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

Anonymous Referee #4

Referee comment on "Oxygen export to the deep ocean following Labrador Sea Water formation" by Jannes Koelling et al., Biogeosciences Discuss.,
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In this study the authors use moored hydrographic/oxygen measurements in the outflowing boundary current of the Labrador Sea at 53N, combined with Argo float profiles from the Labrador Sea, to investigate the timing of Labrador Sea Water (LSW) outflow and its implications for the subsurface O₂ budget. Their central finding is the outflowing boundary current at 600m exhibits a relatively rapid increase in O₂ concentration over a period of a few weeks in early March, accompanied by a shift toward lower spice (but with approximately no change in density), and accompanied by a substantial increase in the temporal variability of the water mass properties. The modification of the boundary current water mass properties draws them closer to the properties of LSW and persists for a few months, before gradually decay back toward lower-O₂, spicier properties that more closely resemble Irminger Water. Consistent with this finding, Argo floats from 2010-2020 primarily enter the Labrador Sea boundary current from the interior primarily between March and May. The authors draw inferences regarding the connection between deep convection in the interior of the Labrador Sea and export in the boundary current, and estimate the contribution of O₂ export via the boundary current to the subsurface O₂ budget.

This manuscript is a good fit for Biogeosciences because it substantially address both physical and biogeochemical aspects of Labrador Sea processes. The findings are novel and substantially advance currently understanding. The manuscript is well-written, with clear figures that largely support the claims made in the text. However, various aspects of methodology were not described in sufficient detail to allow reproducibility of the authors' results. I am also concerned about the authors' interpretation of the timing and amplitude of O₂ variations inside vs outside the outflowing boundary current. Finally, the manuscript lacks discussion regarding how representative these findings are of other years (noting that Labrador Sea exhibits pronounced interannual and decadal variability), and lacks explanation of the high-frequency variability that accompanies LSW arrival. These concerns are listed below, among various other specific comments on the manuscript. In my opinion, these concerns are collectively sufficient to warrant major revision of the

manuscript before it can be accepted for publication

General comments:

1. 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.

2. 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.

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.

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

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

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?

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 is simply a suggestion for the authors, which they are welcome to take or leave as they see fit.

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

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.

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.

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.

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.

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

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

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.

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.

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

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.

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.

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.

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.

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 float as they enter the boundary current?

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.

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.

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 ($d(O_2)/dt > 0$), while northward velocities would produce a negative O₂ tendency ($d(O_2)/dt < 0$). 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.

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 $\mu\text{mol/L}$ with those at the other moorings, for which the modal concentrations only reach $\sim 305 \mu\text{mol/L}$). 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?