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Biogeosciences Discussions

Interactive comment on "Air-sea CO₂ fluxes in the Atlantic as measured during the FICARAM cruises" *by* X. A. Padin et al.

Anonymous Referee #2

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Review of the manuscript submitted to Biogeosciences

Title: Air-Sea CO2 fluxes in the Atlantic as measured during the FICARAM cruises Author(s): X. A. Padin et al. MS No.: bg-2009-113

Decision: This manuscript is not acceptable for publication in its present form

General comment:

This paper presents new observations of sea surface pCO2 (or fCO2) in the Atlantic Ocean for the period 2000-2008 from cruises regularly conducted along long transects from Europe to Antarctica during spring and autumn. In the context of international pCO2 data synthesis and global carbon budget estimates, this study represents very important complementary data and should be published. However, the manuscript is

somehow presented like a cruise report; discussions and interpretations are lacking, especially regarding previous works in the same regions. It is not easy to know what new results/informations have been obtained from this impresive 8 years of data.

Other comments:

1) Abstract: line 8: Authors indicate: -The obtained spatial and temporal distributions of FCO2 follow the generally expected patterns and annual trends.- However, the paper does not compare the new FCO2 calculations with previous studies and words in the abstract suggest that FCO2 distribution did not really changed over a decade. If this is correct for all regions investigated here, from north to south, this is an important result as this would mean that the ocean carbon sink is not varying, at least in this region. If this result is robust, authors should highligth this finding in the abstract. However, although the title of the MS calls for FCO2 results, the abstract only recalls one number, the influence of the Amazon River.

2) Introduction: authors should recall previous studies that investigate seasonal and interannual variability of FCO2 in the Atlantic ocean. Introduction should include recent observational analysis (e.g. Takahashi et al., 2009; Schuster et al, 2009 and reference herein) and inverse atmospheric methods (e.g. Transcomm). In the atlantic ocean, boardered by continents (large terrestrial source/sink variability) and where few long-term atmospheric CO2 monitoring station exist, oceanic FCO2 observations, as presented in this paper, would greatly help to constraint the global carbon budget. Authors should also indicate what are the current FCO2 errors in the Atlantic Ocean. Is it 0.1 PgC/yr or 1 PgC/yr ? They should also specify what is not known concerning the processes that control FCO2 variability. This would more clearly justify why such long-term oceanic project like FICARAM is important.

3) Introduction page 5591 line 22: authors indicate that FICARAM data are available in several global databases. They should specify where the data are available. For example, using the Mercury search, FICARAM data are not available at CDIAC (see

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also the link http://cdiac.ornl.gov/oceans/VOS_Program/hesperides.html).

4) Methods: pages 5593 and 5594: during FICARAM, atmospheric CO2 has been regularly recorded (using air pump, DEKABON tube...). However, those data are not used for FCO2 calculations. Instead authors prefered to use NOAA monitoring observations because during some FICARAM cruises atmospheric data are not available (why ?). It would have been interesting to compare FICARAM atmospheric xCO2 data (when available) with those derived from NOAA atmospheric stations. Are the atmospheric data obtained onboard present or not significant deviations from continental and islands records. Are atmospheric xCO2 data selected at monitoring stations have been filtered following air-mass trajectories to discard continental signal ? If atmospheric data recorded onboard are not used, why authors describe these data in the methods section ?

5) Methods: page 5595: authors convert pCO2 to fCO2, and then used Takahashi et al 1993 equation (but expressed for pCO2, not fCO2) to correct for temperature (SST-Eq). This has certainly no impact on results but this is not recommanded and should be revised.

6) Methods: page 5595. FCO2 is calculated following equation (3), where “a” is a unit conversion factor. Could you please indicate what is the value used for “a”

7) 2.4 Biogeochemical oceanographic provinces: authors separate different regions based on SST/SSS distribution and some known features such as upwelling, equatorial current, etc... I don’t see where biogeochemistry is refered when you select the provinces ? Change the title of section 2.4.

8) 3 Results and discussion: part 3 of the MS presents data and FCO2 calculation, there is no discussion. Change the title of section 3.

9) Page 5601, line 19: need a reference when quoting upwelling system along Mauritanian coast.

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10) Page 5601, line 22-23: Authors compare their results for autumn with annual flux from Takahashi et al. They should compare the results for the same seasons and using the same units.

11) Page 5601, line 26-27: Authors compare their seasonal results for the NEC (16N-8N) with annual flux from Takahashi et al over 14N-14S. They should compare the results for the same seasons and regions. By the way, are the FICARAM data included in Takahashi's pCO2 synthesis. If yes, this is strange to obtain different results. If not, I strongly suggest authors to send their data in global databases (as it has been indicated by authors in the introduction, see comment 3 above).

12) Page 5603: line20-24: authors refered to several studies concerning the Livingston Island but those studies did not investigate this region.

13) Page 5605: authors refere to decreasing NAO index during the FICARAM experiment; however, in 2001-2008 the NAO index moved from negative to positive values.

14) Page 5606: authors indicate that increase of rainfall and riverine inputs from Amazon explain the observed decrease of SSS. Please add a reference.

15) 3.4: could you explain why you are using an empirical algorithm to detect the forcing of fCO2 variability.

16) Page 5607: could you justify the use of Lat/Long, SST and SSS second and third polynomials ? What are the physical/biogeochemical justifications (meaning) of such selection in the diagnostic model.

17) Page 5607: I understand you are first normalizing fCO2 at constant SST to establish Equation (4). How this helped to investigate thermodynamic processes ?

18) Page 5607: before establishing Eq 4, authors adjust fCO2 data to a reference year 2005, i.e. they assume ocean CO2 follows atmospheric trend but is it correct ? Recent studies suggest that oceanic fCO2 growth rate is different depending the region and period. Is it realistic to apply the same correction in the Atlantic basin, from North to

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South ? Why not including the period (year) in Equation 4, that may help to separate natural versus anthropogenic CO2 signals.

19) Page 5608, line 14: authors indicate that in the southern hemisphere fCO2 variability was poorly resolved with the model (their Eq 4) likely due to a larger coastal influence. However, on page 5598, authors specified that they have excluded data in coastal waters. Should you test another data selection for the southern hemisphere ?

20) Figures: figures 2 and 3 are very small; difficult to see all details.

21) There are many references in the text that are not in the reference list:

Cooper et al 1998; Richardson and Reverdin, 1987; Richardson and McKee, 1984; Klinck and Nowlin, 1986; Poisson et al., 1994; Pakhomov and Froneman, 1999; etc....

Interactive comment on Biogeosciences Discuss., 6, 5589, 2009.