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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.

Anonymous Referee #3

Received and published: 25 June 2007

Review of the manuscript Biogeoscience Ms. Ref. No.:2006GB002825

Title: "The sea surface CO2 fugacity and its relationship with enironmental parameters in the subpolar North Atlantic 2005."

Authors: Are Olsen et al.,

Decision: This manuscript is acceptable for publication after major revisions

General comment:

This paper presents very interesting new data set, corresponding to an unprecedented coverage in the high latitudes of the north atlantic. In this paper authors described the fCO2 annual cycle at regional scale (following 4-6 regions), attempted to quantify processes (SST, bio/mix,...) that control the fCO2 seasonality, explored relationships of fCO2 with oceanic properties (SST, Chl, MLD) and discussed briefly how the fCO2 distribution and fluxes in 2005 compared with other studies. The paper present fantastic new data, with very high spatial and temporal resolutions. This is the first time that such a complete fCO2 annual cycle is obtained in the region investigated. I think the paper should focus more on the description of these data, the annual cycle and on processes analysis, a story which is not finalized in the present manuscript (residuals are large) and needs a sensitivity analysis (errors in the CO2 budget). As it is described shortly in the "summary", I think this paper should also include a specific section on the air-sea CO2 fluxes as derived from this new data-set and discuss in detail how they compared with previous estimates. The data suggest small disequilibrium during winter, which has important implications to reduce uncertainties of the north atlantic CO2 sink (or source) as well as regarding hypothesis when estimating preformed DIC in regions of water mass formations. The fCO2 relationships presented and discussed in this paper are interesting but could be also presented in another manuscript; we understand that authors investigate these relationships in another paper, including multi-regressions (Chierici et al in preparation) that aims at exploring different relations to extrapolate the observations at basin-scale. This is a suggestion for the authors (not a final recommandation), in order to gain space to develop the seasonal budget in more details here (including sensitivity analysis and discussion). However, they also can maintain the fCO2/relation in this manuscript if the link with the seasonal budget analysis is more clearly presented.

Specific comment/questions:

Page 1737: regarding my general comment the title should be changed (depending if fCO2/properties relationships are or not presented in this manuscript). What about using the running title: "Sea surface CO2 fugacity in the subpolar North Atlantic 2005."

Page 1738: regarding my general comment the abstract should be changed (depending if fCO2/properties relationships are or not presented in this manuscript). . Page 1739: line 5: this portion is very "occidental" (reference to US/CO2 and EU CARBOOCEAN only); don't forget the important work conducted by Japan for instance, or Australian teams. On an international side, you can refer to IOCCP (or better the recent SOLAS-IMBER, SOCOVV meeting, a short report should be soon published in EOS). This would recall in an elegant way all international efforts.

Page 1740, line 10; authors indicate they present data from FOAM. At that stage they don't specify they will also use HyCOM. This is not a problem as I suggest HyCOM results to be delete in the paper (see comment below)

Page 1740, line 19: I agree that the "ultimate goal of the global fCO2 observations effort is to constrain regional ocean carbon uptake on seasonal to interannual timescales" and authors identify that the use of other data (e.g. satellite product) represent a crucial step to extrapolate local semi-continuous in-situ observations. I also agree with this. However, I think that when trying to "constrain regional ocean carbon uptake on seasonal to interannual timescales" we also have to call for ocean carbon models. The observational effort is very important for the modelling community. Seasonal data presented and analyzed in this paper (figure 5 and 6 are very informative in this regard) will be very helpful to develop and/or correct current biogeochemical parametrizations (I think models are not so good in the north atlantic, especially at high latitudes). I can also add that such data are also very important to constraint atmospheric transport models that experienced difficulties in separating ocean and terrestrial carbon sink in the northern hemisphere.

Page 1741, line 23: I don't know where is the Hatton Trough (not in Figure 1)

Pages 1744-45, section 2.4. Authors have checked FOAM and HyCOM against in-situ SST and salinity. They conclude that FOAM data are preferred to obtain high resolution salinity and MLD fields. I think this section could be much shorter to explain the final choice; Based on SST,SSS comparisons with FOAM, I would trust authors selection.

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Figure 4 could be delete as well as reference to HyCOM (not used).

Page 1744: line 18: the MLD is an important parameter in this analysis; it is used both in the seasonal budget calculations as well as for exploring fCO2 relationships. These results are certainly very sensitive to the values of collocated MLD. Therefore, the MLD definition (criteria) used in FOAM should be recall.

Page 1746, line 19. The effect of temperature on fCO2 variations are calculated from DIC/AT. Why not using the temperature dependence of Takahashi et al (1993), the same you used to process the data.

Page 1747, line 5: equation for At as a function of salinity needs ....salinity somewhere. As it is refered as personal communication, you should also specify the cruise/data from which is derived this new AT/SSS relation.

