. 2011). The small warming trend observed is over one order of magnitude smaller than that examined in the authors’ companion submission following polynya events (van Westen and Dijkstra, in review), and large decadal fluctuations complicate the detection of even that small observed trend. In fact, local rebound in heat content near Maud Rise following the major 1974-1976 polynyas occurred by 1984 at the latest (Smedsrud 2005), and likely in shorter time. Analysis of about 3,000 temperature profiles from 2002-2017 from ships, floats, and instrumented seals indicated that there was no local buildup of mid-depth heat leading to the 2016-2017 Maud Rise polynyas (Campbell et al. 2019; see section 'Sub-pycnocline temperature records' and Extended Data Fig. 9). These substantial differences between models and reality unfortunately limit the utility of the authors' CESM simulation as a direct analogue for investigating the recent polynya events.

In contrast, the published high-resolution model simulations that best reproduce the Maud Rise polynya phenomenon point to upper-ocean destratification from surface salinity anomalies, not subsurface heat accumulation, as most important in triggering polynyas (Kurtakoti et al. 2018; Kaufman et al. 2020). These papers should be cited and discussed. Along similar lines, the authors neglect the observational and theoretical evidence that points to low upper-ocean haline stratification as a critical factor in allowing Maud Rise polynyas to emerge in 2016 and 2017 but not in most other years (Campbell et al. 2019). It is inaccurate to characterize our study as attributing the 2016 polynya solely to intense winter storms (as stated in the authors' Abstract, Lines 1-2; Page 8, Lines 21-23; Page 12, Lines 11-12). Both weak upper-ocean stratification and strong storms appear to have been necessary, and strong storms – which were more frequent than usual in 2016 but are still a regular occurrence in most (non-polynya) years – are apparently not a sufficient condition for polynya formation. While the authors attribute the 2017 polynya to a "weakly stable surface layer" (e.g., Abstract, Line 7), they look elsewhere for the immediate cause of the earlier 2016 polynya. I find this puzzling, as the profiling float measurements examined in Campbell et al. (2019) show that a "weakly stable surface layer" existed both prior to the 2016 polynya as well as during the following winter.

Surface-driven deep convection is a phenomenon that has been well-documented in decades of observations from the North Atlantic and Mediterranean Sea (e.g., de Jong et al. 2012; Testor et al. 2018). Its theoretical underpinnings are also reasonably well-understood (e.g., Marshall and Schott 1999). In my view, there is little reason to expect that deep convection in the Weddell Sea is not similarly initiated by buoyancy

loss and/or dynamic perturbations in the upper ocean. Indeed, this is the canonical model for the formation of polynyas near Maud Rise (Martinson et al. 1981; Motoi et al. 1987). However, if the authors wish to assert that subsurface-initiated deep convection is of real importance, I would suggest critically engaging with literature that may offer a theoretical basis for this mechanism. Harcourt (2005) and Akitomo (2019), for instance, describe subtle processes by which subsurface overturning may be initiated through nonlinearities in the seawater equation of state, but it is important to recognize that this is a fundamentally different sequence of events than that which seems to be occurring in the authors' CESM run.

Regardless of the theoretical feasibility of subsurface-initiated overturning, the phenomenon that the authors highlight from their CESM run is unlikely to have occurred in 2016. In the attached Figure 1 (see below), I have plotted potential density profiles from the Argo float 5904471, which was present at Maud Rise during the recent polynyas (Campbell et al. 2019). This figure is a direct comparison to the authors' Figure 2c from their CESM run, which shows a statically unstable profile below about 75 m prior to polynya formation. In contrast, the observations show that potential density increased steadily with depth, including immediately prior to the 2016 polynya, as is typical throughout the world ocean. While the CESM profile seems to be characterized by a bolus of mid-depth heat anomalies, which create a remarkably thick potential density inversion layer, these features are unsurprisingly absent in the observations. (This is to say nothing of the model's twofold bias in its deep-to-surface potential density difference, which is greater than 0.2 kg/m3 in the model but is no more than 0.1 kg/m3 in the observations, or the inappropriate use of surface-referenced potential density [sigma-theta] to characterize inversions when a locally-referenced potential density should be used instead.)

The authors seem to dismiss the possibility of using the float observations to conduct an analysis similar to their assessment of CESM model output and Mercator reanalysis data, stating that "these Argo float observations are too sparse (every 10 days) to analyse the oceanic state (and e.g. convection)" (Page 9, Lines 28-30). In the context of investigating preconditioning for a polynya, this is inaccurate. The oceanic state – particularly below the mixed layer – does not vary substantially on time scales less than 10 days, as seems to be indicated by the authors' own analysis of the CESM output and Mercator data. The float data are plenty useful and are, in fact, the best records we have on conditions in 2016 and 2017 at Maud Rise. (It is important to note here that the Mercator reanalysis data is poorly constrained in the ice-covered Weddell Sea, and should be approached with greater caution than is done here. Comparison with a single float profile (Figure 4b) – towards which the reanalysis is likely nudged – is not an adequate validation of its skill.)

Ultimately, the float observations analyzed in Campbell et al. (2019) as well as the body of previous work on Weddell Sea polynyas and hydrography offer ample evidence that model-observation disagreement is severe in this region and in the context of this phenomenon. With this in mind, I would argue that it is probably counterproductive to interrogate the causes of observed polynya events through direct comparison with a model that behaves very differently than the real world.

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Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-33, 2020.

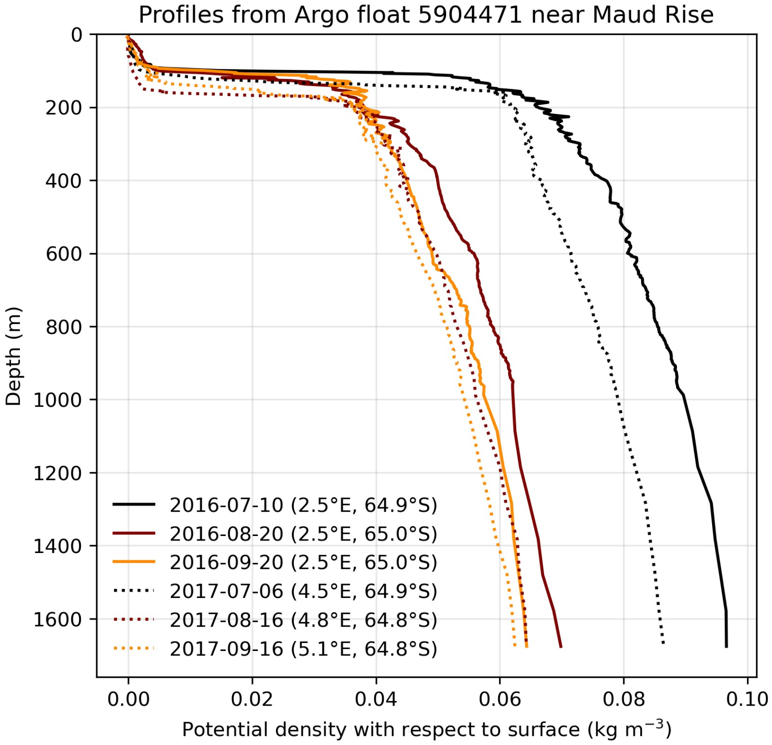

Fig. 1. Float observations presented as a point of comparison to the authors' Figure 2c.