

Biogeosciences Discuss., author comment AC1  
<https://doi.org/10.5194/bg-2021-185-AC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Reply on RC1

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-AC1>, 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 1

#### Specific Comments

*1) I have some technical comments about some of the figures which I will address later, but I do have a specific comment about Figure 8. Figure 8 and Figure 9a show exactly the same thing. I find unnecessary to have two figures which shows the same, considering that the paper is actually long. I am not asking to short the paper. I do not have a problem with that but reducing the number of figures is already an improvement. Everything that is described in session 3.2 can be easily and actually better follow by looking at Figure 9a. Figure 8 might look fancy but 3D figures are difficult to look on a 2D plane, there is always some part which is hidden. But I could see all the points you described much easier in Fig.9a. I then suggest to remove this figure and refer directly to Figure 9a for the seasonal cycle in session 3.2*

We understand the concern about showing duplicate information in Figure 8 and 9a. However, we believe that having figure 8 is valuable because the arrival of LSW at 53N can be seen more intuitively. We were also hoping to use Figure 8 as the highlight figure shown on the article page on the website, which may not be possible if it is not shown in the paper itself.

*2) Doesn't make more sense to compare the calculated export of 1.57 Tmol instead of with the global oxygen loss of 70 Tmol year<sup>-1</sup> with the value calculated only for the deep Atlantic. I think it is an oxygen gain of about 1.7 Tmol year<sup>-1</sup> for the deep ocean according to the extended table from Schmidtko et al., 2017.*

The value for the deep Atlantic is already mentioned in the previous lines (line 383-384: "The deep North Atlantic is one of the few ocean regions that has not experienced significant deoxygenation in recent decades (Schmidtko et al., 2017), which may be related to the continued formation and export of LSW and NADW."). However, we do not currently mention the exact value from the Schmidtko et al. paper, and will include this in the new version. Nonetheless, we believe that mentioning the global value is still important for context.

*3) The calculation of the oxygen export at 53°W of 1.35 Tmol and 1.57 Tmol are of course approximations, so I think it should be mentioned. The same for the calculation of the oxygen consumption of 2.2 Tmol by organisms. Regarding this calculation specifically I do have some concern. The authors use a global value of aOUR of 0.1 μmol kg<sup>-1</sup> year<sup>-1</sup> at 1500 m from Karstensen et al., 2008. First of all, the calculation was made separately for the Pacific and Atlantic oceans, although it seems to be the same for both oceans. Nevertheless, I would indicate the value for the Atlantic. Moreover, the aOUR values are on an order of 10 μmol kg<sup>-1</sup> year<sup>-1</sup> below the euphotic zone and decay exponentially with a value assumed to be 0.1 μmol kg<sup>-1</sup> year<sup>-1</sup> at 1500 m. The authors chose this last value for their calculation, however the isopycnal range they considered "27.68 kg m<sup>-3</sup> and 27.8 kg m<sup>-3</sup>" is not exactly at 1500 m. It is of course changing according to location, so the layer can be as deep as 1500 m but also as shallow as about 200 m or shallower so basically if you always assume that the thickness of this layer is constant (800 m, which is also an approximation because it of course not constant) then your consumption value varies from about 200 Tmol to about 2 Tmol which means the southward export of LSW might supply between 0.71% to 71% of the oxygen demand in this layer.*

We addressed this comment by redoing our calculation using the depth-varying profile from Karstensen et al., see discussion in supplement file. The resulting value is about 3.79Tmol/year, so 42% would be supplied by the LSW export. We will include this as a range in the revision (i.e. 42-71%)

## **Technical Comments**

**Line 31:** *Irminger Water appears here the first time. You could think of define already here the acronym (IW). In the rest of the manuscript, I noticed that sometimes you write Irminger Water other time IW. I think the correct way is once the acronym is defined to stick with that. So please replace Irminger Water with IW in the text after that is defined in line 31. The same for Labrador Sea Water (LSW). Once defined it should be mentioned always as LSW. I found some of them which I highlighted further down but I might have missed some more.*

**Line 31:** "originating in the Irminger Sea" instead of "originating in the Atlantic Ocean"

**Line 83:** wasn't Pickart et al., 1997 the first to define LSW at this boundary. It should be cited before Zantopp et al., 2017.

