

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

Anonymous Referee #2

Received and published: 30 May 2012

General comments:

This paper presents a detailed calculation of mass, nutrients and oxygen budgets in the North Eastern European Basin and the Irminger Sea – two regions whose hydrodynamic and biogeochemical fluxes are strongly affected by both horizontal and vertical processes. Using an optimization method and a linear model, the authors combine improved transport estimates from recent studies with high spatial resolution observations of tracer concentrations (oxygen, oxygen solubility, nitrate and phosphate) and historical data to generate 4-year climatological mean budgets of the entire North East Atlantic. The results of this study suggest that 70% of water mass is being redistributed between the two boxes separated by the Reykjanes Ridge, while only 30% is exchanged with the surrounding seas. The authors conclude that oceanic oxygen up-

C1582

take is largely driven by an air-sea heat flux cooling which increases the surface ocean oxygen solubility. On the other hand, biological production of oxygen inferred from positive nutrient fluxes indicates that this region is autotrophic and is a net organic matter production region.

This paper is a significant contribution because it attempts to revisit and improve mass, nutrient and oxygen budgets in a region whose patterns of biological production and air-sea fluxes need to be better monitored and explained, in particular in response to inter-annual and decadal variability. In general, the selected scientific approach and methods are valid but are often insufficiently described. Calculations and results are consistent with previous studies that the authors frequently and adequately refer to. The paper is well organized but rather poor editing makes many paragraphs difficult to interpret. Moreover, there are a number of assumptions and potential limitations that are not mentioned or sufficiently commented on by the authors in the paper. I recommend publication of this article only after addressing the concerns listed below:

1. My biggest concern is related to the general disregard in accurately describing and discussing the temporal scales of tracer budget calculations. The authors claim to use a decadal climatology budget yet it appears to me that they base their tracer calculations on data from only four years. It is not clear to me how the tracer budgets were optimized for a decadal time scale as suggested in lines 15-16 on page 4336. Throughout the paper the authors refer to decadal scales and even inter-decadal variability (line 25, page 4330) which appears inadequate considering the four-year OVIDE sampling period considered. Was the WOA09 climatology constructed over a longer period? Are the transport estimates optimized for the entire decade? None of this is clear to me from the domain description on page 4327. The aspect of temporal scales considered requires much more clarification both in the Methods and Discussion section. I recommend that the authors remove any mention of the words decadal and interdecadal from this paper unless more convincing explanations are provided as to how these calculations are relevant to decadal time scales.

C1583

2. How reasonable is the steady-state assumption? Is it relevant to a four year or a decadal time scale? Alvarez et al. 2002 estimated the temporal scale of this assumption for nitrate to be 10 years. Are your assumptions and relevant time scales the same? Is it possible to estimate the rate of change of tracer concentrations inside the boxes? The authors refer to this in a confusing way on page 4330 in lines 24-26, in the context of flux error estimates. Why are the tracer time derivatives omitted from the equations? Also, I would like to see a comment on how uncertainty related to interannual variability in tracer concentrations might affect the residuals summarized in Figures 2 and 3.

3. It appears to me that the tracer budgets can potentially be just a mean of four quite different snapshots of the physical and biogeochemical conditions in this region. If so, they could hardly represent a decadal state. For example, the optimized flux through the OVIDE section is almost identical to the a priori one which is the sum of the 2002, 2004 and 2006 values (12 vs 11 kmol s⁻¹) yet with half the original uncertainty (16 vs 31 kmol s⁻¹, by the way, why such a big difference in this estimate?) – lines 15-20, page 4336. The flux values from individual years cover a broad range with both negative and positive signs. I would like to see more discussion on the potential impact of including such large inter-annual discrepancies into the total budgets. For example, how, if at all, would the final conclusions (regarding oxygen and organic matter production) change if lower and upper bounds of net oxygen and nutrient transports were assumed instead of the means?

4. The authors do not comment on any potential seasonal bias in the biochemical conditions considered. It is not mentioned in the paper that OVIDE measurements were carried out only between May and June and thus represent typical summer conditions. Were the WOA09 data also summer climatology only or were they annual climatology? Could this seasonal bias partly explain why some of your net biological flux directions differ from the ones in Alvarez et al. 2002? Or do the differences originate from improved transport estimates alone? How sensitive could the partitioning of

C1584

air-sea fluxes between thermal and mixing be to changing seasonal conditions? These questions could be somehow reflected in the discussion part of the paper. Information about sampling period and period of climatology analysis should be included when data sources are described.

