

Author's response to the editor'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 editor for evaluating our manuscript and for the suggestions, which have helped to improve its clarity and quality. Our point-wise response is detailed below in blue.

Iker Fer (Editor)

Received and published: 29 July 2019

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.

Thank you for giving us the chance to address the lack of clarity in the method and the approach. Kindly find below our point-by-point answers to the comments that were raised. In the updated version of the manuscript currently under preparation, we rigorously address all the questions regarding the methods and approaches that you and the reviewer have pointed out.

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.

As you already mentioned, 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 in lines 13-15, 108-109, 113, to acknowledge the manuscripts published recently.

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 K_{rho} 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.

Regarding the three approaches:

(1) The Thorpe scale is used to identify the vertical overturning scale from fine-scale density profiles (Garget and Garner, 2008). When applying careful data analysis and processing such as we describe in another comment below (see the comment for Lines 214-217 - RMS), the Thorpe scale is a solid and widely used method to quantify diffusivity (e.g. Park et al., 2014). The Thorpe scale, calculated from the SOCCOM profiles, provides a robust approach to identify convection-driven changes in weakly stratified regions. We analyze these variations

to the North of, and over, Maud Rise. The convection-led mixing processes can be seen clearly in the K_{ρ} estimations, which identify when thermal barrier reach shallow waters increasing the ventilation at the surface (Figure 3). We also include Thorpe scale profiles as part of Figure 3 (see below), and analyze these in section 4.2.1 to support our diapycnal diffusivity calculation.

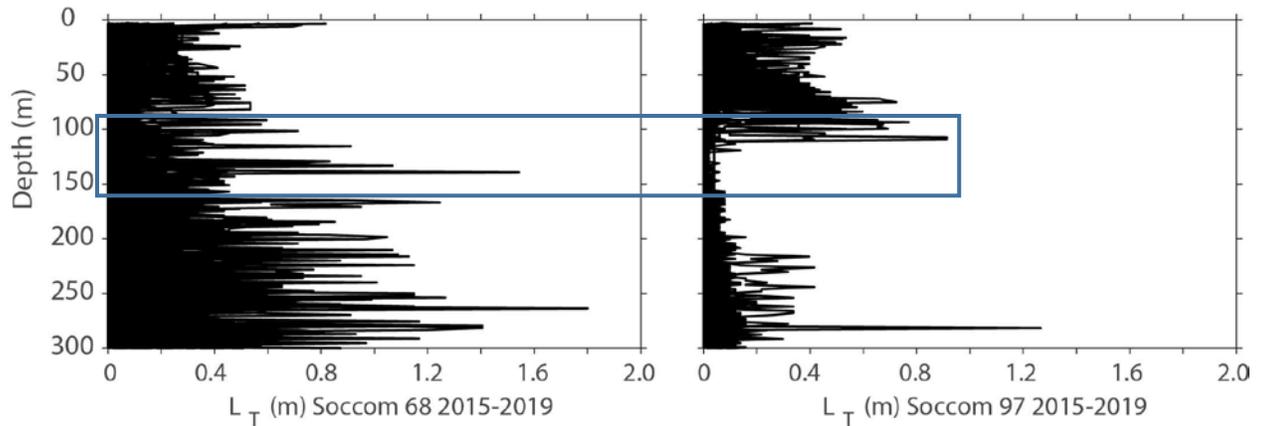


Figure 1. Thorpe scale, float *68 (over Maud Rise) and *97 (North of Maud Rise). (Blue rectangle) Note the remarkable decrease at the thermal barrier depth for float *97 compared to the overturning at Maud Rise. Figure to be included in the manuscript as part of Figure 3.

- (2) We agree with you regarding the current inability of the model to resolve such high-resolution processes, and we have now included a description to highlight the limitations of the model. As you mentioned, we used velocity fields from HYCOM to infer lateral fluxes. However, in light of the recent literature and given the fact that lateral fluxes play a very small role in polynya preconditioning, we will shorten the section on this topic and reorganize subsections 2.2 and 4.2.2 to emphasize our analysis of the vertical mixing processes.
- (3) The values of heat fluxes are small because we only quantified the range across the interface of the thermal barrier. We now include the values across the water column (0 – 1000 m, $\sim 200 \text{ W m}^{-2}$). In this way, we can relate the flux variability to the convective mixing processes we quantified previously, thus connecting approaches 1 and 3 together. We include more information on this in table 1 and describe the new heat fluxes in section 4.2.3.

