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

Anonymous Referee #2

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This paper reports on a very interesting analysis of recent Argo data in the subpolar North Atlantic. They claim that deep convection in the Irminger Sea, which began in 2015, persisted through 2018 because of favorable preconditioning. They perform some novel analyses and the findings will be of great interest to the community. However, I agree with the review posted by Femke de Jong, which raises issues with the way that mixed layers are defined in the study. This is central to the interpretation and conclusions of the study, and I think that at the very least some major re-framing of the work is necessary. I recommend this work for publication after major revisions.
Thank you very much for your constructive review. In the following we answer to each of your comments and describe how we are going to take into account your suggestions in the revised manuscript.

Major comments
I would like to echo de Jong's comments regarding the mixed layer depth derivation. In order to show that deep convection occurred in 2016-2018, they should show that all properties (including temperature and salinity) were homogeneous throughout, not just that a density threshold was exceeded at a very deep depth.
The three referees agreed on this point. Consequently, we have adapted our methodology to estimate the MLD considering density, temperature and salinity profiles. Please, refer to the beginning of the answer to de Jong (referee 1) in order to see how we have modified our methodology to estimate MLD. The new results (MLD and properties) do not change the main conclusions of our paper.

Regardless of the method selected by the authors in their revision they should include much more detail on it in the text as it is a central calculation. They should also be clear about how sensitive their results are to the method used and to the thresholds that are selected. They should also detail how their methods relate to the methods used by previous studies in the region.
Right, we will explain our revised methodology to estimate the MLD indicating the threshold of density, temperature and salinity used. Moreover, we will add a figure in supplementary material showing that our estimates of MLD favorable compare with the estimates resulted when using the methods of Pickart et al. (2002) or de Jong et al. (2012) as discussed at the beginning of response to referee 1.

The authors should address how sensitive their results are to the Argo float coverage, and what portion of the Argo floats present have deep mixed layers. They report how many floats have mixed layers deeper than 700m, but not how many were present. Does the percentage of floats with deep mixed layers decrease over time? The authors should comment on why they think so few Argo floats have deep mixed layers. Is it consistent with their buoyancy forcing analysis? Does the sampling in time account for some of this: i.e. Are deep mixed layers seen more commonly late in winter?
Thanks to your comment we realized that our discussion was misleading because it was based on the percentage of profiles showing deep convection during the *entire* winter, which is small by construction because only profiles at the end of winter show deep convection. We rather should have count the number of floats showing deep convection during a given year. Accordingly, we now identify the period when deep convection occurs as the period when at least one profile shows MLD > 700 m (the period begins when a profile with MLD > 700 m is detected for the first time for the given winter and it ends when there is no more profiles with MLD > 700 m). Then, we quantified the

percentage of floats with deep MLD present in that period and region (pink box in figure R4 of answer to referee 1). This information is summarized in table R2 of this document and will be included in section 4.1 of the revised manuscript. The percentage varies between 33% and 73%. In 2017, the three profiles with deep mixed layer were recorded by three different floats, all located in the southwest corner of our region. This shows that the convection area was confined to a small area of the SECF region and explains that the lowest percentage is observed in 2017.

Table R2. Sensitivity study about the Argo float coverage in the SECF region (pink box in Figure R4 of 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.

	Period	n floats in the region	n floats in the region with deep convection	percentage 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%

The mixed layers reported in winter 2018 are almost all to the south of Cape Farewell, and not in the Irminger Sea. Further, the TS properties in 2018 are much more similar to Labrador Sea properties than Irminger Sea properties (Figure 3). This is consistent with the SCF box properties reported in Piron et al. 2017. Some of the properties in 2016 and 2017 may also fall in that category, I don't think the author's should be calling this "Irminger Sea convection".
Right. In the revised manuscript we changed the northern limit of the pink box to 59.3°N instead of 61°N previously and refer to the pink box as Southeast Cape Farewell (SECF).

The author's show a very interesting analysis of Labrador Sea properties which are advected into Irminger Sea and contribute to the deepening of the Irminger Sea halocline (Figures 7 and 9). Is this advection limited to the 1200-1400 range they consider? How does advection from the Labrador Sea in other depth ranges fit in?
Advection from the Labrador Sea certainly contributed to vary the properties from the surface to 1000 m. However, the buoyancy budget showed that this is minor contribution compared to the buoyancy loss due to the local air-sea flux. We add a comment about it in the revised manuscript.

The title is confusing, and I wonder if, in general, the author's should shift their focus from the Irminger Sea in particular and instead focus on the important connections between the intermediate waters in the subpolar North Atlantic. I think the authors have an opportunity here to clarify that intermediate waters are formed in many places and how the connections between these basins

affect intermediate water mass properties. Their focus on salinity in addition to temperature would make this angle particularly interesting.
In the revised manuscript we change the title to: “Why did convection persist over 4 consecutive winters (2015-2018) South East of Cape Farewell?”
Moreover, we now mention several times the role of advection from the Labrador Sea. We also added to the discussion the following paragraph:
The Labrador Sea, SECF region and Irminger Sea are three distinct deep convection sites (e.g. Yashayaev et al., 2007; Bacon et al., 2003; Pickart et al., 2003; Piron et al., 2017). In this work, we give new insights on the connections between the different sites, showing how lateral advection of fresh LSW formed in the Labrador Sea favored the preconditioning in the SECF region fostering deeper convection.”

