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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Received and published: 21 June 2019

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 (_0.25_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.

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.

Thank you very much for your constructive comments. In the following we answer point by point to your comments and indicate how the manuscript is going to be revised.

Following your suggestion, we revised the manuscript to define the MLDs based on density, temperature and salinity criteria (and not density criteria only). We adapted our method to include temperature and salinity criteria in addition to density criteria and we compared our results to two alternative methods of determination of the MLD previously used by de Jong et al. (2012) and Pickart et al. (2002). In our revised method, we determined the MLD as the shallowest of the three MLD estimates obtained separately from temperature, salinity and density profiles using the threshold method (de Boyer Montégut et al., 2004). The threshold criteria were the differences in property between the surface (30 m) and the MLD set to 0.01 kg m^{-3} in density (Piron et al. 2017), 0.1°C in temperature and 0.012 in salinity. The temperature threshold of 0.1°C and the salinity threshold of 0.012 were selected because they correspond to a threshold of 0.01 kg m^{-3} in density that was previously shown to perform well in the subpolar gyre (Piron et al., 2016). Indeed, MLD based on this density threshold favorably compared to those estimated by the method of Thomson and Fine (2003) as demonstrated in Piron et al. (2016; 2017) and visual inspection.
We used de Jong's methodology as follows. First we interpolated the Argo data into 10 m depth steps. Then, we estimated the standard deviations of density, temperature and salinity from the surface to each depth level. Following de Jong et al. method's, three MLD were defined as the depths were the standard deviations were smaller than 0.05 kg m^{-3}, 0.05°C and 0.005 for density, temperature and salinity, respectively. The final MLD was the shallowest of the three estimates.
The Pickart's methodology was applied as follows. We used the estimates of our threshold method as a first guess for the MLD. Then, the mean and standard deviation of the density, temperature and salinity were estimated from the surface to the initially defined MLD. Finally, we plotted the two–standard deviation envelope overlaid on the original profile. The mixed layer depth was determined as the location where the profile permanently crossed outside of the two–standard deviation envelope.
The MLDs resulting from our method are shallower than the MLD resulting from the method of de Jong et al. (see examples in figures R1 – R3). Moreover, sometimes, the MLD defined by de Jong's method in terms of temperature or salinity is not placed at the base of the mixed layer (as visually defined), e.g. profiles 6900446 – 213 (Fig. R1) or 5904772 – 33 (Fig. R3). Otherwise, the MLDs

estimated by our method are coherent with the MLDs resulting from the method of Pickart et al. (2002): see the envelopes (discontinuous vertical lines in figures R1 – R3) of mean ± two - times the standard deviation of density, salinity and potential temperature, from the surface to the MLD estimated with our method. Finally, we also compared our results with the MLDs determined using Holte & Talley (2009)'s method and available in the web. However, MLDs were not available for all our floats, e.g. float 6900446, or the method provides too shallow MLD, e.g. profile 6901171 – 101 (89 m, see Fig. R2).

