

Biogeosciences Discuss., referee comment RC3
<https://doi.org/10.5194/bg-2021-185-RC3>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



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

Graeme MacGilchrist (Referee)

Referee 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-RC3>, 2021

This paper considers the supply of oxygen to the deep waters of the North Atlantic using mooring observations in the southern Labrador Sea boundary current, together with Argo observations across the region. The authors find seasonal enrichment of oxygen at 600m in the density range commonly associated with Labrador Sea Water. They infer that this enrichment arises from the mixing of recently ventilated water with relatively oxygen-depleted waters entering the region in the north from the Irminger Sea. The authors suggest, making use of Argo floats pathways and profiles, that the recently ventilated waters most likely arise from convection within or close to the boundary current upstream of the moorings, as opposed to that in the central Labrador Sea. A substantial role of the boundary current for both overturning and ventilation has been considered plausible for years, but this is the first study to provide clear evidence for its impact on the supply of oxygen to the deep ocean.

The study is well written and results clearly presented. The findings are novel and notably far-reaching, considering the relatively limited dataset that the authors have access to. I would therefore strongly recommend the paper for publication, subject to a few minor revisions that I outline below.

Sincerely,

Graeme MacGilchrist

Minor Revisions

1. 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 1.57×10^{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. Stendaro also picked up on this point, providing further specifics on one potentially large source of uncertainty.

2. 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 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.

3. 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.

4. 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?

5. 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").