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Reply on RC4

Jannes Koelling et al.

Author comment on "Oxygen export to the deep ocean following Labrador Sea Water formation" by Jannes Koelling et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-185-AC4, 2021

Dear Reviewers,

Thank you all for taking the time to read our manuscript and provide constructive and helpful feedback that we believe will improve the final version of the paper. We hope that despite the unusually high number of reviewers we were able to sufficiently answer all of your comments. There were two main suggestions for more significant changes that were each picked up by several reviewers, with some overlap in the reviewer's comments: The estimates of oxygen export from the Labrador Sea and oxygen demand in the Atlantic Ocean in section 4.2, and the definition of LSW "export" used for Figures 7 and 9b. We found it appropriate to address all these comments together in a comprehensive manner, rather than responding to each reviewer separately. The answers to the reviewer's comments on these topics and proposed changes for the revised manuscript are summarized in a supplement file which we uploaded along with each author comment, and the individual response to each reviewer's more specific comments is found below.

Kind regards,

Jannes Koelling

Reviewer 4

General comments:

- *Convection in the Labrador Sea exhibits pronounced interannual and decadal variability (e.g. Fisher et al. 2010, GRL). For example, convection was particularly intense between 1987-1994 (Marshall et al. 2001, J. Clim.). All of the results in this paper are derived from a single year of measurements, but no discussion is given to how representative these measurements and findings are of other years, and of the long-term mean behavior of the Labrador Sea. The authors should discuss these issues clearly throughout the abstract and manuscript. It seems to me that the results might differ substantially if the measurements were made in a year of particularly intense convection in the LS; if this is the case, the authors should clarify throughout the* *manuscript that their results apply to the particular year of measurements, and that its generalizability to other years remains unclear.*

The effect of interannual variability in Labrador Sea Water formation is an important question that we unfortunately cannot adequately answer with the current data set, but we hope to address it in a future study using a longer record.

The overall outflow in the Labrador Sea Water density layer has been shown in previous studies to not have a clear connection to the strength of convection, as shown in the Fischer et al paper you mentioned: "[…] the annual mean flow varies at all depth levels by the order of 10% relative to the 'decadal' mean, and again there are no detectable systematic trends in the boundary current intensity.", as well as other studies using 53N data (Dengler et al., 2006, GRL).

Nonetheless, the amount of LSW entering the boundary current from the interior, and/or the strength of convection in the boundary current itself, will likely be different between period like the 1987-1994 convection regime compared to periods like 2005-2008 when convection was shallower. Accordingly, the properties of the outflow may be closer to the IW endmember as used in our study during a weak convection period, and closer to "pure" LSW in a strong convection period, which would also affect the oxygen content of the outflow. We will add a paragraph to the discussion in section 4.2 to address the subject of interannual variability, and discuss how it might change the results.

- *The authors measurements exhibit two key features that mark the arrival of LSW at the boundary current array: (i) a rapid shift of the water mass properties toward lower spice and higher O2, and (ii) increased temporal variability in O2 (and presumably spice) about the monthly mean. While the authors discuss the drivers of feature (i) extensively and conclusively, I could not find any explanation for feature (ii). Arguably feature (i) is more directly relevant to the rates of LSW and O2 export via the boundary current, but the processes underlying (ii) should at least be discussed, even if only to offer some speculation as to its origins based on previous studies.*

We do mention a possible explanation for the high temporal variability in section 3.1, lines 164-166: "This stark heterogeneity of properties may suggest that newly formed LSW is rapidly exported out of the Labrador Sea in February, and some of it is transported with the boundary current without much mixing with the surrounding water." and in the conclusions, lines 423-425: "LSW first arrives at the offshore moorings in the second half of February, with a wide range of temperature, salinity, and oxygen properties reflecting a sporadic input of heterogeneous LSW formed in or near the boundary current."

However, we concede that the text currently does not give a sufficiently detailed explanation of the possible drivers. We believe that the large range of properties is due to spatial inhomogeneities in the boundary current during the early stages of convection; i.e. convection occurs in "patches", and at the start of the convective season, newly convected water can coexist next to old boundary current water with properties closer to IW, and/or convected water with slightly different properties. As convection progresses, the boundary current becomes more and more homogenized, and the high-frequency variability subsides. High-frequency variability in properties in the earlier stages of convection is also observed in Cuny et al. (2005, JPO), who discuss convection within the boundary current further upstream. They also relate this difference to spatial inhomogeneities, which they suggest could result from spatial variability within the boundary current regime in atmospheric forcing or preconditioning.

We will add a brief discussion of these possible explanations in section 3.2 in order to more clearly state what the drivers for the high-frequency variability could be.