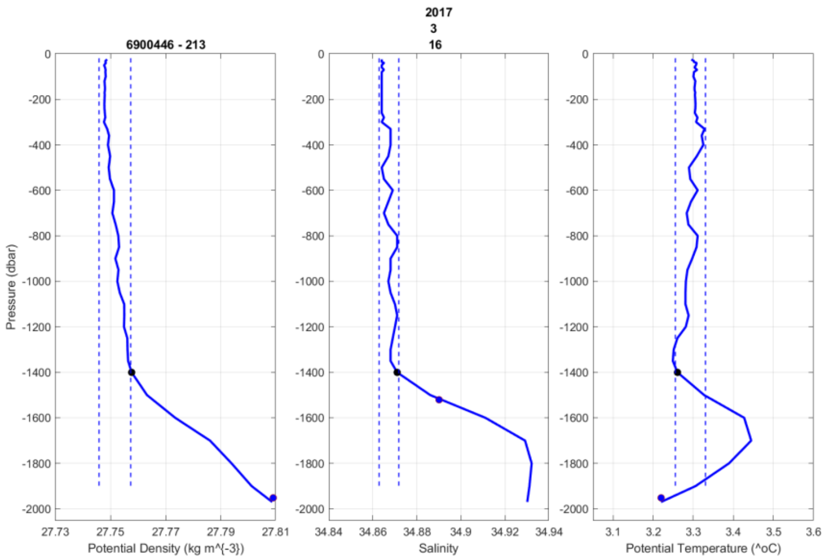

Figure R1. Vertical profiles of potential density, salinity and potential temperature of profile 6900446 - 213. The black points are the MLD estimated by our threshold method. The blue points indicate the MLDs resulting from the method of de Jong et al. (2012): in the density plot the MLD derived from density profile, in the salinity plot the MLD derived from salinity profile and in the temperature plot the MLD derived from temperature profile; the final MLD is the shallowest of the three defined MLDs. Following Pickart et al. (2002), the envelopes of mean ± two - times the standard deviation of the density, salinity and potential temperature from the surface to the MLD estimated using as a first guess for the MLD our threshold method were estimated and represented as discontinuous vertical lines.

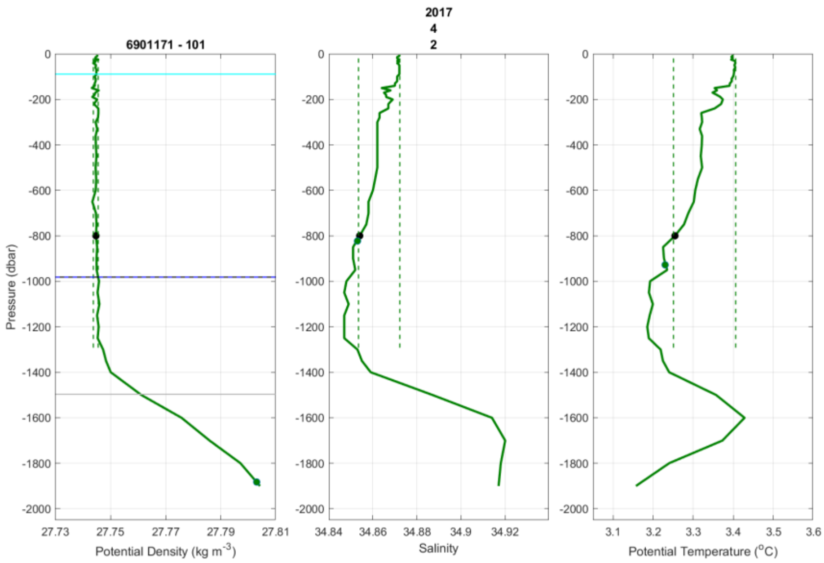

Figure R2. Same than Fig. R2 but for profile 6901171 – 101. Additionally, the horizontal lines on the left side plot represent the MLDs estimated by the Holte and Talley's method: in gray the MLD defined by the density threshold, in black the MLD defined by the density algorithm, in blue the MLD defined by the temperature threshold and in cyan the MLD defined by the temperature algorithm; note that black and blue lines are overlapping.

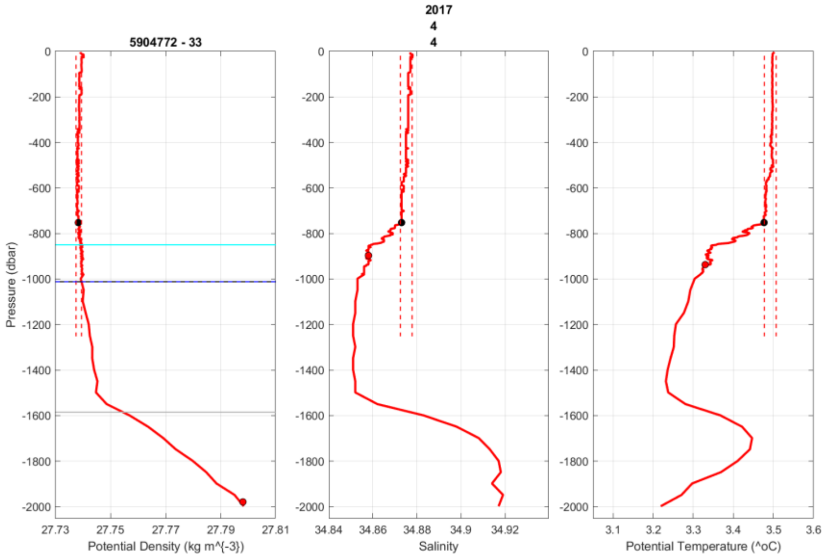

Figure R3. Same than Fig. R2 but for profile 5904772 - 33.

