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Biogeosciences
Discussions

Interactive comment on "The sea surface CO_2 fugacity and its relationship with environmental parameters in the subpolar North Atlantic 2005" *by* A. Olsen et al.

A. Olsen et al.

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We appreciate the thoughtful and careful comments by the three reviewers and have prepared a quite substantially revised version in response to these. We hope that these changes improve the readability of the manuscript, make the goals of each section clearer and supplies the information requested by the reviewers. Our general comment and response to the specific issues raised by the reviewers are given in the following.

General Comment

We have done some structural changes in the paper. Primarily introducing a discussion section. The results now solely presents the Hovmöller plots, as this can be considered as the main result. The determination of the processes controlling monthly changes in fCO2 and evaluation of fCO2-parameter relationships have been placed in a discussion section. Each of these sections includes a brief introduction of their respective goals. This has been done as reviewer 1 in particular seems a bit at loss with respect to the objectives, in particular the section on fCO2-parameter relationships. This section has been further modified and simplified by removing two of the shelf regions, the Iceland Shelf and the Faeroe Bank from the analyses. These areas had anyway little data and results were of minor significance.

The section on processes controlling monthly changes in fCO2 has also been modified. Both reviewer 1 and 3 questioned the use of climatological nitrate values in the computations. We agree with their objections and this approach for computing the mixing and biology term has been discared. fCO2 in itself carries a signature from biology and mixing, and given that all other processes have been accounted for this will equal the residual change. Realising this we now compute the mixing and biology term by difference as detailed in the revised manuscript. Alternatively, the approach of Takahashi et al. (2002) for computing biological fCO2 variations could be followed. This was tried and gave similar results as the present approach. Moreover we decided to not follow the suggestion of reviewer 3 to determine this term from chl a data for reasons detailed further down (in the replies to the specific comments). We have also included an analysis of the errors in our budget as requested by revs 1 and 3.

We have included a comparison of our data with those of Takahashi et al. (1993) at the start of the discussion as suggested by rev 3. We did not compare with Taylor et al. (1991) since this is an early modeling analysis and is likely not fully representative of contemporary models. This section also includes the comparison with the Takahashi climatology previously appearing in the summary and further remarks section which thus has been converted into a pure summary section as suggested by rev 3. The discussion on the increase in fCO2 which appeared right at the end has been removed as we agree with rev 1 that it was speculative. This issue requires a separate paper.

Reply to specific comments

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Interactive Comment

Rev 1:

-Indicate regions on map

Done

-Calculations not clear

Clarified

-Error in At, use of climatological nitrate

Effect of error in At has been quantified and is discussed in section 5.1.

Climatological nitrate is not used in present approach.

-Hovmöller diagrams are difficult to read and should be replaced with figures of monthly averages. Effect of changes in ship track.

We do not agree and have chosen to retain these. The Hövmoller diagrams provide a more accurate representation of the seasonal cycle than simply monthly means in different regions. In particular, the latter would not be able to reproduce the spatial and temporal coherency of the changes in fCO2 , chl a, and MLD noted on page 1749 and highlighted by reviewer 3 as a very important conclusion. We urge however Biogeosciences to use one full page on this figure as this will improve readability.

Evaluating the error due to changes in ship track is not easy since there are no data outside the track. However, occasions where we have reason to believe that this has influenced the results has been mentioned in the paper, i.e. data obtained during the Reykjavik port call in February and southern route across EGC in June (mentioned in section 4, revised version). It is the intention that the inclusion of bathymetry in the Hovmöller should reveal these issues.

-Analysis of factors controlling monthly changes, use of climatological nitrate

Approach discontinued, see general comment.

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-fCO2 parameter relationships, objective and main results.

Objective clearly stated at the start of the section. We hope that removing the two minor shelf regions has improved readability.

Figure 8 has been removed.

-Summary and further remarks, validity of relationships.

Statements regarding validity and predictive capability of the relationships have been modified, both here and in the abstract as called upon by the reviewer. We do no longer claim that the rms values reflect their predictive capability, of course that has to be shown using independent data.

-Comparison with Takahashi climatology etc.

Comparison with growth rate estimates of Lèfevre et al (2004), Corbiere et al (2007) and Omar and Olsen (2006) has been removed. Comparison with Takahashi et al. (1993) retained, but moved to opening of discussion. This is an important comparison to make. We have also retained the comparison with the Takahashi et al (2002) climatology, this has important implications for anthropogenic CO2 calculations as stated by rev 3 .

Heike Luger review.

1. Predictive capability of combining SST, cpl a and MLD.

Thus will be adressed by Chierici et al. as stated in the paper.