Following the previous statements, we will connect the 3 approaches through figures 2 and 3 where we see the effects of the convective mixing in shallow water (low stratification, small variability in salinity), and thermobaric convection below the thermal barrier (plumes on the thermobaric coefficients and variability in the diapycnal diffusivity). We will make a short statement on the low contribution of the lateral fluxes in order to give a full description of the oceanic processes that provide preconditioning for the Maud Rise Polynya.

Minor comments / clarifications:

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

We will change the manuscript title to “Characterization of Ocean Convective mixing during the 2017 Maud Rise Polynya Event”. In this way, we clarify our focus subject as the assessment of the ocean conditions that make Maud Rise susceptible to a polynya opening.

abstract, line 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?

In our original figures 3 and 6, we presented a temporal evolution of the thermal barrier during the polynya event. The three factors you mentioned contribute in part to the energy flux. We will indicate in the temporal series the decreasing surface buoyancy that produces convection. This generates instabilities at the thermal barrier which, to maintain a balance, supplies energy via thermobaric processes throughout the water column. In the updated version, we will include a statistical analysis in section 4, to quantify how these variables changed in 2017 compared to previous years, and describe how the decreasing buoyancy broke the thermal barrier.

Line 16-18 rewritten ‘The results reveal that the incidence of convective mixing and thermobaric convection over the Maud Rise drives the exchange of energy in the water column’.

Sect 2.1, in situ data: please tell us 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?

Float profiles: ~10 days. Sampling data binned into 2dbar intervals. Measurement accuracy for temperature: 0.005°C, salinity 0.01. Speed of ascent: 8-10 cm/sec. On average, we consider 140 profiles for each float. We have included this information in Section 2, line 144-147.

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?

Please see previous reply about the use of HYCOM.

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.

Thanks for the suggestion. We moved and clarified the inclusion of the Ro number in section 4.1. Line 299-302. "To assess the regime of the system, we estimate the Rossby number ($R_o = U/fL$, where U is the velocity scale, f is the Coriolis parameter, and L is the horizontal length scale). Considering the MR length scale, $R_o = 0.02$ relates a weak inflow, and the conditions of weak stratification, and upwelling, conducive to the formation of a Taylor cap (Ou, 1991; Alvenson and Owens, 1996)."

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 ndetal_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".

Thanks for the suggestion. We rewrote line 202 following your suggestion, and rewrote the equations; including our original equation 3, as a simple text version (line 204-205).

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?

We calculated the RMS values of the sorting potential density. Dividing by RMS Thorpe fluctuation scales the T and S deviation to the density amplitude of the suspected overturn. The resultant ratios were scored on a scale from 0 - 1, according to the strength of the T-S relationship. Scores below 0.5 were assigned to regions that would be discarded (e.g. regions with loops), in this way, we have T-S relationships that are robust enough to be regarded as a signature of overturning motion. We have included this information in new lines 214-217.

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.

We started a new paragraph for isopycnal diffusion from line 221. We reviewed and corrected the reference. We initially chose 1-year time (Line 232) averaging to get the overall picture of annual salinity evolution and to understand why this process did not fully develop in previous years. We will include a seasonal analysis to identify the fluxes and avoid any misunderstanding with possible eddies. This will form part of section 4.2.2 lines 386-399 in the updated manuscript.

Li 253: please insert "diffusive" before heat flux

Done, line rewritten.

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

Done, line rewritten.

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.

We don't rule out advection. The high salinity and small variability between consecutive profiles (Fig. 2) suggest that advection makes a small contribution. A relevant process for polynya precondition is brine rejection during sea-ice formation, which creates the conditions for convective mixing. Buoyancy profiles (Figure 2), show a destabilized upper ocean due to the increased salinity, as we presented in line 318-320. During the sea-ice melting period, the system tries to balance the subsequent freshening by creating a weak stratification that allows convective mixing in the surface water layer. We include this information on advection in lines 322-325. The observations (temporal sampling) allow us to identify the small salinity variation during the polynya event. In figure 2, there is no interpolation, which is why there is missing data in the regions where the SOCCOM buoys cannot reach the surface because of the presence of ice. We include a mark to identify each of the individual profiles in figures 2, 3, 6, and S1.

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

Yes, changed.

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.

We included a description and quantification of the variability of the surface salinity in lines 316-330 that will help to clarify why we ruled out advection, as described in the previous comment. In addition, we included analysis and statements that support our approach for diapycnal and isopycnal mixing.

Li 417: kh does not represent lateral energy

Yes, kh represents the horizontal diffusivity; we changed this in line 417.