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Discussions

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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.

Anonymous Referee #2

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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.

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

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 over-turning. 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.

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.

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.

Line 256 (equation 9) and line 301: This formulation of F_H 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 it 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.

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

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

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

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

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