

Interactive
Comment

***Interactive comment on* “The sea surface CO₂ fugacity and its relationship with environmental parameters in the subpolar North Atlantic 2005” by A. Olsen et al.**

Anonymous Referee #1

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This manuscript presents a new dataset of fCO₂ collected between Denmark and Greenland in 2005 using an autonomous system installed on board a merchant ship. Chlorophyll a concentrations from SeaWIFS, mixed layer depth from a model are correlated with the measured fCO₂ to explain the causes of the fCO₂ variability in this region in 2005. The approach used raises several comments. The computation of monthly changes is questionable as the parameters used for determining the effect of biology and mixing, of air-sea exchange and of salinity come from climatological nitrate values, modelled MLD and salinity. How well can they reflect in situ changes in 2005 along a ship track? The objective of using fCO₂ - single parameter relationships is not very clear. Moreover, the robustness of these relationships is assumed but never demon-

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strated. It is speculative to consider that the predictive capability is good on the basis of regressions made in 2005. These relationships might help to illustrate the processes governing the fCO₂ distribution in 2005 but their predictive capability is not proven.

Specific comments

- Determination of the effect of processes controlling monthly changes of fCO₂. The sampling area is divided into four regions: the East Greenland Current (EGC), the Irminger Basin (IrB), the Iceland Basin (IcB), and the North Sea (NS). It would be clearer to indicate the regions on the map.
- The calculation is not clear. Averages values are assumed to be representative of the 15th of each month and the change in fCO₂ is calculated between the values of the first day of the month. This needs to be clarified.
- How does the Lee et al. (2006) relationship compare with the other two At-S relationships? What is the error on At based on these relationships? What is the error made on Ct? Climatological nitrate data cannot be used to explain variations of fCO₂ measured in situ in 2005 along a ship track.

Results.

- Variations of fCO₂ and related parameters in 2005. The Hovmöller diagrams are very difficult to read. It would be better to plot the seasonal cycle using monthly averages in the different regions. The reference of Sverdrup (1953) to comment the phytoplankton bloom in fig. 5f should be removed. From figure 2, the ship does not repeat the same track. How is the seasonal variability affected by the ship track?
- Analysis of factors controlling monthly changes. It is difficult to really determine the changes as there are some questions about the use of climatological nitrate and some of the results might be simply an artefact of the approach as noted by the authors at the beginning and at the end of this section.
- Relationships fCO₂ temperature, fCO₂ chlorophyll a, fCO₂ mixed layer depth. This

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section should highlight the main results instead of presenting a catalogue of regressions. It is difficult to see the points made here, the purpose of all the regressions and of figure 8. Why are two more regions (IcB and FB) introduced? It seems that the objective of these regressions is to find a fair predictor for fCO₂ or an extrapolation parameter.

- Summary and further remarks These relationships have been established from data in 2005 but the validity of them is not addressed at all. It is very speculative to conclude that in contrast to other studies (Lüger et al., 2004) we have been able to identify basin-wide relationships between fCO₂ and chl_a valid on nearly annual time scales. The predictive capability of chl_a alone in the subpolar Atlantic is better than that of relationships in the Pacific Ocean: the robustness of the relationships has not been demonstrated in this manuscript.

- Comparison of the Nuka 2005 data with the more recent Takahashi et al. 2002. The comparison reveals a difference of 20 microatm, the climatology being undersaturated. One of the possible explanations is the large increase in fCO₂. Further down we read that Takahashi et al. observed a slight positive disequilibrium at 63N, 20W and comparing with the 2005 data, the situation appears unchanged. This in contrast to Corbiere et al (2007) and Lefevre et al. (2004) who both observed oceanic growth rates larger than the atmospheric. This illustrates that using a different region, a different time, we get a different picture. The comparison is not valid as these studies are performed in different regions and on different time scales.

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