Because the comment of the referee focused on profiles for winter 2017, we expose in more details here the differences between the previous and the new MLD estimates for this winter 2017. First, the profile 4901809 – 35 has been eliminated because the stratification of the upper 250 m corresponds to the seasonal stratification (this profile was measured on 29th April 2017). In any case, applying the criterion of temperature threshold of 0.1°C, the MLD would be 337 m, shallower than 700 m. Second, the MLD of the profile 6901171 – 101 changes from 1250 m (the previous estimate) to 801 m (the new estimate). The MLDs estimated for profiles 6900446 – 213 and 5904772 – 33 do not change.

The MLDs of all the profiles measured Southeast Cape Farewell (SECP) during winters 2015 – 2018 were recalculated with our revised method. The positions and MLDs of the profiles showing MLDs deeper than 700 m are represented in Fig. R4. Comparing these new results with the previous results, we find that the number of profiles showing MLD deeper than 700 m decreased: 31 profiles (new) in place of 36 (previous) for winter 2015, 3 profiles (new) in place of 7 profiles (previous) for winter 2016, 3 profiles (new) in place of 4 profiles (previous) for winter 2017 and 9 profiles (new) in place of 10 profiles (previous) for winter 2018.

We have also recalculated all the properties showed in the table 1 of the previous version of the paper. Note that these properties are now estimated considering only the profiles inside the SECF box (pink box in Fig. R4.) The new results (table R1 in this document) are in line with the results of the submitted paper.

Table R1. Properties of the deep convection in the SECF (56.5°N-59.3°N, 45°W – 38°W) in winters 2015 – 2018. We show: the maximal MLD observed, the aggregate maximum depth of convection Q3, the σ_0, θ and S of the winter mixed layer formed during the convection event and n, which is the number of Argo profiles indicating deep convection. The uncertainties given with σ_0, θ and S are the standard deviation of the n values considered to estimate the mean values.

	Maximal MLD (m)	Q3 (m)	σ_0 (Kg m^{-3})	θ (°C)	S	N
W2015	1710	1205	27.733 ± 0.007	3.478 ± 0.130	34.866± 0.013	29
W2016	1575	1471	27.746± 0.002	3.388 ± 0.032	34.871± 0.003	3
W2017	1400	1251	27.745± 0.007	34.868± 0.007	3.364± 0.109	3
*W2018	*1300	*1250	*27.752± 0.004	*34.857± 0.003	*3.204± 0.069	*4
W2018	1300	1300	27.748± 0.001	34.859± 0.003	3.263± 0.031	2

*W2018 line corresponds to the properties of the mixed layer in W2018 in SEFC when the data of Float 5903102 were considered in the analysis. Finally, following the suggestion of referee 3, we decide to exclude the data of float 5903102 of our analysis because their MLDs matched with the maximal depth dived by the float.

