

Biogeosciences Discuss., author comment AC2
<https://doi.org/10.5194/bg-2021-185-AC2>, 2021
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Reply on RC2

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-AC2>, 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 2

Review on "Oxygen export to the deep ocean following Labrador Sea Water formation" by Koelling et al.

General comments

Using moored data at the exit of the Labrador Sea (~53°N), Koelling et al. reported the seasonal variability of oxygen concentration in the outflowing boundary current. The origin

of the oxygen signal in the boundary current was further discussed by comparing property fields between the boundary current and basin interior, and by tracking spreading pathways of newly-convected Labrador Sea Water (LSW) with Argo floats. Finally, the importance of Labrador Sea convection was emphasized by estimating the oxygen supply from the Labrador Sea to the deep North Atlantic.

I find the manuscript well-written and the focus on oxygen export is of interest to many people in physical oceanography and biogeosciences. Even so, I still have one major suggestion. The Labrador Sea has been extensively studied, both in terms of the boundary current variability and the spreading pathways of LSW. Some of the findings in the current study, such as the seasonality and LSW pathways, are similar to previous work. Besides the oxygen export estimates, what new/different information is revealed by the current study? Specifying the novel findings in a Conclusion paragraph will greatly enhance the importance of this work.

Below I list specific comments.

Specific comments

[1]. "The export of oxygen from subpolar gyre is ~71% of the oxygen consumed annually in the upper NADW layer in the Atlantic Ocean between EQ and 50N". This number was only roughly estimated in the Discussion and should not be listed as a key point in the Abstract.

We will remove the exact number and replace it by a more general statement, e.g.: "The export of oxygen was used to estimate the importance of this direct southward pathway of LSW for supplying oxygen that is consumed in the upper North Atlantic Deep Water layer between the equator and 50N"

[2]. Lines 62-64: I believe the importance of LSW in supplying oxygen to global ocean is well known, as stated at the beginning of the Introduction. I suggest the authors to refine their conclusions and implications by specifying the novel findings of the study.

While it is true that the importance of the Labrador Sea is well known, we are not aware of any previous efforts to directly link deep convection, LSW export, and oxygen supply to the rest of the basin, or to quantify this effect. We will rephrase this paragraph to more clearly highlight the novel aspects of our findings.

[3]. Figure 2 caption: Please check "□"

[4]. Lines 88-89: Please include 27.74 isopycnal in Figure 2.

[5]. Line 94: It may not be appropriate to call the saltier waters in the Labrador Current as "remnant of Irminger Water" since the waters are significantly mixed. "Remnant" sounds like that a part of Irminger Water is not mixed but retains in the boundary current. Maybe you can simply say it is a mixture of Irminger Water and convective water.

Comments 3-5 will be addressed as suggested

[6]. Line 116: You are defining the export from interior Labrador to the boundary current, instead of the export out of the Labrador Sea (stated in Line 110). Floats that once exported to the boundary current (and stayed for at least 2 cycles) could possibly re-enter the basin interior. A more rigorous selection would be to choose floats that entered the boundary current and stayed there until they exited the Labrador Sea (53N). That would

give a smaller number of usable floats but appear to be more appropriate.

We appreciate the comments on the definition of export, which were also shared by other reviewers. We tested the suggested more rigorous criterion, and found that it did not change our results - see discussion in supplement file for details. We will also change the wording in the method section to clarify that we are defining the input of LSW into the boundary current, rather than the export south of 53N

[7]. Figure 3 caption: Suggest using "convective interior" or "interior" instead of "convection".

Will be changed as suggested

[8]. Figure 4: Please make y-axis consistent between panels. The increase of oxygen in March 2018 is quite sharp compared to March 2017. Could you please include a few comments on that?

The y-axis is the same for K8-K10, and is only shifted down by 5uM for K7 since mean concentrations are lower, but the vertical extent of each axis is the same. This is done to "zoom in" as much as possible on the curves to highlight the variability while still showing all data points. We will add this information in the caption, but we can also adjust the axes limits to be consistent between all panels if preferred.

Reviewer 4 suggested to add a discussion of interannual variability, which will be included in section 4.2. We can add a brief analysis of the differences between 2017 and 2018 in that context.

