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Discussions

Interactive comment on "Direct observations of diel biological CO_2 fixation in the oceans" *by* H. Thomas et al.

Anonymous Referee #2

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1. General comments

While most past studies were focused on pCO2 variations over seasonal or longer time scales, this study explores the high frequency processes in controlling the carbon cycle in the surface waters of the Scotian Shelf region over a complete annual cycle. However, the authors did not spell out how such information may help improve our overall grasp of the human perturbed marine carbon cycle or achieve better understanding of the physiology of algal photosynthesis. This is an interesting study, but the authors need to better define their goals and explicitly expound implications of their discovery. This manuscript further investigates the CARIOCA data set, which has been partially presented by Shadwick et al. (2010 and 2011). It is OK to skip some detailed descriptions of methods and materials, but, since this is a separate manuscript, it is better to

include brief descriptions of methods employed or, at least, refer to the other papers or original references when alluding to data processing.

2. Specific comments

Abstract:

Line 3 on p. 2154: "through processes such as heat fluxes" Heat fluxes are not a process. Solar heating is.

Lines 6-7 on p. 2154: "limited to a time span of several days to months, or exceptionally, for longer periods." It is not clear what "exceptionally" means in this context.

Lines 7-8 on p. 2154: ", however corresponding investigations of the oceanic CO2 system are lacking." "however" is not the appropriate conjunction.

Line 11 on p. 2154: "and its effects on annual budgets." I do not find any discussion on "its effects on annual budgets." If this refers to Fig. 4, it needs considerable elaboration.

Introduction:

Line 15 on p. 2155: Replace the comma with a semicolon in ". . ...2011), however corresponding investigations of. . ."

Lines 2-11 on p. 2156: "Controls of the seasonal to interannual variability of the surface CO2 system . . ..The study, presented here, sheds light on the role of high frequency processes in controlling the carbon cycle in the surface waters of the Scotian Shelf region over a complete annual cycle." It is highly desirable to spell out what insights the authors expect to gain from the high frequency processes and how those may help us better understand the marine carbon cycle.

Material and Methods:

Lines 1-3 on p. 2157: "In an attempt to resolve the contribution of phytoplankton . . ., we derived chlorophyll a concentration . . . Nahorniak et al. (2001)." It is necessary to

qualify “chlorophyll a concentration.” Is it the chlorophyll a concentration at sea surface or certain specific depth or the average over certain depth range?

Lines 11-14 on p. 2157: “Chlmod values were compared with discrete, ... (RMSE) values of 0.89 and 0.83 mg m-3 (N = 8) respectively, were obtained.” It would be better to present the validation plot, because the statistical parameters do not necessarily reflect the goodness of fit or the reliability of the modelled chlorophyll a values. One may argue that the range of chlorophyll a variation shown in Fig. 3 is considerably smaller than the RMSE (0.83 mg m-3), and, therefore, not meaningful.

Results and Discussion:

Lines 5-6 on p. 2158: “We corrected the observed pCO2 data (pCO2,obs) to a daily mean temperature to give pCO2,temp.” Calculation of pCO2,temp is not trivial, but there is no mentioning of the method any where in the ms.

Lines 7-10 on p. 2158: “The difference between pCO2,obs and pCO2,temp yielded pCO2 data that are governed by processes other than temperature within a 24-h period. Since we did not detect processes other than SST variability acting on the 24 h period, the remaining pCO2 10 variability can be ascribed to biological activity (pCO2,bio).” To what degree does air-sea exchange of CO2 affect pCO2? Will it interfere with the signal? If not, why not?

Lines 13-14 on p. 2158: “With the onset of the spring bloom, at approximately day 90, the diel amplitude drastically increased (Fig. 3a).” It looks to me the spring bloom started after Day 95 instead of on Day 90, if it did occur as suggested by the authors. Is there any other evidence, such as Chl-a data, indicating the occurrence of the spring bloom?

Lines 23-25 on p. 2158: “More importantly, a phase shift was detectable between pCO2,obs and pCO2,bio, with the latter occuring approximately 3 h earlier than the pCO2,obs (Fig. 3d).” It is not clear how Fig. 3d is constructed. Some description

is necessary. Why the phase shift occurs only in this period but not in the other two deserves some explanation. “occuring” should be “occurring.”.

Lines 7-8 on p. 2159: “NCP, indicated by a negative gradient in the pCO2 anomaly (Fig. 3d), dominates the system until dusk.” “Gradient” is often referred to in a spatial sense rather than temporal sense. “Negative slope” may be a better term.

Lines 11-13 on p. 2159: “The corresponding respiration rate, assumed to be constant throughout the day, is estimated to be 0.05 μmolC(l h)$-$1; the rates of NCP and GPP are 0.26 μmolC(l h)$-$1 and 0.31 μmolC(l h)$-$1, respectively, both lasting approximately 10 h per day.” It will be more convincing, if the estimated values are reasonable as compared to direct observations in the same area and the same season.

Lines 2-5 on p. 2160: “we used the model of Nahorniak et al. (2001) to derive estimates of chlorophyll-a concentration (Chlmod; mg m$-$3) every two hours during daylight from SeaHorse profiler measurements of multispectral downwelling irradiance, Ed(λ).” The statement is redundant; the info has been given in Materials and Methods..

Line 10 on p. 2160: “the change in gradient of pCO2,bio from positive to negative” “Gradient” should be slope.

Lines 11-13 on p. 2160: “In other words, our data suggest that a threshold Chlmod must first be attained before the system achieves net CO2 drawdown.” “Chlmod” should be chlorophyll concentration. Since net CO2 drawdown still persisted after the Chl value dropped below the apparent “threshold value,” it is better to modify the statement by inserting “during the growth phase” after “attained.’

Lines 6-7 on p. 2161: “We have obtained the seasonal dynamics of NCP integrating the hourly pCO2,bio values (Fig. 4.). The maximum value of NCP is 3.4 molCm$-$2, or 271 μmolC L$-$1.” What is the time period pertaining to the NCP values mentioned here, “mol Cm$-$2 d-1, and “μmolC L$-$1 d-1”? Again some comparison with observations would make the estimates more convincing.

Conclusions:

Lines 16-17 on p. 2161: “In summary, we observed a statistically significant diurnal periodicity of the CO2 system only during the period, when the water is warming.” It is worth some discussion why significant diurnal periodicity of the CO2 system occurs only during the warming period. Is mixing too strong during other periods, when stratification is weaker, so that the diel signal of pCO2 is obscured.

Figures:

Fig. 1b. It should be specified in the legend that the blue crosses indicate the MLD.

Fig. 2c “High coherence at the 24 h period occurs only during the period when the water is warming.” This plot takes considerable space but little is said about it. The figure caption is rather confusing. To me high coherence at the 24 hour period occurred from March to mid July, not just Day 160-200.

Fig. 4. “Annual cycle of biological DIC uptake.” It is not clear how this plot was made or the meaning of it. If it is about DIC uptake, it should show units of rate. The caption mentions “mixed layer inventory,” which is confusing and needs explanation.

Interactive comment on Biogeosciences Discuss., 9, 2153, 2012.