Two superimposed cold and fresh anomalies enhanced Irminger Sea deep convection in 2016 - 2018 by Patricia Zunino, Herlé Mercier, and Virginie Thierry

In this manuscript persistence of deep convection in the Irminger Sea is investigated. One winter of particularly severe atmospheric forcing and deep convection was followed by three winters of climatological strength which also had deep mixed layers. The authors quantified the buoyancy loss required for deep convection to commence each winter and concluded that the preconditioning arising from the previous winter's homogenization of the water column was a main reason for the persistence of deep convection.

I think this manuscript has the potential to be an important and valuable contribution to better understand deep water formation in the Irminger Sea /subpolar North Atlantic. However, as made clear also by the other reviewers, I have concerns about the determination of mixed-layer depths. As such, I recommend that the paper be revised before publication.

Thank you for your valuable comments; they help us improve our work. In the following we answer point by point to all of your comments and explain how we will modify the manuscript accordingly.

Major comments:

I am not convinced that automated routines, such as the threshold or split and merge methods, are particularly suitable for determining the vertical extent of the mixed layer. These routines generally perform well when applied to summer and fall profiles, when the upper ocean is stratified and there is a pronounced density difference between the mixed layer and the lower part of the profile. However, they are less accurate during periods of active convection when stratification is eroded. Furthermore, such routines cannot identify mixed layers that are isolated from the surface, either in the form of vertically stacked mixed layers or by early stages of surface restratification. Such isolated mixed layers are prevalent in the Labrador and Irminger Seas during winter (e.g. Pickart *et al.*, 2002). As pointed out by the other referees, if the density profile is considered in isolation, changes in temperature and salinity may be density-compensated such that the water column can appear to be homogenized while in reality it is not. Examples of that can be seen in Figure 2a-c (in particular 4901809 - 35). To avoid erroneous mixed-layer depths, I strongly recommend employing the semi-objective method developed by Pickart *et al.* (2002) instead of relying on automated routines.

In agreement with the three referees, we have revised our method for estimating MLD. Please see the first part of the response to de Jong (Referee 1) in order to see:

1. the specifications of our revised method for estimating MLD,
2. the comparison of MLD estimated with our revised method and estimated with other methods (de Jong et al., 2012; Pickart et al, 2002).
3. The region and profiles considered for the computation of the characteristics of the MLD (max MLD, Q3, density, temperature, salinity) formed Southeast of Cape Farewell.
4. The similarities and differences between our previous and new estimates.

Deep convection evidently took place in winter 2015 as documented by the many deep mixed layers shown in Figure 1. For winters 2016-2018, on the other hand, the vast majority of the Irminger Sea profiles do not have particularly deep mixed layers. If widespread deep convection occurred also during these winters, there should be many more profiles with deep mixed layers. Is it possible that the mixed-layer depths determined by the automated routines are remnants of deep convection from a previous winter or from the Labrador Sea where mixed layers are generally deeper?

The percentage of profiles with deep MLD depends on the period during when we compute the statistics. Our previous method was misleading because we considered the *entire* winter for computing the statistics and not only the convection period (see also answer on this point to referee 2). We now identify the period during which deep MLDs > 700 m were observed for each winter in the Southeast Cape Farewell (SECF) region (pink box in Fig. R4 in referee 1 answer) (see answer to reviewer 2 for more details). Then, we quantified the percentage of floats that measured deep MLD in the region and during the period of deep convection. The results are shown in table R2. The lower % is found for winter 2017, but it is still substantial and reflects the fact by the fact that the floats showing deep MLD were found southwest of the SECF box suggesting that convection did not occur over the full box. The results of this sensitive study will be added to the section 4.1 of the revised manuscript.

Table R2. Sensitivity study about the Argo float coverage in the SECF region (pink box in Figure R4 in the answer to referee 1). Period is the period during which floats with deep mixed layers were observed. We indicate the total number of floats found in the SECF region during the indicated period, and the number of floats showing deep convection. Finally, the percentage of floats showing deep convection is indicated.

	Deep convection period	n floats in the region	n floats in the region with deep convection	% of floats in the region with deep convection
W2015	15/01/2015 to 21/04/2015	11	8	73%
W2016	22/02/2016 to 21/03/2016	4	2	50%
W2017	16/03/2017 to 04/04/2017	9	3	33%
W2018	24/02/2018 to 26/03/2018	4	2	50%

We do not think that the observed MLD are remnants of deep convection from a previous winter or from the Labrador Sea because the new estimates of MLD are from profiles homogenous in terms of density, temperature and salinity. Most importantly, the fact that the 1D-buoyancy budget is nearly closed (section 4.3) is also an indication that deep convection occurred locally in the SECF box during winters 2016, 2017 and 2018.