2. How representative is the 2005 data for other years.

This is an important issue. Large changes in the North Atlantic carbon system on several timescales have been observed by several authors (Corbiere et al., 2004, Olsen et al. 2006, Omar and Olsen, 2006, Lefevre et al., 2004). A brief comparison with these results were included in the submission but deemed speculative by rev. 1. We agree

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with that comment and have removed that part from the paper, realising that this issue requires and deserves a more in depth analysis., This, also goes for an analysis of the year-to-year variability in the Nuka data and so we have not included that here

Rev 3

-Change of title

The analysis of fCO2-parameter relationships have been retained and thus the title.

-Change of abstract

Not changed for same reason as above.

-Page 1739, line 5, Occidentality.

Agree, now we merely refer to the SOCOVV report.

-Page 1740, line 10.

Variability of HYCOM data are not really presented (compared to FOAM) and so it does not seem fair to state that we present these data as well.

-Page 1740, line 19.

We agree with the reviewer that these are all important issues. However, they are not really relevant for the present paper and so have not been specifically mentioned. However, the statement (which now appears in section 5.2) has been modified to read :one of the ultimate goals.

-Page 1741, line 23

Hatton replaced with Rockall which appear in Figure 1.

-Page 1744-1745, section 2.4 (now 3.2)

We have chosen to retain the full validation of both the FOAM and HYCOM data. It is important to show that care has to be excersised when selecting ocean analysis

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data. The validation is moreover valuable in itself, removing the figure and explaining in words our findings did not save much space.

-Page 1744, line 18.

MLD criterion has been stated.

-Page 1746.

The analysis now use the temperature dependence of Takahashi et al. (1993). For the record, there were no discernible differences between the approaches.

-Page 1747, line 5

Salinity has been added in the function as well as a specific reference to the cruises used (this now appears in Sect. 5.1).

-Page 1747, line 6

These errors have been determined, they had no discernible effect on the results (this now appears in Sect. 5.1).

-Page 1747, Determination of vertical mixing and biological component.

We have decided to not follow this approach for two reasons. (1) Estimation of vertical mixing component through MLD changes requires a knowledge of Ct below the mixed layer, such data are not available to us. (2) Estimation of biological pump requires the implementation of a primary production algorithm. There are many algorithms available, with large differences (Campbell et al., 2002) this introduces a significant uncertainty. Moreover, these algorithms estimate net primary production, not net community production which is relevant for the inorganic carbon system. Converting between these on sub annual time scales is non-trivial as knowledge of biomass build up and export is required. Given this we decided that it is better to utilise the fact that fCO2 is influenced by biology and mixing to estimate this component (i.e. by difference).

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-Page 1748, section 3.1

Adding bathymetry on the Hovmöller is relevant. In particular with regard to the comment of rev. 1 on the possible effect of changes in the cruise track on the seasonal cycle. This occurs in February as noted in the paper

Sign has been recalled.

-Page 1749, line 15

Highlighted after synchronicity has been described.

-Page 1750, section 3.2.

Method has been changed and errors are described (see general comment). This now appears in section 5.1.

-Page 1751, end of section 3.2.

A brief comparison with the annual cycle of Takahashi et al. (1993) has been added at the start of the discussion section. We did not compare with Taylor et al. (1991) since this is an early modelling analysis and is likely not fully representative of contemporary models. Neither did we compare with Takahashi et al (2002) as this has too little detail.

-Page 1752, sections 3.3, 3.4, and 3.5

These have now been placed in the second part of the discussion (section 5.2). We still believe that having them as subsections improves readability. Description of fCO2/SST relationship has been made much shorter, Fig. 8 has been removed, and statements regarding the use of such relationships have been made in the summary

-Page 1755

The relationship between MLD and cpl a is not as good as the relationship between fCO2 and either of these. It is best approximated with an exponential decay curve with MLD on the y-axis (i.e. decreasing cpl a with increasing MLD), similar to the fCO2/cpl a relationship. In the IcB this yielded an r2 of 0.31, and in the IrB an r2 of 0.65.

-Page 1755, line 25

True, this has been noted in the paper.

-Page 1756, line 17

Changed.

-Page 1756, section 4.

This is now section 6 and has been converted into a pure summary section. Comparison with previous analyses have been placed at the start of the discussion section.

References (not appearing in the paper) Campbell, J., Antoine, D., Armstrong, R. et al.: Comparison of algorithms for estimating ocean primary production from surface chlorophyll, temperature and irradiance, Glob. Biogeochem. Cyc. 16, 1035, doi: 10.1029/2001GB001444, 2002

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