Detailed comments:

Abstract: Consider adding one sentence which explains how this region is important to the physical circulation and biogeochemical cycling in the North Atlantic. It is presented in an excellent way in the introduction and could perhaps be stated up front in the abstract as well.

Page 4325, line 28: anthropogenic instead of anthropic

Page 4325, line 3: same as above

Page 4325, line 10: was not were

Page 4325, line 21: how can this be decadal? Change to 4-year or explain where the 10 years come from.

Page 4325, line 23 and elsewhere in the paper: Replace 'in the bibliography' with 'in literature' or 'in previous studies.'

Page 4325, line 26: follows not follow

Page 4326, line 1 and elsewhere in the paper: constrain not constraint whenever used as a verb.

Page 4328, line 7: other not others

Page 4328, line 14: improves not improve

Page 4328, line 22: Why does oxygen solubility need to be analyzed separately to oxygen concentration? Could the authors elaborate on this when they present their model in section 2 and not only in section 4 when presenting equation 10?

C1585

Page 4328, line 26: You need be really careful later on when you talk about optimized nutrient and oxygen fluxes. I am confused as to whether tracer concentrations were optimized as well or not so that they become relevant on a decadal time scale (lines 15-20, page 4336).

Page 4329, equation 6: explain AOU, it not obvious to everybody that this is apparent oxygen utilization

Page 4330, line 17: What are these standard transports? Are they the same as in the optimization tables? Please make a reference to the correct Table or Appendix for literature cited values.

Page 4330, line 21: is not if

Page 4330, line 24-26: in the context of error estimates the authors mention "upper bounds of the uncertainties of the tracer conservation equations due to interdecadal variability (i.e. the amplitude of the tracer time derivative omitted in conservation equations)." First of all, what interdecadal (or even decadal) variability can be inferred from four years of data? Second, if there are any estimates of change of tracer concentration in time, why are they not listed in the paper?

On page 4331, line 15: accounts not account

On page 4331, line 24-25: awkward sentence

On page 4332, line 25: non-negligible not none negligible. What do you mean by vertical mixing here? Do you mean diffusive fluxes as opposed to surface wind-driven mixing?

On page 4333, line 10: Over what range of Redfieldian ratios was this sensitivity conducted? Consider including this information in the text.

On page 4334, line 22: I think these should be shown in a table format. Your total number of tables and figures is still relatively low and such a transport comparison

C1586

would be useful.

On page 4335, line 13: meaningless sentence

On page 4335, line 13-17: In other words not other word. This is only true if you assume a steady-state, correct? What if it is not? The fluxes shown vary greatly from year to year, even in sign. Is this caused by variability in concentrations?

On page 4337, line 5-11: I think that the authors admit that even the sign of net nitrate transport is not significant due to large interannual variability, and that the decadal signal cannot be resolved at this temporal resolution of observations. How does this impact the overall conclusions on biological production? This entire paragraph is actually not comprehensible to me, especially what does it mean that the choice of combining OVIDE data with a climatological dataset "cannot be ruled out"?

On page 4337, line 13: Yes. This is precisely my point. Is there any point in referring to current budget estimates as decadal?

On page 4337, line 14-27: Are there any quantitative estimates of nutrient upwelling fluxes in this region that could be compared to your indirect estimates from horizontal flux budgets? If this region is in fact upwelling dominated, then you are not explicitly taking into account the largest nutrient flux contribution.

On page 4338, line 16: Yes, but it was also based on hydrographic data from a very different time period. Is it possible that part of the difference, or even the sign, originates from interannual or even interseasonal differences in nutrient fields?

On page 4338, line 27-28: this is a repetition from line 10 on page 4333.

On page 4339, line 9: reassuring not re-ensuring

On page 4340, line 16: find not found

On page 4340, line 27: not sure I would call this Redfield ratio state of the art... Stoichiometric is written incorrectly as stoechiometric in many places in this paper.

C1587

On page 4341, line 5: represent not represents

On page 4341, line 7: result not results

On page 4341, line 25-26: what does it mean realistic? What does it mean to have some merit?

Table 1: Why are there eastern face concentrations but not transports, and western face transports but no concentrations?

Figure 2 & 3: What are the individual box and domain residuals in the lower left corners from top to bottom? Label them accordingly or explain further in the figure caption.

There are many more spelling and grammar mistakes that need to be fixed before the paper is accepted for publication.

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