Minor comments
The link between anthropogenic forcing and the recent convection that is drawn in the first few sentences in the abstract and throughout the introduction is a bit of a stretch. The motivation could be made more direct and convincing, and this type of speculation could remain in the discussion where it is more relevant.
Ok, in the revised manuscript we exclude the references about the anthropogenic forcing by deleting the first sentence in the Abstract and the first paragraph in the Introduction.

L109: “for the first time to our knowledge in this region” - this is a broad claim and not necessary. Ok. Deleted.

Section 3.1: Please add significant detail on the mixed layer estimation method.
Right, it has been added in the revised manuscript as indicated at the beginning of this document.

L222: The 2018 profiles with deep mixed layers are not in the Irminger Sea.
Right, as explained above, we changed the limit of the pink box and refer to our pink box as SECF.

L237: “Water masses formed are very similar” It should at least be acknowledged that they are formed much closer to the Labrador Sea than in previous years.

Right, when excluding the floats south of Cape Farewell as requested by referee 3, the properties of the water mass formed in the SECF region in W2018 is not similar to the formed in the Labrador Sea in W2018. The sentence “Water masses formed are very similar” is excluded in the new version of the manuscript.

L247: maybe instead: “heat alone” at the end of this sentence. Ok

L248: This paragraph is confusing. Perhaps referring to Figure 4 earlier on would help?
Ok, thank you for noting it. The objective of the paragraph was to show that SFek cannot be neglected in BFek. We present this point more clearly in the revision. Morevoer, in this section, we add a paragraph describing Figure 4.

L331: “despite they were also fresher” ! “despite the fact that they were also fresher”
Ok, we will change it.

L340: Refer to figure 6. Ok, we will write, “The predicted convection depths are determined as the depth at which B(zi) (Fig. 6a), equals the atmospheric forcing.”

L348: Clarify what happened here. These floats only profiled down to 1,100m?

Exactly. We would rewrite the sentence as: "This result is in line with the fact that among the 10 profiles that we used to compute Q3 in W2018, 6 showed deep convection down to 1,100 m and were recorded by floats **with a maximum profiling depth** of 1,100 m, most likely leading to an underestimation of the MLD." However, this sentence is going to be deleted in the revised manuscript. In the revised paper, and following the suggestion of referee 3, we exclude from the analysis the profiles that do not extend beneath the base of the mixed layer, because it results in bias in the properties related to the mixed layer.

L351: I was also confused by the fact that the author's claim to neglect advection, but cite advection of properties from the Labrador Sea as a reason for favorable preconditioning. Perhaps remove that claim. Additionally, the fact that the T and S properties are not homogeneous goes against the idea that deep convection is occurring locally.
We agree that this paragraph was confusing. We now identify lateral advection as a possible cause for the buoyancy budget residuals. The profiles with non-homogenous TS in the mixed layers are now excluded from the analysis.

L370: hydrological ! hydrographic. Yes, hydrographic, we will change it.

L370: anomalies relative to what? Related to the mean 2002 – 2016, we added it in the text.

L383: Why would only the properties in the 1200-1400 depth range be advected? Or are they the only ones that have a profound effect? See above. Please clarify.
See answer in your comment above.

L415/Figure 10: Not sure how this figure and paragraph are linked to the rest of the study.
This figure and paragraph were not essential for the conclusions of the paper. We decide to remove them in the revised manuscript.

L470: hydrological ! hydrographic Yes, hydrographic, we will change it.

Figure 4: Note the differences between the axis ranges in the caption. This figure could be featured earlier as it provides important context.
Ok, we will write in the figure caption: "Note the differences between the axis ranges". We refer to this figure earlier in the section 4.2 of the revised manuscript.

Figure 5: From Figure 5d, it appears that the thick density layers are actually below the densities that are being ventilated in the Irminger Sea (white areas). This supports the idea that they are being advected from the Labrador Sea.
In winter 2015 and 2016 the thick density layers have a density of 32.37 Kg m^3, that corresponds to sig0 equals to 27.746 Kg m^3 which is the density of the mixed layer in the SECF (Fig. 3 in the manuscript). In winter 2017 and 2018 the thick density layers are found at denser density (32.38 Kg m^3), that corresponds to sig0 equals to 27.754 Kg m^3 which is the density of the mixed layer in the SECF for these winters (Fig. 3). These results support local formation.
Accordingly, we add at the end of the first paragraph of section 4.3: "The denser density of the core of the thick layers in 2017 -2018 compared with 2015 - 2016 agrees with the densification of the mixed layer SECF shown in Table 1 and Fig. 3."

Figure 6: Please clarify: are you using all Argo data within the box, or only the ones with deep mixed layers?
All data. To clarify, the Figure caption will be modified as, "they were calculated from **all** Argo data measured in the Irminger box (see Fig. 1) in September before the winter indicated in the legend."

Figure 7: This is a very interesting figure! Could feature more prominently and be usedto describe some key differences between the Labrador and Irminger Seas.

Right, we used this figure in the discussion (lines 394 -414) when comparing the preconditioning in the Labrador Sea and in SECF.

Reddish! red. Bluish ! blue.Ok, in the revised manuscript we change the figure caption of this figure.

Figure 8: missing a) b) c) labels on the figure. Ok, we add them.