Page 1747, line 6. Authors deduced DIC from fCO2 and AT, which is a nice way to create data not observed during the cruises; this could introduce some errors, both regarding the AT/SSS relations as well as depending the constants used and nutrients. The errors in this calculation could be easily determined and a sensitivity analysis (e.g. montecarlo) should be applied to obtained an associate error on the monthly CO2 budget. This may explain part of the large residuals in the seasonal budget (fig 6).

Page 1747, as the authors calculate DIC for each month of year 2005, and they have a nice field of collocated MLD, I don't understand why they are not calculating the effect of vertical mixing from DIC, instead of using nitrates climatology. The same for biological process; it could be possible to derive the biological effect from collocated Chl concentrations of year 2005 (e.g. Taylor et al, 1991; Louanchi et al, 1996 and many others).

Page 1748, Section 3.1 This is the heart of the paper. Figure 5 and 6. In figure 5 I'm not convinced that adding bathymetry on the Hovmoller diagram is relevant. In figure 5b, recall the sign of DeltafCO2 (here positive is a ocean sink, i.e. add CO2 in the

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ocean following Equation 9).

Page 1749, line 15: authors indicate that the "seasonal evolution of fCO2 appears synchronous with that of both MLD and chla". This is a very important conclusion based on Fig 5 and calls for exploring fCO2 relationship with these parameters rather than with SST. This could be highlighted at the end of this section to create the link.

Page 1750, Section 3.2. author recognize that residuals are quite large compared to the observed fCO2 variations. They attribute this to the use of nutrient climatology. This is a possibility, but this is not quantified. I think this section really needs a sensitivity analysis in order to show how each process is or not well constraint (nutrients, MLD, gas-exchange, TA/SSS relations, C/N ratio, wind field, ...) and how residuals change regarding the uncertainlies attached to these contraints.

Page 1751, end of section 3.2. There is no discussion of the seasonal budget. How does this compared with previous analysis (Takahashi et al 1993, 2002; Taylor et al 1991). As it appears that the "bio and mix" seasonality seems to follow the fCO2 variations, this could be recall at the end of this section 3.2 to introduce section 3.3, 3.4 and 3.5 and why author are exploring various fCO2/property relationships.

Page 1752: Sections 3.3,3.4 and 3.5 could be merged in one section (4). Plotting the fCO2 data versus SST, Chla and MLD is a nice way to describe how fCO2 is or not related to the environnemental conditions. However, I think the description of fCO2/SST is to long. I don't think anybody will use the fCO2/SST relations and I suggest to remove Table1 and figure 8. Authors should state that fCO2/SST relations are very poor in all regions and such regressions could not be used to extrapolate sparse fCO2 data to reconstruct seasonal fCO2 fields. This is not new, and it has been shown several times that at high latitudes (north and south) SST is a poor parameter to reproduce fCO2. On the other hand, authors should focuss on fCO2/Chl and MLD relations that appear to reproduce quite nicely the seasonality (Fig 10).These realtions are not unexpected, but here, with an important data set, they are clearly observed. This is encouraging

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for futur work that aim at extrapolating fCO2 at regional and basin scales (and we understand authors are performing such analysis, Chierici et al in prep). As for section 3.1, the discussion is not developped. For example, some groups are using fCO2/SST relations to establish global fCO2 maps and calculate fluxes. Don't you think you have shown here that at least in the north atlantlic such simple extrapolation is not correct and leads to erroneous monthly fCO2 fields ?

Page 1755, authors plot independently fCO2 with MLD and Chla. What about the MLD/Chla relation (just curious) ?

Page 1755, line 25. Authors indicate that MLD regressions are not good on the shelves. Would it be possible that the MLD from FOAM are not very good and could create suspicious relation with observed fCO2 ?

Page 1756, line 17. Sverdrup (1953).

Page 1756, section 4 presented as a "Summary and further remarks". In some parts, this section appears like a discussion more than a "summary". I have the feeling authors wanted to tell more when finishing the manuscript and decide to include the discussion in the summary. I suggest the comparison with previous analysis (fluxes) be presented at the end of section 3.1 (results). Also, comparison of fCO2 relationship could be part of the discussion attached to section 3.2,3.2,3.3 (or new section 4). In the summary you simply recall the main results of your study and open new challenge as mentionned (large scale extrapolation and secular trends).

References cited in the review (and not in the paper):

Louanchi et al., 1996. Modelling the monthly sea surface fCO2 fields in the Indian Ocean. Marine Chemistry, 55, 265-279

Taylor et al., 1991. A modelling investigation of the role of the phytoplankton in the balance of carbon at the seas surface of the North Atlantic. Global Biogeochemical Cycles, 5, 2, 151-171..

Interactive comment on Biogeosciences Discuss., 4, 1737, 2007.