Specific comments:

Abstract: The abstract is accurate, but I was surprised to find no mention of the Argo float analysis, nor the authors' inferences regarding the supply of LSW to the boundary current.

Thank you for the suggestion, a sentence about the Argo analysis will be added.

L19-22: These claims should be supported by citations.

Citations will be added

L71-72: Please clarify (briefly) in what way the observations are optimized. I presume the authors are referring to the selection of instrumentation and the locations of the instruments across the section

The observations are optimized primarily to capture the variability of the boundary current transport, as well as the depth of interfaces between water masses. We will add this information to the paper

Fig. 1: This is an excellent introductory figure. However, I did not see any details given in the text regarding the calculation of the mean salinity. E.g. how are the Argo floats binned into horizontal grid boxes to create this figure? Is this an average over all seasons? What criteria (e.g. quality controls) are applied to decide which Argo profiles to include/exclude?

The Figure is an average over all seasons from 20 years of data, using all data that were flagged as 'good' in the Argo catalog. The data are binned into overlapping 0.25x0.25 degree bins. The caption will be updated to include this information

One aspect of the Labrador Sea circulation that this figure does not highlight is the properties and volume of LSW and other water masses. A section across the central LS would show this nicely (though Argo measurements may be too shallow), and would complement the discussion in section 1 (e.g. lines 30-33, lines 47-51). Note that this simply a suggestion for the authors, which they are welcome to take or leave as they see fit.

We will add more specific references to refer the reader to existing section plots of the Labrador Sea; i.e. Yashayaev et al. (2017, GRL) for T, S, and O2 sections along AR7W, and Zou et al. (2020, Nat. Geosciences) for average T and S properties from the OSNAP array.

Also, why have the authors used Smith and Sandwell (1997), rather than more recent bathymetric products?

The product used is the most recent version of the Smith and Sandwell product (SRTM15+ from 2019), but we used the original reference for the dataset. We will update with a citation of the actual dataset used.

Finally, the directions of the arrows in this figure are difficult to discern. I think I see a flow reversal across the mooring array, but it is difficult to tell. I suggest that the authors use larger, wider arrowheads here.

The figure will be changed with larger flow vectors

Fig. 2, L77-79: I understand that the plotted properties are averages across four cruises. Were the measurements made at the same locations on each cruise? If, so it would be appropriate to indicate these locations on the plot. If not, then some additional explanation is required to explain the procedure via which the measurements were gridded to create these plots.

The measurements are mostly from standard stations occupied during each cruise. We will add symbols showing the locations from one representative cruise.

L113-115: The authors should explain their choice of density threshold for the mixed layer depth. If this choice is standard then citations should be given, or if they have selected it then they should explain why they used this specific threshold, and discuss the sensitivity of their results to this threshold.

References will be added.

L116-118: I did not see a similar export criterion for determining Lagrangian floats in the cited studies. By the authors' definition, floats will be considered to have been "exported" if they merely enter the boundary current across the 3000m isobath, remain there for two subsequent profiles, and then leave the boundary current without returning. This does not conform to my conception of "export", and requires further explanation or possibly modification.

The export criterion is similar to definitions used in the Georgiou et al. study in the sense that they tracked floats that were found in the boundary current near Greenland and determined whether or not they stayed in the boundary current or entered the interior, noting that floats sometimes have short "excursions" into the interior before returning to the boundary current. The criterion was meant to allow for such short excursions after floats enter the BC.

We understand the concern about the definition of export, and addressed this with a short discussion and figures in the attached supplement. We tested a stricter criterion, requiring floats to leave the Labrador Sea in the boundary current south of 53N, as suggested by another reviewer, and found that the resulting export estimate is almost unchanged. We hope that this is sufficient to show that our definition does not lead to major biases. We will also change the wording in the methods section to clarify that the method looks at floats entering the boundary current, rather than those that are exported south of 53N, e.g. by calling it "input into the boundary current" (as done in Figure 9b) rather than "export"

Fig. 3: The second paragraph of this caption really belongs somewhere in the main text.

Paragraph will be moved to main text

Table 1 captions: "locations", "depths" and "drifts" should be plural in this caption.

Caption will be changed as suggested

Fig 5.: A very large number of data points are shown on these diagrams (over 70,000 at each morning, assuming that the 15 minute-frequency data are used). Consequently, many of the points overlap, obscuring a substantial fraction of the oxygen measurements. To clarify the presentation I recommend instead binning the oxygen measurements into discrete T/S bins, and then plotting the mean O2 in each bin (although other statistics, such as the standard deviation, may be of interest too) on a regular T/S grid.

Thank you for the comments. We already accounted for this, but unfortunately did not mention it in the figure caption. The data points shown in figure 5 are daily averages, and we will add this information to the caption.