To get a more robust estimate of convection in the subpolar North Atlantic these winters, I suggest dispensing with the 700 m “deep convection” criterion and showing if not every mixed layer at least the 50-80% deepest mixed layers encountered by each float every winter. That would remove shallow mixed layers arising from early phases of the seasonal evolution of the mixed layer and profiles obtained within stratified eddies, while the remaining mixed layers would allow for more robust quantification of the general depth of convection.

OK, this seems to be a nice idea, but it would bias low the estimate of convection depth if the statistics of MLD were made using the profiles for the entire winter. The criteria should be applied to the convection period that we select here by considering profiles deeper than 700 m because it is the minimum depth that should be reached for LSW renewal. If apply to those profiles your criteria would not be much different from our Q3. Note that our estimate of convection depth based on the statistical criteria Q3 is equivalent to the aggregate maximal convection depth used by Yashayaev and Loder (2017) and allows direct comparison with this author's results.

Profiles that do not extend beneath the base of the mixed layer (there may be some examples in Figure 2d-f) would result in a shallow bias of the mixed-layer depth estimate and should be excluded from the analysis.
We agree. These profiles located between 48°W and 45°W are not consider in our new results.

Specific comments:
Line 95:
It should be: "...Argo and mooring data..." Corrected

Lines 106 and 361:
Mixed layers exceeding 1400 m depth were determined also from shipboard measurements in the Irminger Sea in April 2015 (Fröb *et al.*, 2016). We add this reference to the revised manuscript.

Line 122:
If the TEOS-10 convention is used, conservative temperature and absolute salinity should be used instead of potential temperature and salinity.
TEOS-10 allows the computation of theta and practical salinity.

Line 123:
Please explain why a salinity of 35 was chosen as a reference value.
This sentence is deleted in the manuscript because we do not use FW in the paper. Sorry for the confusion it may have caused.

Line 124:
Please provide more information about the gridded products. Are different time periods and resolutions the only difference between the products? What are the errors, in particular for the EN4 product which extends back to 1900 and covers some very data-sparse periods?
ISAS and EN4 are optimal interpolation of in situ data, but the optimal interpolation method is not exactly the same in both products due to different choices for the spatial and temporal correlation functions used for the optimal interpolation. Details about both databases are described in the references given in the manuscript (Gaillard et al., 2016; Kolodziejczyk et al., 2017; Good et al. 2013). Note that we used EN4 data from 1993 afterwards and that the monthly temperature and salinity fields at a given time only depends on the data found in a short time window around the date of the analysis. The data sparse-period at the beginning of the 1900 did not influence our results.

Line 130:
Does the net air-sea heat flux include radiative fluxes or only turbulent fluxes?

It includes both radiative and turbulent fluxes. We indicate it in the revised manuscript.

Line 149:

I do not think that 48◦W is commonly used as a border between the Labrador and the Irminger Seas. Many of the deep mixed layers were recorded directly south of Greenland, in a region that is not really part of either the Labrador or the Irminger Seas.

Ok, the limit at 48°W was used just to include in the analysis of the MLD properties the profiles found between 48°W and 45°W in 2018. In the revised computation we used only profiles inside the pink box which limit is at 45°W and we now refer to the pink box as Southeast Cape Farewell (SECF) instead of Irminger Sea. Note that the northern limit of the box is changed from 61°N to 59.3°N. We calculated the atmospheric forcing and the preconditioning considering this new box limit and it does not change the main results and conclusions of our work.

Line 156 and elsewhere:

Please insure that all papers cited in the text are included in the References section. For example is Gill (1982) missing. Ok, thank you for noting it.

Line 174:

How was the depth of the Ekman layer estimated?

We used the Ekman transport and we considered that the SST is representative of the temperature in the Ekman layer. We will clarify this point in the revision.

Line 179:

For consistency, it might be better to use SST also from the EN4 product.

Ok, we have estimated the Ekman Buoyancy Flux (BFek) using EN4 SST.

The horizontal Ekman Buoyancy flux in the SECF region (pink box in Fig. R4 in response to referee 1), accumulated from 1 September to 31 August the year after was estimated with: i) with EN4 SST and EN4 SSS and ii) with ERA SST and EN4 SSS; they are represented in Figure R5. Both time series show the same behavior but the results obtained with EN4 SSS and EN4 SST are smoother than the results obtained with ERA SST and EN4 SSS. Thank you for your comment, we switched to EN4 SST.