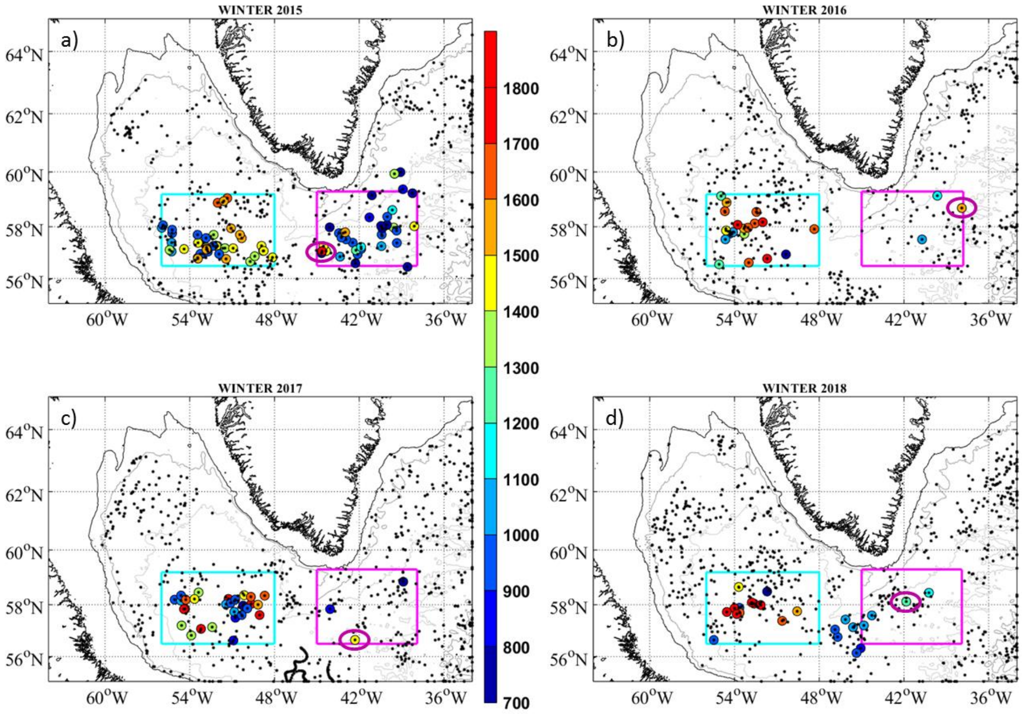

Figure R4. Positions of all Argo float north of 55°N in the Atlantic between 1 January and 30 April a) 2015, b) 2016, c) 2017 and d) 2018 (black and colored points). The colored points and color bar indicate the mixed layer depth (MLD) when MLD was deeper than 700 m. The pink circles indicate the position of the maximal MLD observed SECF each winter. The pink and cyan boxes delimit the regions used for estimating the time series of atmospheric forcing and the vertical profiles of buoyancy to be removed in the SECF region and the Labrador Sea, respectively (SECF: 56.5°N – 59.3°N and 45.0°W – 38.0°W, Labrador Sea: 56.5°N – 59.2°N and 56°W – 48°W).

We want also to clarify that in the previous version of the paper and in the new results, the deepest MLD observed in the SECF in winter 2017 was recorded by profile 6900446 – 213 and not by profile 4901809 – 35 (bright blue profile in the Fig. 2 of the previous version of the paper) as indicated by the referee. Note that for 6900446 – 213, the new MLD is the same than in the previous version of the manuscript.

Concluding, when recalculating the MLDs as suggested by the referees, the maximal MLD observed in the SECF was deeper than 1300 m in winters 2016, 2017 and 2018 (see fig. R4 and table R1). It indicates that deep convection occurred during the studied winters. This is the first important result of our paper, which does not change when recalculating the MLD.

Concerning the title, in order to avoid preconceived ideas to the reader, in the revised manuscript we change it to:
"Why did convection persist over 4 consecutive winters (2015-2018) South East of Cape Farewell?"

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".

We agree. Following Piron et al. (2015), we focus on convection deeper than 700 m, which is the minimum MLD for LSW renewal. We clarified the sentence that now reads :

"In the Irminger Sea, Argo and mooring data showed that ~~deep~~ convection **deeper than 700 m** happened ~~in the Irminger Sea~~ during winters 2008, 2009, 2012, 2015 and 2016 (…)."

Data

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

TEOS-10 allows the computation of theta and practical salinity.

Please explain briefly why 35 is chosen as a reference.

This sentence is going to be deleted because we do not use FW in the paper. Sorry for the confusion it may have caused.

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

It could be interesting to check the results obtained using the new ERA5 dataset. However, the first author of this paper, who processed the data, is now working in a private company and she has not the time of redoing calculations with this new database.

Method

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

See above our answer to the major comment.

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.
We agree that 48° W is not the limit between Labrador and Irminger Sea. When splitting the region in three areas as in Piron et al. (2017) we did not observe deep convection in the northernmost Irminger Sea (note that with the previous method of MLD computation we had a few deep MLD in the northernmost Irminger Sea in winter 2016 (those MLD corresponding to profiles not homogenous in temperature and salinity were not diagnosed with the new MLD method). In the new version of the paper we define a new pink box that we refer to as Southeast Cape Farewell (SECF) region (see Figure R4). The only change in the pink box is its northern limit: 61°N/59.3°N in the previous/revised version of the manuscript. The new box encloses all the profiles showing deep MLD during winter 2016, 2017 and 2018 Southeast of Cape Farewell. Note that the pink box is also used to estimate the atmospheric forcing and the preconditioning of the region. We recalculated it: the new results are very similar to the results shown in the previous version of the paper and do not change the conclusions of the paper.

