

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

Received and published: 27 July 2009

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

C1282

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 impressive 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 highlight 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, bordered 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

C1283

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 preferred 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 recommended 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 referred 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.

C1284

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: line 20-24: authors referred to several studies concerning the Livingston Island but those studies did not investigate this region.

13) Page 5605: authors refer 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

C1285

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.