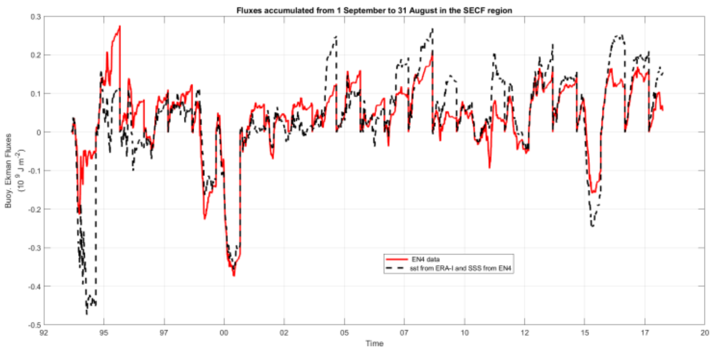

Figure R5. Horizontal Ekman Buoyancy flux in the SECF region (56.5° - 59.3°N, 45°W – 38°W), accumulated from 1 September to 31 August the year after estimated: i) with EN4 SSS and EN4SST and ii) with ERA SST and EN4 SSS.

Line 185:

It should be: "...most of the Argo profiles..." Corrected.

Line 197:
It should be: "...to be removed (B(zi)) from the late summer density profile..." Corrected.

Line 234:
Salted, in this context, is not appropriate. "Became saltier" would be a better expression. Corrected.

Line 284:
If B remained nearly constant, does that imply that restratification and advection are unimportant?

It means that the homogeneous layer (600 – 1400 m) formed at the end of winter was not destroyed by the advection by eddies and large scale circulation during the following spring and summer.

Line 297:
Units (m) are missing after 800-1000. Corrected.

Line 321:
What was the basis for choosing the point 59◦N, 40◦W?
Our objective here was to see the evolution of the anomalies in depth and in time. Therefore we choose a point, 59°N, 40°W, in the middle of the box. The result is not sensitive to the location of the point inside the pink box. This information is added to the revised manuscript. In the revised manuscript we present the same figure at 58°N, 40°W, which is centered in the new pink box, instead that at 59°N, 40°W.

Line 377:
If convection exceeded 1400 m in winter 2014-15 (e.g. Fröb *et al.*, 2016), why is it unlikely that this layer was locally formed?
Right, we cannot exclude that the convection of winter 2014-15 cause salinity decrease in the water column. We slightly modified this paragraph in the revised manuscript.

Line 382: The papers by Lavender *et al.* (2000) and Straneo *et al.* (2003) could also be cited here. Ok, we add them to the revised paper.

Line 383: Corroborated is misspelled. Right.

Line 388:
If deep convection occurs every year, perhaps the definition of deep convection should be revised.
This sentence is confusing. In the revised manuscript, this sentence is written as:
"We now compare the atmospheric forcing and the preconditioning of the water column in the SECF region with those of the nearby Labrador Sea where deep convection happens almost every year."

Line 403:
It should be: “...the deep halocline was successively deepening...” Right, thank you.

Line 410:
I am sceptical of the claim that the deepest convection-depth ever observed in the Labrador Sea occurred in winters 2016-2018. Very likely convection in the successive high-NAO winters of the early l990s substantially exceeded convection in winters 2016-2018. At that time mixed-layer depths were at least 2300 m (e.g. Avsic *et al.*, 2006).
Yes, you are right. So, we add to the sentence “since the beginning of the Argo period”.

Lines 419 and 421:
Density units are not capitalized consistently. Ok.

Line 425:
It should be: “...observed in both basins...” Right, thank you.

Line 430:
There were no wintertime measurements in the Irminger Sea in the early 1990s, but there is strong indirect evidence that deep convection occurred in the Irminger Sea at that time (see for example publications from the group of R. Pickart).
Right, there are evidences that deep convection occurred in the Irminger Sea in early 1990s (Pickart et al., 2003).
In any case, the three referees find something wrong in this paragraph and Figure 10. Because this paragraph and figure is not important for the conclusions of our paper we decide to remove them in the revised version of the paper.

Line 481:
Acknowledgement is misspelled. Right, thank you.

Lines 519 and 522:
The name de Jong is inconsistently capitalized. Right, corrected.

Figure 5:
Please indicate, for example using tick marks along the top axis, when Argo float profiles were available in the Irminger Sea.
Ok, we add the tick marks in plot 5b.