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Biogeosciences Discussions

Interactive comment on "Spatio-temporal variability of the CO₂ system on the Scotian Shelf" *by* E. H. Shadwick et al.

Anonymous Referee #1

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I. General comments

This manuscript presents DIC, TA, and pCO2 data along three cross-shelf transects in the Scotian shelf area and one transect crossing the Cabot Strait in April and September 2007. On the basis of the newly observed data set and previously published POC export production and sedimentation rate, the authors construct a seasonal carbon budget between spring and summer for the study area. They further suggest that the unbalanced term in the constructed budget can be explained by advection, and thereby argue that "continental shelf pump" mechanism may operate in the Scotian shelf area during the study period. Though this manuscript was generally well organized and its subject meets the general interest of Biogeosciences, the related calculations (both in NCP and carbon budget) were not presented in a detailed and precise fashion, so

that the calculated results are not very convincible. I suggest that the authors should state clearly what are the assumptions and justify thoroughly the uncertainties in these calculations before this manuscript can be accepted for publication at Biogeosciences. Please see the followings for detailed comments.

II. Major comments

(a) I think that the circulation pattern would largely control the spatial variability of carbonate system parameters and play an important role on the operation of "continental shelf pump". I thus suggest the authors to better present the physical oceanography setting in the study area, and describe how the circulation field agrees with the current distribution of the carbonate system parameters. An illustration about the general circulation pattern may be needed.

(b) In section 4.1 , the authors reported three different kinds of net community production in Table 1, namely NCP, NCPas and NCPas, ve, which was estimated on the basis of different assumptions:

(1)The underlying assumption for the calculation of NCP is that biological net community production is the only factor controlling the observed nDIC variation between April and September, i.e.

DeltanDICobs = DeltanDICncp

(2) The underlying assumption for the calculation of NCPas is that biological net community production and air-sea CO2 exchange are the two factors controlling the observed nDIC variation between April and September, i.e.

DeltanDICobs =DeltanDICncp + DeltanDICas

(3) The underlying assumption for the calculation of NCPas, ve is that biological net community production, air-sea CO2 exchange and vertical entrainment are the three factors controlling the observed nDIC variation between April and September, i.e.

DeltanDICobs = DeltanDICncp + DeltanDICas+ DeltanDICve.

However, as pointed out by the authors in section 4.2, horizontal advection can also affect seasonal variation of nDIC. Therefore, a complete consideration for the observed nDIC variation between April and September should include the effect of advection, i.e.

DeltanDICobs = DeltanDICncp + DeltanDICas+ DeltanDICve+ DeltanDICha

DeltanDICobs: the observed nDIC difference between April and September

DeltanDICncp: nDIC change caused by net community production

DeltanDICas: nDIC change caused by air-sea CO2 exchange

DeltanDICve: nDIC change caused by vertical entrainment

DeltanDICha: nDIC change caused by horizontal advection

From the above equations, we can see clearly that the reported NCP in Table 1 should represent the net effect of biological net community production, air-sea CO2 exchange, vertical entrainment and horizontal advection on the seasonal change of nDIC; the reported NCPas in Table 1 should represent the net effect of biological net community production, vertical entrainment and horizontal advection on the seasonal change of nDIC; the reported NCPas, ve in Table 1 should represent the net effect of biological net community production and horizontal advection on the seasonal change of nDIC. In other words, the reported NCP, NCPas and NCPas, ve in Table 1 all have already included the effect of DIC advection inside. As a result, the advection term in the seasonal carbon budget presented in Section 4.2 cannot include the advection in DIC form any more, i.e. only advection in POC and DOC forms would be allowed. The authors must clearly explain this point in the related discussion.

(c) I think that the authors have to state clearly what are the assumptions and uncertainties in the calculation of NCP in Section 4.1 and in the construction of seasonal carbon budget in Section 4.2, i.e. an error analysis should be provided. Unless this

is done, it would be difficult for them and for any reader to assess to what extent the proposed interpretation (the potential for a continental shelf pump mechanism) can be supported by the observations.

III. Minor comments

1. P. 12017 line 21: please explain how do you justify the overall uncertainty of underway pCO2 measurement to be less than 1 ïĄ■atm?

2. P. 12018 lines 14-18: From Fig. 2b, I cannot see there is a difference in the slopes of the relationship between TA and salinity in April and September. Is it statistically examined? Additionally, even if the seasonal slope is significantly different, it cannot be explained by the seasonal variation of water delivery from the St. Lawrence estuary system. It more likely reflects the seasonal variation in TA end-member from the St. Lawrence estuary system.

3. P. 12022 line 15: Fig. 5 > Fig. 5g

4. P. 12024 lines 5-7: “In the subsurface there is no significant increase in DIC in September, relative to April, as seen along the Halifax and Cabot Strait sections as a result of organic matter respiration.” This statement is logically wrong, since organic matter respiration should result in significant increase in DIC in the subsurface water.

5. P. 12025 lines 9-12: “Significant increases in surface pH are observed in April relative to September; this increase in pH is coincident with the DIC drawdown due to photosynthesis and also results in an increase in the aragonite saturation state.” This statement conflicts with the observed results. As shown in Fig. 9, surface pH in April is significant higher than that in September. The authors suggest that the high pH may result from biological production, which would correspondingly lead to a decrease in DIC and an increase in aragonite saturation state. Therefore, one would expect that low DIC and high aragonite saturation state should appear in April. However, the results show that DIC is higher (Fig. 3c & g and Fig. 7c & g) and aragonite saturation

state is lower (Fig. 10a, b, d & e) in April than that in September. The authors should explain this discrepancy. One possibility for this is seasonal temperature variation. Because pH is a non-conservative parameter, if the authors want to discuss the effect of biological production on pH variation, it would be better to report pH values at a constant temperature.

6. Pp. 12026-12030 Section 4.1: I suggest moving the calculation method of net community production to the Methods section so that the discussion can be focused.

7. P12045 Fig. 1: please add the general circulation pattern to Fig. 1.

8. P12045 Fig. 8: Showing the real pCO2 data along the cruise tracks would be enough. A typo in the y-axis (CO2 should be replaced by pCO2).

9. P12048 Fig. 11: Why nDIC in April is negatively correlated with salinity both in surface and subsurface waters and what does this relationship mean? Some discussion on this negative correlation between nDIC and salinity may be needed.

Interactive comment on Biogeosciences Discuss., 8, 12013, 2011.