**Line 123:** "LSW" instead of "Labrador Sea Water"

**Line 124:** "LSW" instead of "Labrador Sea Water"

Suggested changes above will all be included

**Line 125 until end of session 2.2:** here Figure 3 should help to understand the method. But I found this paragraph a bit confusing. Figure 3 is made out of three panels (a, b and c) they should be motioned in the text and help to understand the method.

We will move part of the caption of this figure to the main text, and rewrite it to make the method more clear

**Line 140:** could you give a precise range instead of bigger than 0.5?

We can add the range if needed; see also discussion below

**Line 140:** I do not follow the argument here. If you say that the correlation between  $O_2$  saturation and temperature is high it should be an indication that concentration changes are due to temperature-driven solubility differences unless the correlation is lower than the one between  $O_2$  and temperature. That's why I suggested above to give a range and not simply saying that is bigger than 0.5.

We believe that the overall statement is true, but it may have been phrased in a confusing way. "Saturation" in this case refers to saturation percentage, so if changes were purely due to solubility (I.e. saturation stays at a constant percentage, but concentration changes with temperature), then the correlation of saturation percentage with temperature would be zero, but correlation of  $O_2$  concentration and temperature would be high.

We therefore interpret the fact that there is still a high correlation with saturation percentage to show that this is not the case. Another (and perhaps better) way to phrase this is that the two water masses discussed in the paper are distinct in both  $O_2$  concentration and  $O_2$  saturation percentage, with both being higher for LSW. We will rephrase this paragraph to more clearly state this

**Line 154:** February, March and April 2017 or 2018 or both?

The values given are for 2017, we will add this information

**Line 154 & 155:** "oscillate between values typical of the months prior" what do you mean?

The word "oscillate" may not have been a good choice here, we will change it to "vary between [...]"

**Line 179:** It looks like you used a dataset that is not described in the data and method session. Please add a description of the dataset from Holte et al., 2017 in the data session.

We will add a section "additional data" where under data and methods where this will be

added

**Line 189:** *"...but are more concentrated towards the sides of the patch..." To me doesn't look like it. A lot of the red dots are in the middle of the patch. What is more concentrated on the sides are the yellow and green. Do you mean that?*

This statement referred to the fact that there are some differences in the distribution between the contours and the points showing convection. For example, there are parts in the interior Labrador Sea where the convective activity inferred from the Holte et al. dataset is maximum, yet there are no points from the float dataset showing convection.

This is likely due to the different methods, with the contours showing a fraction of all profiles measuring convection, while the points only show the last profile with deep mixed layers for each float; so if a float measures several profiles with deep mixed layers in the convective interior while traveling towards the boundary current, only the last profile would be shown as a "convection profile" in the figure.

We will rewrite this sentence to be less confusing, to "The points generally line up with the mean picture from the Holte et al. (2017) data, with differences between the two likely occurring due to continued modification during convection."

**Line 201:** *"a handful", you could quantify that with a number, 7 right?*

Will include the exact number in the revision

**Line 215:** *IW instead of Irminger Water*

**Line 215:** *LSW instead of Labrador Sea Water*

**Line 227:** *O<sub>2</sub> instead of O2 (twice)*

**Line 229:** *O<sub>2</sub> instead of O2*

Changes above will be included

**Line 229:** *What is the central O<sub>2</sub> bin?*

Central O<sub>2</sub> bin refers to the most commonly measured concentration for each month

**Line 242:** *"another." Instead of "another:"*

Will be changed

**Line 246:** *"Oxygen concentration in the interior before January are lower than those at 53°N during March-July..." to me it doesn't look too much lower. Maybe you could quantify it? Moreover, the SeaCycler mooring has two picks in January. Could you explain why? I think this should be mentioned in the results.*

The value of the central bin (304uM for Seacycler in December, 306uM for K9 in April) is fairly similar, particularly considering measurement uncertainty. However, 304uM is also the maximum value measured at Seacycler in December, while the maximum value at K9 in April is 315uM. So export of older water that is already present in the interior of the basin before convection starts in January cannot explain the high boundary current O<sub>2</sub> during the spring. Some numbers will be added to the text to make this statement more clear.

