**Biogeosciences
Discussions**

Interactive
Comment

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

1   Key issues

1. **"The aspect of temporal scales considered requires much more clarification"**

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In this study we focus on tracer budgets using the high quality OVIDE dataset of 2002, 2004 and 2006. Therefore, the accurate timescale for the western most faces of the domain is the period 2002-2006.

In order to derive budgets, complementary informations about tracers and transports were required at the Greenland-Scotland Ridge (GSR). Unfortunately, direct observations for the period 2002-2006 are not available. In order to bypass this lack of synchronous data, we selected the level of error estimates to encompass interannual to decadal variability at GSR (Dickson et al, 2008: Arctic–Subarctic Ocean Fluxes: Defining the Role of the Northern Seas in Climate). This implies that there's no specific timescale associated with the GSR data used in this study.

To conclude, the timescale over which budgets described in this study are a priori valid is thus the period 2002-2006. We acknowledge that it cannot be claimed differently and propose to follow your recommendation of removing any mention of the decadal and interdecadal words from the paper. We also propose to insert more details about the relevant timescale of the study along the line of the above answer in the model description section.

2. **"How reasonable is the steady state assumption ?"**
   Referee #1 raised a similar concern. Note that because the accumulation terms (rate of change over the period 2002-2006) are unknown in this area, they cannot be explicitly resolved by the optimization method. Accordingly, the error bars on constraint residuals reflect our lack of knowledge of the system (see answer to referee #1).

   We proposed to referee #1 to add a clear description of this assumption when describing the constraints residual error bars used in the study (page 4330, lines 21-26).

   How uncertainty related to interannual variability in tracer concentrations affect the residuals summarized in the Figures 2 and 3 ? See below.

3. **"It appears to me that the tracer budgets can potentially be just a mean of four quite different snapshots."**
First note that we used OVIDE data from 3 surveys, not 4.

Second, a mean is by definition a synthesis of potentially different conditions. How different those conditions are compared to the mean is given by the standard deviation, and in our case is reflected by the amplitude of the a priori error bars. Taking the NO3 transports as an example (page 4336, lines 15-20): we see that transports go from -1 to 16 to 20. More importantly the error bars on each of these values (49, 37 and 32) imply that 2002, 2004 and 2006 can be very similar conditions as well as very different: we can't call. That's intrinsically expressed by the error amplitude of the mean: $12 \pm 31 \mathrm{kmol\,s}^{-1}$. All the interest of the optimization method is that it combines different source of informations to reduced this error. The optimization method can also be seen as a way to help distinguish very similar from very different conditions.

4. **"I would like to see more discussion on the potential impact of interannual variability on budgets"**
It is clear that the interannual variability in tracer concentrations affect the residuals summarized in the Figures 2 and 3. However, this is taken into account in the optimization method through the amplitudes of the error bars. We did not commented on the direction residuals would take if one a priori tracer concentration should be increased or reduced within the imposed error bars though. We thus propose to comment on this specific issue in the discussion section 5.2. This comment would go page 4337 lines l5-13. One additional discussion could be added around a comparison of the residuals using the mean OVIDE dataset vs the mean of residuals obtained using each OVIDE survey.

5. **"The authors do not comment on any potential seasonal bias"**
On one hand we used the WOA09 annual climatology based on all available observations to date in 2009. Because in this region in-situ observations are sam-

pled preferably in the summer time, it may exist a seasonal bias in the WOA09 data. On the other hand, OVIDE surveys were conducted in late-May to mid-July and therefore a seasonal bias toward late spring/early summer conditions may also be present in the a priori transports estimates across the western faces of the domain.

All the dataset used to determine the a priori state of the model thus point toward a seasonal bias. We agree that this should be added in the text and propose to do so.

The Alvarez et al, 2002 study used data from the Fourex 1997 survey performed in August, i.e. almost in the same period of OVIDE's, so we believe that differences mainly originate from improved transport estimates.

We propose to add a paragraph in the discussion section to comment on the potential seasonal bias of our estimates, especially for the biological source/sink terms and abiotic air-sea oxygen flux partitioning.

2 Detailed comments

- We will add one sentence in the abstract to explain how this region is important.

- All typos will be corrected, thanks for your careful reading.

- About the oxygen solubility: referee #1 raised the same issue, please read the answer to referee #1.

- Page 4328, line 26: We propose to clarify the section 2.2 with regard to which variables are optimized or not. Referee #1 raised the same issue, please read the answer to referee #1.

- page 4330, line 17: Standard transports estimates for the a priori state are listed in table 1. We'll clarify the text about that.

- page 4330, line 24-26: This paragraph will be re-written according to the comments above about the steady state assumption and interannual variability.

- page 4332, line 25: Here by vertical mixing we mean mixing related to the mixed layer dynamic. Text will be clarified.

- page 4333, line 10: We tested the biological oxygen term sensitivity to $r_{O:N}$ redfield ratios from $-180/14$ to $-120/18$. This will be added to the text.

- page 4334, line 22: We don't think the heat fluxes should be shown in a table. However, we now think that all air-sea oxygen flux estimates could be shown in a table and we propose to do so in the revised manuscript.

- page 4335, line 13-17: Correct, this is only true if we assume a steady-state. Note that because it is rather unclear if and how the B term can be directly linked to the biotic air-sea oxygen flux, and in light of the referee #1 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

- page 4437, line 5-11: We agree that this paragraph is quite unclear about the point to be made here. Yes, the sign of the net nitrate transport through the OVIDE line is not significant. This is due to the fact that the net transport is the sum of two large transports of opposite directions, south and north of the Reykjanes Ridge across OVIDE. What need to be said here is that error bars make a null net transport possible for each survey, as well as for their time average. The 'decadal' time scale reference will be remove (see major issues above) and this paragraph rewritten.

- page 4337, line 13: yes, we agree (see major issues above).

- page 4337, line 14-27: We are not aware of any quantitative estimates of nutrient upwelling in this region. Note that even if this region is upwelling dominated, we couldn't explicitly take into account this vertical flux because our model is for the top-to-bottom water column.

- page 4338, line 16: Your comment is correct. We propose to be more precise about all the possible explanations in the text (thus including signal variability).

- page 4340, line 27: we propose to remove the reference to a 'state of the art' stoichiometric ratio.

- page 4341, lne 25-26: what we meant by 'realistic' is that our estimates are consistent with previous studies, and thus the analysis we propose has some 'merit', meaning it cannot be ruled out due to un-realistic results. we agree that this vocabulary may not be the most appropriate to express this idea and thus propose to re-write the sentence.

- table 1: see model details in appendix B. However, we propose to complete table 1 to list all transports and tracer concentrations for all faces.

- Figures 2/3: residuals are the l.h.s. term for Eqs(2-5). This is will be explained in fig's caption.

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