Biogeosciences Discussions

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Reply on RC3

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-AC3, 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 3

Minor Revisions

- *More appropriate handling of uncertainty in the back-of-the-envelope calculation.*

I commend the authors for attempting to put numbers to the supply of oxygen to the deep waters of the Atlantic - this is a valuable contribution. However, I felt that numbers like 1.57 x 10^{12} mol O2 yr^{-1} and 71% imply a level of accuracy that is inconsistent with the uncertainty and assumptions that have gone into their calculation. I would ask that the authors present the numbers as a range that takes into account the uncertainty *associated with each component of the calculation.*

I note that Dr. Stendardo also picked up on this point, providing further specifics on one potentially large source of uncertainty.

We discussed possible changes that to include uncertainty estimates in the supplement file. The proposed changes would allow us to add an uncertainty estimate to the O2 export (e.g. $(1.57 \pm 0.42) \times 10^{12}$ mol O2 yr^{-1}), and give the estimate of O2 consumption in the North Atlantic south of 50N as a range (e.g. 42-71%).

- *Inference of timescales and hypothesis of eddy-driven exchange from central Labrador Sea.*

The authors use the difference in the timing of the seasonal oxygen peak in the central Labrador Sea and at the moorings to infer a speed associated with oxygen transport between the two (paragraph beginning Line 255; discussed again Lines 320-323). Noting that this is much larger than the speed of the time-mean flow, they use this is as evidence for the role of time-varying, eddying flow in driving this transport. However, the authors previously argued convincingly that the boundary current peak was more than likely arising from convective processes within or close to the boundary current itself. Therefore, inferring instead a timescale of exchange from the central region appears inconsistent with this. Please could the authors clarify if I am misunderstanding something here, or else revise these statements, which I don't believe their observations support.

The previous discussion suggested that the initial increase of oxygen (i.e. February-March) at 53N is associated with boundary convection, but both boundary and interior convection contribute to the increase later in the season. This paragraph was meant to only discuss the mechanism by which the part of LSW convected in the interior enters the boundary current. We concede that it was unfortunate to phrase it as "If the bulk of the LSW arriving at 53◦ N originates in the center of the basin near the SeaCycler mooring", which might be interpreted to imply that convection in the boundary current is not important; we will rephrase this paragraph to clarify.

The importance of both interior and boundary convection is also evident from figure 7, and the new version of figure 9 shown in the supplement file. The new analysis also shows that the peak export does occur somewhat later for the interior LSW than for the combined estimate, so the time scale for export of LSW sourced from the interior will have to be revised to 1-2 months, implying a range of export speeds from 4.4-9 cm/s.

- *Evidence for and against local ventilation at 53N*

The authors make an effort to affirm that the oxygen and watermass changes at the mooring locations are most likely driven by processes taking place upstream, rather than occurring locally (i.e. from surface forcing impacting the water column above). While I agree that this is probably true, I thought some of the lines of evidence presented were not entirely conclusive. In particular, the authors cite the lack of density changes over a seasonal cycle (paragraph starting Line 67). However, Fig 6 (and to some extent Fig 5) do indeed show density changes on the order of 0.025 kg m-3 over the seasonal cycle, indicative of a slight warming and freshening concurrent with oxygen increases. These density changes could be significant in this weakly stratified region. Of course, such density changes may or may not be indicative of local surface forcing (more likely there is a seasonality in the doming of isopycnals coincident with the strength of the gyre) but the *authors' assertion that there are no changes is likely to confuse readers.*

The authors state that an absence of density changes confirms that local ventilation is not taking place. However, convection (and therefore local ventilation) to a certain depth, need not be accompanied by diabatic transformation at that depth. It is possible that homogenization of the upper water column could take place without changing the density at 600m itself, since it requires the densification only of the waters above. Diabatic changes at 600m would indicate a mixed layer extending into stratified water much deeper than 600m.

It may be the case that the strongest evidence for the absence of local ventilation comes from the lack of static instability relative to surface density, which the authors allude to on Line 170, but the data for which they don't show. The authors should consider showing that data, and centering this argument in their reasoning for changes being driven by upstream processes.

We appreciate the suggestions and explanation, and will focus the argument more on the near-surface density data in the revision.

- *The role of solubility in oxygen variations.*

Lines 139-141: I didn't follow the argument concerning the correlation of oxygen saturation with temperature, and how this refutes the possibility that oxygen concentration variability simply reflects solubility changes. Further, I was not sure why solubility-driven changes should be considered less relevant here? I would have, perhaps naively, thought that solubility derived changes would be a relevant and important mechanism by which LSW is oxygen replete relative to warmer waters. Could the authors please elaborate on their explanation here and clarify the point that I am missing?

(copied from response to reviewer 1) 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 O2 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 O2 concentration and O2 saturation percentage, with both being higher for LSW. We will rephrase this paragraph to more clearly state this

- *Clarity in Figure 7.*

I found myself confused about the labelling of points in Fig. 7. I understand from reading the caption that all of these points are convection locations, with the yellow and green points being the convection locations of floats that subsequently showed export across 3000m. However, the marker styles and legend could be read as suggesting that convection is taking place only at the red points, and that the yellow/green points are perhaps the locations of export or some other notion associated with the pathways. Either way, I initially inferred that the yellow, and green points, are somehow functionally distinct from the red points. In fact it is the case that all of the yellow points are also red points, and all of the green points are also red and yellow points. A possible solution would be to keep all the marker styles the same, but with different colors - clarifying that these *points fundamentally show the same thing (convection location) but with distinguishing characteristics (pathways following convection). Likewise, the wording of the legend should be changed to be clearer in this regard (e.g. "convection profiles, float not exported; convection profiles, float exported (any time); convection profiles, float exported Jan-Feb").*

Thank you for the suggestions, we will change the figure accordingly to be easier to understand

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-185/bg-2021-185-AC3-supplement.pdf