The two peaks in January occur because convection reaches the depth of the sensor some time during the month, so measurements before show generally lower O<sub>2</sub> values, while measurements after show higher ones. We will add this information to the manuscript as well

**Line 273:** "LC" instead of "Labrador Current"

**Line 279:** "density." Instead of "density:"

**Line 362:** "salinity." Instead of "salinity:"

Line 372: "LSW." Instead of "LSW:"

Changes above will be included

**In section 4.2 (line 375)** you calculated a supply of oxygen for the 6 months (March to August) of 1.35 Tmol from an increase oxygen increase of 6  $\mu\text{mol/L}$ . Could you specify the increase of oxygen for the whole year as well? I guess the 1.57 Tmol/year that you calculated for the whole year is determined by this value since all the other parameters stays the same (mean velocity, layer thickness and section width), otherwise it would be 2.7 Tmol/year.

The average increase for the whole year is 3.5  $\mu\text{mol/L}$ , this will be added in the revision

**Line 376:** "21.2 mol m<sup>-2</sup>" instead of "21.2 mol m<sup>2</sup>"

**Line 437:** "NADW" instead of "North Atlantic Deep Water"

Changes above will be included

## Figures

First of all, I think the authors did a good job with the figures, they are clear, with a good choice of the color schemes and font size. So here are just few suggestions for further improvements:

**Figure 3:** Figure caption is really long and the second part it looks like it belongs to the text in the session and not as caption. Moreover 27.74 kg m<sup>-3</sup> should not be written in italic. Figure 3a it is a bit confusing what is displaying since it is nowhere written what is the thick green line and what are the thin white lines. Figure 3c, why the dark blue line stop at 27.75 kg m<sup>-3</sup>?

The second part of the caption will be moved to the main text, and we will add clarification to the caption for Figure 3a. In Figure 3c, the dark blue line stops at 27.75 kg/m<sup>3</sup> because this is the surface outcrop for this profile, so there is no water at lighter densities.

**Figure 5:** The reference at 600 dbar to me seems unnecessary. Moreover, the comparison with the work from Atamanchuk et al., 2020 (purple stars and ellipse) and from Pacini et al., (2020) (green symbols and ellipse) sounds interesting but I do not find it discussed in the paper. If you put that into a figure then it should be discussed, otherwise remove it. Finally, how is the anomaly calculated?

The reference to 600dbar will be removed to be more clear. The two references are the studies used for the points shown in green and purple in the figure, which are typical properties of IW and LSW. The oxygen anomaly is relative to the mean for each measurement location, which will be mentioned in the revised figure caption,

**Figure 6:** *The blue to red colormap for absolute values is a bit confusing, especially if it has been used in the previous figure for the anomaly. Also here the reference to 600 dbar is not necessary.*

We will change the colormap and remove the 600dbar reference.

**Figure 7:** *the red dots are too small; I can hardly see them. Could you make the symbol the same size?*

We will increase the symbol size.

**Figure 10:** *The K9 is already displayed in Figure 9a, it is a repetition. Comparison can be easily done by looking at the two figures and best if you plot K7, K8 and K10 one below each other and with the same length of K9 in figure 9a. Moreover, I would also put for the 3 other moorings the black line showing the bin with the highest number of observations.*

These suggested changes will be included in the revision.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2021-185/bg-2021-185-AC1-supplement.pdf>