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Biogeosciences Discussions

Interactive comment on "Mass, nutrients and oxygen budgets for the North Eastern Atlantic Ocean" *by* G. Maze et al.

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We thank the referee #1 for her/his careful reading of the submitted manuscript. Below, in section 1 we answer and propose modifications to the mansucript in order to address all the key issues pointed out by the referee. Other minor issues are answered in section 2.

1 Key issues

1. **"The quasi-steady state assumption. [...] Some justifications of this assumption will be needed"**

We did not explicitely resolved the accumulation terms (hence the steady state assumption in Eqs(1-5): null rate of change over the period 2002-2006) due to a lack of information to constrain those terms. Instead we prefered to account for them when estimating the error bars on constraint residuals. Thus those error bars were chosen to include the variability of tracer transports across the OVIDE transects. (Compare for instance the interannual variability of NO3 transports through the OVIDE transect given page 4336, lines 19/20: -1 ± 49 kmol s^{-1}, 16 ± 37 kmol s^{-1} and 20 ± 32 kmol s^{-1} with the NO3 conservation equation residual error bar of 10kmol s^{-1}).

We acknowledge that the text may not be clear enough about this important point of the model set-up. We thus propose to revise the manuscript page 4330 in the paragraph lines 21 to 26 to clarify this issue along the line of the above answer to your comment.

This issue is somehow redundant to the biggest concern of referee #2 about the temporal scales of our results. Note that we proposed to referee #2 to remove any explicit mention of the "decadal" time scale.

2. **"I am not sure why oxygen solubility should be included in the optimization process"**
In principle, the oxygen solubility and total oxygen conservation equations 4 and 5 could be merged into a single conservation equation for the Apparent Oyxgen Utilization (AOU). This, in fact, is what we used to relate B' to B (see Eqs 7-8). The reason why we explicitly added the oxygen solubility in the optimization process is for the abiotic air-sea oxygen flux to be constrained by two conservation equations instead of one. Although analytically, using these two conservation equations (oxygen solubility and total oxygen) does not add more information to the system, it provides the method clues to optimize more efficiently the abiotic air-sea oxygen flux. Moreover, it is true that oxygen solubility depends not only on temperature but also on salinity (note that it does not depend on pressure).

Although we did used all dependencies to compute the a priori oxygen solubility, we did not included other than temperature dependencies in the conservation equation because (i) source/sink terms would have been difficult to incorporate in the equation and (ii) solubility primarily depends on temperature and the salinity dependency falls below the amplitude of the error bars. Therefore, we think it is appropriate to keep using the oxygen solubility conservation equation as it is and we propose to add an explicit mention of the above comments in the text section 2.2.

3. **About optimized variables: "some clarification will be needed"**
   It is true that the text may not be clear enough with regard to which variables are optimized or not. The optimized variables are: western faces mass transports T, other faces volume fluxes F, biological source/sink terms B and total abiotic air-sea oxygen fluxes J^a. We propose to carefully re-write section 2.2 to be clear about that.

4. **About the method to compute mean tracer concentrations along the box boundaries: "such an approach is problematic"**
   It is precisely because along both eastern faces there are two-layer current structures that we adopted Eq(B2) to compute the a priori mean tracer concentrations. If we use a simple top to bottom concentration average, the a priori tracer transports accross those boundaries are badly represented. This simple method thus allow for the a priori top to bottom tracer transports to be close to those computed using an explicit two-layer structure. This basically means that, if one doesn't have access to the current's structure $T(z)$ but only to layers transports $[T]^{top}$, $[T]^{bottom}$ and to the concentration's structure $C(z)$, then an estimate of the top to bottom tracer transport is best approximated by:

$$[CT] \simeq [T]^{top} \int_{z(top)} C(z)dz + [T]^{bottom} \int_{z(bottom)} C(z)dz \qquad (1)$$

than by:

$$[CT] \simeq \left([T]^{top} + [T]^{bottom}\right) \int_{z} C(z)dz \qquad (2)$$

The text may not be clear enough about the motivation of our choice, so we propose to clarify the text with regard to why we used Eq(B2).

2 Minor issues

1. **About the horizontal mixing and air-sea fresh water flux**
   All faces of the two boxes are mainly oriented so that they intersect the main currents and horizontal tracer gradients perpendicularly. We thus assumed that the horizontal mixing terms were small and omitted them (this was also noted in a numerical simulation, Treguier et al, 2006). This will be specified in the model description section 2.2.

   About the air-sea fresh water flux and E-P terms: based on the analysis of the OAflux dataset (not shown) we estimated that those contributions to the mass budget were below the level of error bar imposed on the constraints residual. That's why they are not included in the budgets. This will be specified in the model description section 2.2.

2. **About the euphotic vs top-to-bottom estimates**
   The referee's comment is correct. However, we did performed some sensitivity studies to the a priori estimates of the biological source/sink term B and found that the a posteriori estimates are not much sensible to them. The conclusion is that even if the a priori estimate is for the euphotic zone, the eventual adjustment to obtain a top-to-bottom value would not change the outcome of the optimization process. We realize that these experiments are not reported in the article and we

propose to do so in light of this subtle precision that the a priori state of B was determined for the euphotic zone.

3. **About the biotic air-sea oxygen flux**
   Again, the referee's comment is correct. We did not intended to state that the biological source/sink term is entirely balanced by the biotic air-sea oxygen flux, though the text is not clear enough about that. In light of the referee's comment and further discussion among the coauthors, we propose to remove any mention to the biotic air-sea oxygen flux in section 4 to only focus on the abiotic flux decomposition. Therefore the paragraph 4335, lines 13-17 will be removed. Note that we also proposed to referee 2 to add the flux partitioning results in a table.

4. **Other minor issues**
   All typos and minor issues will be corrected, thanks for your carefull reading.

Interactive comment on Biogeosciences Discuss., 9, 4323, 2012.