[9]. Figure 5: Why are some high oxygen profiles ($\sim 10 \mu\text{M}$) associated with relatively high temperature ($\sim 3.5\text{C}$) and salinity (~ 34.9) at K10? Does that indicate a different source/route of the oxygen input?

This is an interesting question, but probably goes beyond what is discussed in this study. Note also that these are just two days out of the entire record. These measurements also occur at a time with the highest velocities measured at K10 ($>25 \text{ cm/s}$ southward) and show higher densities than usually measured at this site, so they may be related to mooring knockdown to depths much greater than 600m.

[10]. Lines 175-176: If convection occurs in the boundary upstream of 53N, wouldn't the density anomalies propagate downstream to the array? Do you see that density signal at 53N?

There is no clear density signal associated with the arrival of newly convected water, as discussed in line 167. Whether or not a density anomaly associated with upstream convection would propagate to the array would depend on the degree of mixing and restratification occurring between the two points. The lack of a detectable density anomaly could therefore mean that these processes have obfuscated the signal of convection by the time the water reaches 53N

[11]. Line 211: I do not think everyone is familiar with spiciness. I suggest including a paragraph describing how the variable is calculated.

We will include a short description of spiciness.

[12]. Lines 238-240 & Figure 9b: If you separate the exported LSW from convection in the boundary current, will the temporal variability of LSW input be different?

This is a very interesting question, which we discussed in the supplement file with a new figure showing the input from boundary and interior separately. There does appear to be a difference in timing between boundary and interior convection, and we propose adding a brief discussion of this in the revised manuscript

[13]. Lines 246-248: I do not think I fully understand this statement. Could you elaborate?

This statement is meant to show that the increase in O₂ at K9 cannot be related to increased export of older LSW; i.e. the O₂ in the boundary current increases because of recently ventilated water. Since almost all measurements at the Seacycler mooring in December lie in the 304uM bin (and none are above it), they cannot be the same water mass found in the boundary current in February-April, with concentrations as high as 315uM.

[14]. Line 252: The 12-month time series of LSW input is heavily smoothed (5 months). The 1month lag could be entirely artificial.

The time series is smoothed with a 5-point (20-day) filter, not 5 months. We also show the data in the original 5-day bins in the supplement file, and can add this to the revised document if needed to show the actual data along with the smoothed curve.

[15]. Line 266: What is "the boundary with of the LC"?

Line 273: What is "the border of the LC and DWBC"? I suggest showing the velocity structure superimposed with the mooring locations for better illustration.

The velocity structure is shown and discussed in depth in a recent paper on the 53N current meter data, Zantopp et al. (2017). We will add a sentence in the revised manuscript referring the reader to that paper for more details

[16]. Lines 290-291: This contradicts Figure 2, which shows smaller oxygen concentrations at K10 compared to the other moorings.

We are referring here to the spread of O₂ measurements shown in figure 10. As seen in figure 2, the K10 mooring is near a larger horizontal oxygen gradient, so the spread may be related to meanders of the current, as mentioned in the following lines.

[17]. Line 349: I do not think ISIW, the locally convected water in the Irminger Sea, is the same as Irminger Water. The IW is the saltier and warmer Atlantic-originated water that flows in the boundary current. What enters the Labrador Sea is likely a mixture of ISIW and IW.

We appreciate the clarification, and will rephrase this in the revision

[18]. Lines 377-378: I think this is a very intriguing estimate. If recirculation at K10 is considered, how will the (net) export percentage change?

[19]. Line 434: Again, recirculation is not considered and the percentage (71%) could be an overestimation.

This is again an interesting question, but may be beyond the scope of the current study to address. Zantopp et al. (2017) quantified the recirculation strength from LADCP data, and found that it is typically about 10-20% of the DWBCs strength. However, the effect of the

recirculation on the net oxygen export will also depend on how much of the newly convected LSW enters the recirculation, which may also have some input of water from the North Atlantic Current. We can add the 10-20% number to the manuscript as an upper bound for how much LSW could be recirculated northward

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2021-185/bg-2021-185-AC2-supplement.pdf>