L163-164: The implication here is that the water properties vary much more slowly over the rest of the year, but the authors have not plotted the time series that would show this. It would help to show plots analogous to those shown in Fig. 4, but for T and S rather than O2.

While this would be instructive, we feel that the T and S variability is not sufficiently discussed in the paper to justify adding two more figures. Instead, much of this information can be gleaned from comparing figures 5 and 6, and we propose to add a statement to that effect in the text, e.g. "[…] observed within just 20 days (see Figs. 5 and 6)"

L171: Citations are required to support the claim that this criterion is "commonly used".

Citation will be added

L187: Here and elsewhere in the manuscript, the authors should be clear that they are specifically identifying occurrences of what I would refer to as "deep convection". More generally, convection takes place frequently in the surface mixed layer due to local static instabilities, but only penetrates deep enough to form LSW in the interior of the Labrador Sea during winter.

We will clarify this by calling it "deep convection" throughout the manuscript

L190-191: It may be that I am misunderstanding this statement, but it looks to me like most of the floats measuring deep convection are offshore of the 3000m isobath.

This is phrased somewhat confusingly, and will be clarified in the revision. The statement is meant to say that out of those floats that do measure convection inshore of the 3000m isobath, most are found close to the interior convection patch, i.e. concentrated on the western side of the basin at about 57N.

Figs. 8-9 and in the text: Spiciness is missing units; I believe they are usually kg/m^3. If it has been normalized then the normalization should be given.

Units will be added

L228-229: I found "a wider range of export and mixing time scales" to be unclear, and I do not think that the authors have provided evidence to support this claim.

This statement was meant to convey that the boundary current upstream has received more LSW input from both the interior and local convection, and this leads to a more homogenized set of properties. We will change this sentence to reflect this – e.g. "By May, the boundary current has become more homogenized, as evidenced by the higher number of observations in the central O2 bin, suggesting an increase in LSW input."

L237-238: While the authors clearly explained in section 2 how they identify Argo floats moving from the interior LSW to the boundary current, I am unclear on how they have converted this information into an estimate of the LSW flux into the boundary current. Is this derived from some combination the number of Argo floats and the layer thicknesses measured by each Argo flat as they enter the boundary current?

As explained in the discussion in the supplement file, the "LSW input" estimate is not meant to represent an actual volume flux, but is simply an estimate based on the number of floats entering the boundary current during each time step. We will add this information to the manuscript in the methods section

Additionally, the authors should discuss whether sampling biases may be influencing this calculation. The implicit assumption underlying this calculation is (presumably) that the LSW is densely sampled by Argo floats with similar numbers of samples in each 5-day period. Deviations from this ideal (which seems likely, given the limited number of float locations shown in Fig.7) may introduce biases/uncertainties into the distribution of LSW inflow to the boundary current as a function of time, which should be handled appropriately.

As touched upon in section 3., Argo floats are a valuable tool, but due to their lagrangian nature are also unable to sample a region with truly homogeneous spatial coverage. We are aware that an estimate of LSW input over time based on a limited number of floats may not fully reflect the true underlying variability, and we will add a sentence to the revision more clearly stating that this may bias the curve shown in figure 9b.

L257-258: Alternatively (as the authors have indicated before) the LSW could originate from convection occurring in or close to the boundary current, as suggested by Fig. 7.

This paragraph was only referring to the input of LSW from the interior, we will clarify this in the revision

L293-294: Should we expect a correlation with the current speed? For advection of O2 down a mean O2 gradient, southward velocities would produce a positive O2 tendency (d(O2)/dt>0), while northward velocities would produce a negative O2 tendency (d(O2)/dt<0). However, the O2 concentration is equal to the time-integral of its time-tendency, so for a fluctuating flow we might expect a stronger correlation between the O2 concentration and the time-integral of the southward velocity than with the southward velocity itself.

Thank you for pointing this out, we will adjust the argument to no longer use the correlation of O2 and current speed, and instead make a more qualitative statement.

Also, it looks to me like the modal O2 concentration is higher at K10 than at any of the other moorings (compare the May O2 concentration of almost 310 umol/L with those at the other moorings, for which the modal concentrations only reach ~305 umol/L). If the O2 concentration at K10 is the result of southward advection in the boundary current followed by northward recirculation, how does it achieve higher O2 concentrations than the moorings within the boundary current?

As mentioned in the text, the K10 mooring can at different times be either within the DWBC or in the recirculation regime, so the highest O2 concentrations don’t have to be associated with the recirculation. Also, T/S values for the highest O2 measurements at K10 are close to properties of LSW in the interior of the basin (see fig. 5), so these high O2 values may result from LSW entering the boundary current from the interior and mixing less with IW than further onshore (i.e. at K9).

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-185/bg-2021-185-AC4-supplement.pdf