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Interactive comment on "Characterization of Ocean Mixing and Dynamics during the 2017 Maud Rise Polynya Event" *by* Jhon F. Mojica et al.

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Authors response to the reviewer's comments on Manuscript OS-2019-41 'Characterization of Ocean Mixing and Dynamics during the 2017 Maud Rise Polynya Event' by Mojica et al.

The authors would like to thank the reviewer for evaluating our manuscript and for the suggestions which helped to improve the clarity and the quality of the paper. Our point-wise response is detailed below in blue. Anonymous Referee # 2 Received and published: 30 July 2019 This is an interesting paper, one that I enjoyed reading. In general, I find that the authors have explored some new ground with the recent Weddell Sea polynya, and I believe that this paper could eventually be publishable. On the other

hand, I do have some specific comments, enumerated below (some more serious than others), that I hope can be used to improve this paper.

Thank you for all the comments and suggestions. We respond to them below point-by-point.

Line 33: Should be 'Turner' instead of 'Tuner'.

Yes, changed

Line 37: The cyclonic circulation, generated mainly by the wind stress curl, does not produce the upwelling alluded to here that is said to be due to the large-scale overturning. The overturning is presumably due to convective processes caused by vertical instabilities generated by a dense surface layer. There are cyclonic circulations driven by the wind in many places in the world ocean, but deep convective overturning doesn't occur in most of them.

We agree. The upwelling concentrates warm and salty Weddell Deep Water close to the surface, thereby affecting the stratification. We clarify this idea about upwelling and include your statement about the overturning due to convective processes in line 37, following the literature (Campbell et al., 2019).

Line 113 and elsewhere: The authors state that this is 'the first time' that polynya dynamics have been characterized using in situ data. This is clearly untrue, as a paper published in Nature (June 10, 2019; volume 570, pp. 219-225) dealt with many of the same issues raised in the paper under review here. It is possible that the authors do not like the Nature paper or disagree with its conclusions. But it is highly misleading and not even intellectually honest not to even mention the paper in the references. Like it or not, that paper went through a rigorous review process and was published in a major journal, suggesting that the paper likely has some meritorious elements. The authors should at least acknowledge the paper and take issue with whatever parts of it they don't agree with. It is worth noting that the Nature paper used much of the same data (the SOCCOM floats) that are used in this paper.

During the preparation and initial submission of our manuscript, none of those papers was published. Now that they are, we will certainly include their findings when addressing the state-of-the-science in the introduction and we will state clearly the additional contribution our study brings in relation to this new work. For instance, we have found similar results to Cheon and Gordon 2019 and Campbell et al., 2019 with regards to convective mixing. While these two studies describe the ocean and atmospheric interaction over Maud Rise during the 2017 polynya event, we focus on the ocean preconditioning during the years leading up to the occurrence of the Polynya. To do this, we quantified the mixing rates to create a mixing map over Maud Rise. We then compared mixing rates over Maud Rise with those elsewhere to highlight and describe the role of the bathymetry.

We have also deleted the statements on lines 13-15, 108-109, 113, to acknowledge the manuscripts published recently.

Line 185: I doubt that HYCOM does much data assimilation in the winter, since there are no real-time data to assimilate. Thus, while the correlation of model and data might be reasonable in the summer, it is unknown how well the model does in the winter, since there is no baseline for comparison. Since the polynya occurred in late winter, it is hard to trust the model results too much.

We agree that there is a lack of in situ vertical profiles during winter in this region - however, the model still assimilates surface altimeter data. In the absence of vertical profiles, HYCOM uses MODAS (Modular Ocean Data Assimilation System) to generate synthetic profiles (T and S) that are consistent with the along-track altimeter SSHA. Where there is no data to assimilate, the model uses conservative advection routines and conserves salt.

However, in light of the recently published papers and given the fact that the values of lateral fluxes are negligible, we decided to shorten the section using HYCOM outputs.

We reorganized subsection 2.2 and 4.2.2 to reflect this, and we emphasized instead the vertical processes, which appear to be the most relevant.

Line 256 (equation 9) and line 301: This formulation of FH is reasonable if there is no shear to the velocity field. However, this idea is based on homogeneous turbulence, and if there is shear this formulation won't work unless the shear is very weak. How weak? It is unknown, but the authors should attempt to estimate how weak it can be for this to be a useful parameterization.

As seen in the vertical distribution of current velocities (figure 4), shear in this area is weak, because the velocity field has a small velocity gradient over the water column. Velocity values decrease with depth by 0.04 cms-1. We discussed these values in section 4.2.2 lines 373-381. Therefore, our representation of FH is appropriate. We will note on line 256 that there is negligible shear to the velocity field.

Line 309: I believe that the authors mean _ (with subscripts 2015 and 2017) instead of _ here.

Yes, we changed the writing format to emphasize the subscripts.

Lines 318 and 328: The use of the conditional 'could' here sounds like pure speculation. Can this be quantified a bit more?

Absolutely, we included values to illustrate the change in salinity and temperature in the surface waters between consecutive profiles previous to and during the Polynya event (average salinity variation $\sim$ 0.01, temperature $\sim$0.02°C, and buoyancy variability of $\sim$ 0.1 cph; and salinity values of $\sim$ 0.03, temperature $\sim$ 0.1 °C and buoyancy $\sim$ 0.3 cph, respectively). This information was included in section 4.1, lines 318-320.

Line 366: The spread in the estimated values of k_ is so large that the values hardly constrain anything; most of the global ocean above the thermocline would fall somewhere in this range.

There are no previous measurements of diapycnal diffusivity at the same location, which is why we refer to the closest records from two previously published works. We constrain the values measured by Naveria Garabato et al., 2004a to shallow waters, in this case the range changes from ∼3x10-4 – 1 x 10-4 m2s-1. We included this information in line 366.

Lines 428-434: This seems like speculation, little else.

We rewrote this paragraph, comparing the heat transfer values (0.1 Wm-2 during the Polynya event to 0.04 Wm-2 in summer at the same depth) from figure 6 with the thermal and salinity composition during the same period (thermobaric coefficient 3.1 during the Polynya event compared to 2.9 in summer at the same depth). In this way, we demonstrate that these convection processes perturbed the thermal barrier and increased the ventilation, thereby helping to produce the Polynya event in 2017. We added this information in lines 428 - 434.

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-41/os-2019-41-AC3-supplement.pdf

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