Equation 1 and others. There are periods (.) instead of multiplication symbols. Thank you for noting it. We change all of them.

Results
What is Q3? "Q_3 is the MLD value that is exceeded by 25% of the profiles showing MLD deeper than 700 m and is equivalent to the aggregate maximum depth of convection defined by Yashayaev and Loder (2016).", as it was indicated in lines 152 – 153 of the submitted manuscript.

Part of the results paragraph will have to be rewritten when MLD are rederived.
Right, we are going to rewrite this section with the new results.

Line 268: Mean over which period?
1993 – 2016, as indicated in the figure caption of Figure 4. We add 1993 – 2016 in the text.

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.
We are describing the buoyancy profiles from the mean (2008 – 2014) and we see that the thermal component of the buoyancy dominates the total buoyancy. We agree that if a large amount of freshwater is added to the upper ocean, we would find an important contribution of the haline component of the buoyancy, but it is not what we see in the mean (2008 – 2014) buoyancy profiles. We added Fig. 6 at the end of this sentence to make clear that we are describing the results of this figure and that the statement is not a general statement.

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.

This comment has also motivated us to reduce the SECF or pink box. The new estimates of atmospheric forcing correspond to a reduced region closer to the position where deep convection took place.

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.
Your comment makes sense, but note that the new estimates of MLD continue matching adequately with the predicted MLD. In the new version of the manuscript we will mention that the differences between the predicted and observed convection depth could be due to errors in the atmospheric forcing (Josey et al., 2018), lateral advection and/or spatial variation in the convection intensity within the box that was not captured by the Argo sampling.

Discussion
Line 366: This was seen throughout the 1990s and is not quite as surprising as the authors state.
We deleted "surprisingly".

Line 397: The Labrador Sea is always more favorably preconditioned, it is quite visible in the hydrographic sections and has been noted before.
The Labrador Sea is *usually* more favorably preconditioned than the Irminger Sea. However, we see that the water column from the surface to 1,300 m in winter 2017 is more favorably preconditioned in the SECF than in the Labrador Sea (see Fig. 7 in the previous version). For example, in order to homogenize the water column down to 1,300 m, 1.80 x 10^9 J m^{-2} is required in the SECF whereas 2.13 x 10^9 J m^{-2} is needed in the Labrador Sea.

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.
Not really a chicken and egg problem, if you are thinking in terms of preconditioning. To clarify our point we modified the sentence as : "The deep halocline acts as a physical barrier for deep convection in both the Irminger Sea and the Labrador Sea, but because the deep halocline is deeper in the Labrador Sea than in the Irminger Sea, the preconditioning is more favorable to deeper convection in the Labrador Sea than in the Irminger Sea."

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.
You are right and the figure is confusing even when the discussion is limited to deep convection events in the SECF region. Because of your comment and the comments of reviewer 2 we decide to delete Figure 10 and paragraph 415 - 433 in the revised manuscript.

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.

We decide to remove Figure 10 and paragraph 415 -433 in the revised manuscript. It does not change the conclusions of the paper.

Line 435: Bamber et al? Yes, Bamber et al. Thank you for noticing it.

Please reference papers by Dukhovsky et al (2019) and Holliday et al (2019) who both describe the fresh anomaly.
Dukhovsly et al. (2019) describe the freshwater anomaly of the 2010s, so, it does not concern the period we study in our paper.
Otherwise, we think that Holliday et al (2019) has not been published yet (V. Thierry is co-author of the paper).

Conclusions
Line 450 “in or near the Irminger Sea”
In the revised manuscript this sentence is changed to:
“During 2015 – 208 winter deep convection happened in SECF reaching deeper than 1,300 m”.

Line 473: was this only caused by advection of LSW or was the layer eroded by the 1600 m deep convection in 2016?
Our sentence was confusing. We will mention that deep convection of W2016 also favored the preconditioning for winter 2017 – 2018.