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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.

Ilker Fer (Editor)

ilker.fer@uib.no

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Dear Dr. Mojica,

Thank you for your manuscript on the mixing during a Maud Ride polynya event.

One reviewer has been (very) delayed but ensures me that the report is on its way very soon. Therefore I leave the discussion open for a few more days. I apologize for the delay.

Unfortunately, I find major shortcomings in the methods and the approach. It is therefore crucial that you return a convincing response (to my and the reviewers' comments) that demonstrates how you will satisfactorily address the issues raised. I regret to say that I would not recommend you put too much effort into preparing a revised manuscript

before I make a decision based on your final response in open discussion.

1. The polynya event reported has been presented and discussed in several recent papers in high profile journals, none of which were cited or discussed: Cheon and Gordon Scientific Reports (2019); Jena et al. GRL 2019 paper, https://doi.org/10.1029/2018GL081482; Campbell et al Nature 2019 paper (https://doi.org/10.1038/s41586-019-1294-0). I understand perhaps these papers might not have been available when you were preparing your manuscript; however, now that they are, we cannot be ignorant of the new science.

These are key works on the same event you are analyzing. In the light of these recent papers that explain the polynya formation and maintenance processes, your claims in the abstract (li 13-15), introduction (li 108-109) seem too strong ("...lack of information to a complete description...."). Furthermore you state (li 113): "...for the first time, in situ data, ...". This is not correct, see e.g. Cheon and Gordon 2019; Campbell et al 2019, who also used in situ data.

In summing up my main point 1, given the weakness in the methods (results and conclusions remain unconvincing, see below), I cannot find a new contribution in your paper on the description of the polynya event.

2. There're 3 approaches in the paper: 1) vertical mixing from Thorpe scale analyses of in situ data, 2) lateral mixing inferred from (u,v) fields of a 1/12deg resolution model, 3) heat fluxes from a double-diffusion parameterization. First of all, (1) is highly uncertain with the data at hand. Without a clear presentation of some individual profiles and Thorpe scale analysis, and a discussion of uncertainty, these Krho estimates are not convincing or acceptable. (2) is worked out from model fields in which eddy fluxes are parameterized. Given the parameterizations employed in a model, I am not convinced that the complex physics you are describing can be supported with this approach. At least a thorough discussion of caveats is needed. (3) is not meaningful at all in this system. It results in (double diffusive) heat fluxes close to nil, in a system where you

claim vertical mixing and convection is important. Most contribution to vertical heat fluxes would be turbulent and vertical entrainment during convection is likely dominant.

Overall there is also a serious disconnect between the approaches 1 to 3 above. And the results are uncertain and inconclusive.

Minor comments / clarifications:

title: “Ocean Mixing”- mixing in only indirectly inferred from coarse resolution data, and I am sorry to say, unconvincingly.

abstract, li 16-18: the study did not convincingly present processes of exchange of energy. The three relevant factors, are these shown to contribute to the energy flux, as claimed?

Sect 2.1, in situ data: please tell use how often the floats profile. And what is the sampling rate of C/T/P, the vertical profiling speed and the effective vertical resolution of the data? Is it coarser than the accuracy of 2.5 dbar? How many profiles are analyzed in total, in each region? What is a noise estimate of eddy diffusivity from the Thorpe scale analysis for a typical stratification profile?

Sect. 2.2: HYCOM: You’re using (u,v) fields from 1/12deg resolution HYCOM to infer lateral fluxes. Eddy fluxes are parameterized in such models (I think). This is not described. I am not convinced these lateral fluxes from the model field will provide a description on the physics you’re after. Did you consider using the float data to infer lateral diffusivity?

Sect 3. Ro number does not fit to ocean mixing section. Perhaps move/integrate to li 306 where it is used. After introducing Ro, you proceed to Krho which is very confusing and not well motivated.

li 202: Please reconsider revising "ultimately determines the variability in energy between isopycnals". Perhaps "ultimately determines the vertical stratification in the water column"?

please delete the equation for \detal_T in Eq(3), and introduce it in text simply as (a version of) "...is the Thorpe displacement, the vertical distance needed to move the water parcel from the observed profile to the gravitationally stable, sorted profile".

Li 214-217: This description and comparison of RMS values are very unclear. Please clarify. The maximum of RMS of 0.5 – what is it referring to?

Li 221: Start a new parag for isopycnal diffusion. In the Cole reference, is the name of Eric Kunze misspelled? You are using 1-year time averaging. The motivation for this choice or sensitivity thereof is not discussed. Seasonal variability will be interpreted as eddies.

Li 253: please insert “diffusive” before heat flux

Li 262: replace “diffusion convection” with “double-diffusion” processes

Li 273: “We identify a remarkable change of conditions between adjacent profiles confirming diffusive processes” How is this statement supported by observations? How can you rule out advection? Also the temporal sampling (e.g. number of profiles per months) is coarse (not stated) and there is a lot of interpolation (krigging?) in the figure presented.

Li 298: “below the thermohaline”, you mean below the thermocline?

Li 316-330: Here I note several speculations (e.g., salinity increment from brine rejection, occurrence of diapycnal and isopycnal mixing, change in thermal barrier and energy reservoir, trigger vertical and lateral mixing etc.). Most statements remain descriptive or speculative with no attempt of quantification.

Li 417: kh does not represent lateral energy

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