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# *Interactive comment on* "Two superimposed cold and fresh anomalies enhanced Irminger Sea deep convection in 2016–2018" *by* Patricia Zunino et al.

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Review of: "Two superimposed cold and fresh anomalies enhanced Irminger Sea deep convection in 2016-2018" by Zunino, Mercier and Thierry

The manuscript is interesting to read and a nice update on the latest convective activity in the western subpolar gyre. The separation of buoyancy fluxes into different components, including those from Ekman transport, is interesting. However it's not too surprising to see that the Ekman contribution is small given that the horizontal SST gradients are also relative small. Overall I would like to see this paper published eventually, but there is at least one major issue that need to be addressed before.

The paper hangs on the derivation of mixed layer depths and the comparison with previous years published in literature. This comparison is currently troublesome because of the substantially different way the authors derive/define the mixed layer. In fact, some of the derived mixed layers depths do not appear to be associated with actual/recent mixed layers. Although some of the results may be robust to the methods, some others (e.g. max depth per winter, match with predicted MLDs) will clearly have to be adjusted. This should be addressed before publication.

More specifically, in a layer with turbulent mixing all properties, density, salinity and temperature, are homogenized. If the mixing occurred very recently (on the order of days ago), the homogeneous profile will still be visible all the way down to the bottom of the mixed layer. In the literature that is referred to for previous mixed layer depths (de Jong et al., 2012, 2018; de Jong in de Steur, 2016), we therefore always specified that all three properties should be mixed and of the bottoms of the mixed layer identified in each property we take the shallowest as the final mixed layer. Similar criteria were applied by group of Vage et al. in their papers. In the mooring data, as well as Argo, there are cases at the end of the winter where there remains no steplike feature visible in the density profiles at all and where a density criterion would strongly overestimate mixing, while such as steplike feature always remains visible in T and S. Therefore it is even more important to take all variables into account.

The difference between this definition of a mixed layer and that of the authors, which is a density-only criterion, is especially clear in Figure 2. The top three panels show density, salinity and potential temperature profiles from the winter of 2017. The bright blue profile, which the authors identified as having the deepest mixed layer, appears to be somewhat mixed in T and S in the upper 250 dbar (though even that is a bit questionable) but it is clearly stratified between 250 and 1400 dbar. In fact, the stratification in temperature is quite large ($\sim$0.25$^{\circ}$C) for the Irminger Sea. The only profile in the set of four that could (potentially) qualify as having a mixed layer is the greenish profile. This would nearly half the winter maximum mixed layer depth and may also affect how well the predicted MLD match the observations.





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There is code readily available to derive MLD from Argo profiles using all variables (Holte and Talley, 2009; http://mixedlayer.ucsd.edu/). I suggest the authors use this, or some adjustment of their own code, to rederive the MLD for all profiles and adjust the results of the paper accordingly.

My final main comment is that the title could be rephrased to represent the content/conclusions better. The fresh anomaly that seems to be referred to a deep one, the lowering of the halocline. The surface freshwater anomaly, which is discussed in detail elsewhere but is only touched upon here, is was not enhancing convection. It is only the cold surface anomaly that worked to enhance somewhat, but even that is only touched upon. Still, those who have not yet read the abstract may think this paper is about the big surface Sanom currently going around. While in fact, the paper focuses in detail on favorable preconditioning which is not mentioned in the title. So, it is not clear why this title was chosen.

Below are some more minor comments

Introduction

Line 94. “In the Labrador Sea, deep convection occurs almost every year, yet with different intensity. In the Irminger Sea…”. In the Irminger Sea some convection (∼400 m) always occurs as well, and the intensity varies not unlike the Labrador Sea. Please rephrase or add a definition of “deep”.

Data

Why is the TEOS-10 toolbox used, but profiles of theta and practical salinity are still shown instead of CT and SA?

Please explain briefly why 35 is chosen as a reference.

The ERA Interim reanalysis is replaced by ERA5. Best to do a check whether the results are robust to the choice of reanalysis.





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Method

De Boyer and Montégut criterion is not suitable for these profiles as discussed above.

The definition of the Irminger Sea, with 48°W as the limit is rather unusual. The area in Figure 1 southeast of Cape Farewell is not typically referred to as the Irminger Sea as it fall outside of the central Irminger Gyre and profiles here are very likely to have been recently advected from the Labrador Sea. To be more consistent with previous literature it would be better to split this region in three areas: the Labrador Sea, the Irminger Sea and in between the area south of Cape Farewell.

Equation 1 and others. There are periods (.) instead of multiplication symbols.

Results

What is Q3?

Part of the results paragraph will have to be rewritten when MLD are rederived.

Line 268: Mean over which period?

Line 296: This is true only when the upper 600 m already has a density close to that of the layer below (which for example could not be the case when a lot of freshwater is added). Otherwise additional buoyancy fluxes will still be required.

Section 4.4

It would be good to compare fluxes closer to the position of the observed deep MLs. These are sometimes on the very boundary of the box used to calculate the winter flux.

The method used to predict the MLD does not take advection into account. This is counterintuitive because we see advection play a big role throughout winter in the field. The fact that the reanalysis do not quite match with the actual fluxes observed at OOI (Josey et al, 2018) may also be needed to take into account here. It will be interesting to see how much of a match between prediction and observation remains once new

MLD are derived, likely the prediction will overestimate more.

Discussion

Line 366: This was seen throughout the 1990s and is not quite as surprising as the authors state.

Line 397: The Labrador Sea is always more favorably preconditioned, it is quite visible in the hydrographic sections and has been noted before.

Line 406: Bit of a chicken and egg problem. The halocline is also deeper in the Labrador Sea because convection is deeper there. Would rephrase.

Line 416 / Fig 10. The depth is chosen such that it is always in the convective regime in the Labrador Sea, hence the nice steps. It is mostly too deep for this in the Irminger Sea, so a lot of the variability is caused by advection except in exceptionally deep convection years.

Line 430: There is a multitude of evidence that there was very deep convection in the Irminger Sea in the 1990s (but no Argo program). The LSW was advected to the Irminger Sea in the subsequent years and hence properties converged. Please rephrase.

Line 435: Bamber et al?

Please reference papers by Dukhovsky et al (2019) and Holliday et al (2019) who both describe the fresh anomaly.

Conclusions

Line 450 "in or near the Irminger Sea"

Line 473: was this only caused by advection of LSW or was the layer eroded by the 1600 m deep convection in 2016?

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