

## ***Interactive comment on “Spatio-temporal variability of the CO<sub>2</sub> system on the Scotian Shelf” by E. H. Shadwick et al.***

**Anonymous Referee #3**

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The paper reports DIC, TA, pCO<sub>2</sub>, temperature and salinity results obtained for the Scotian Shelf in April and September 2007. Additionally, some pCO<sub>2</sub> results used by the authors for the assessment come from 2010. The measurements were performed along the three cross-shelf transects and one transect crossing the Cabot Strait. Based on the results obtained the authors conclude on the net community production and create the seasonal carbon budget for the investigated region. The scope of the studies presented by the authors in this manuscript is of crucial importance for our understanding of the carbon cycle on the global shelves and thus place the manuscript within the scope of Biogeosciences interests. However, the manuscript should be first improved in several aspects mentioned below and thus requires further revision.

General comments 1) The authors should present more in details the evaluation

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method of NCP and the uncertainties accompanying to the results both of NCP and carbon budget. They inform only the reader that NCP was calculated as the difference between nDIC concentrations observed in September and in April. Are there any available NCP assessments made for the investigated region that base on the other quantification methods e.g. nutrients consumption approach? The April nDIC results presented on fig. 11 differ a lot from one another even for the comparable salinity values. The authors calculated linear best-fit relationships between the April nDIC results and salinity. However, did the author check what is the uncertainty of such linear approximation, and how much such uncertainty influence the final results of NCP? The nDIC results above the solid line on fig. 11b for the subsurface layer are explained by the authors as 'respiration', but what is the meaning of the September nDIC observations below the solid line? In the text there is only poor information about the seasonality of the biological activity in this region, although one of the goal of this study was to assess NCP. The reader doesn't know what was the status of biological activity during sampling period in September. Chlorophyll and particulate organic carbon concentrations might give some impression on this. It might be confusing to use different terms for NCP (NCP, NCPas, NCPas,ve). Net community production is just net community production and all the intermediate results obtained before the final result are not the NCP. In this way, when NCP is estimated from the shifts in carbonate system it should include all the variables (at least these of major importance for the result) that influence the CO<sub>2</sub> (or any other parameter describing it) concentrations in seawater. It is unclear how the nDIC changes caused by the CO<sub>2</sub> exchange with the atmosphere was distinguished from the nDIC changes caused by primary production and mineralization.

2) The differences between the distribution of carbonate system variables (DIC, TA) in April and September are described in section 3.2. In the same place the authors give already some explanations about the causes of these shifts in the carbonate system. In one place, they suggest e.g. that DIC concentrations changes are caused by shifts in salinity, in other place that they are caused by CO<sub>2</sub> outgassing or biological activity

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etc. However, these explanations are only the speculations and rough hypothesis since no statistical confirmation accompanies them.

3) Some more attention should be paid to the hydrographical regime in the studied area since in a large measure it influences the carbonate system.

Minor comments: 1) Page 12015, lines 3-7. Giving the example of the Baltic Sea as a basin acting as a sink for atmospheric CO<sub>2</sub> is unjustified. Baltic Sea is not only Baltic Proper where studies by Thomas and Schneider (1999) were performed. It is composed as well of adjacent gulfs influenced largely by the carbon input from land. According to the recent literature there is no straightforward conclusion about the role of the Baltic as sink or source of CO<sub>2</sub>.

2) Page 12020, line 15. The authors refer to fig. 3 and there is no pCO<sub>2</sub> data on that fig.

3) Page 12021, line 7. The waters warm at surface not only in September.

4) Page 12021, lines 14-17. From fig. 4 g and c does not appear that in September there is a 'substantial increase of DIC' in the upper water column relative to the April observations. We can observe there an increase of DIC in subsurface zone and a decrease of DIC in the surface layer in the investigated period.

5) Page 12022, lines 7-9. How the development of a shallow, warm, surface layer can push the shelf water down? It needs explanation.

6) Page 12023, lines 4-6. From fig. 6 it is difficult to conclude where (on the left or right hand side) is Nova Scotia. Nonetheless, from the text we get to know that Nova Scotia side of profile is colder and less saline. However from the fig. 6 it appears that less saline water on the left hand side of the profile is warmer in September and not colder.

7) Page 12024, lines 5-7. How the authors define the subsurface layer? From fig. 7 we can see that there is a significant increase of DIC in September at the depths of 100-200m.

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8) Figures from 3 to 7 are too small (low resolution?). It is easy to enlarge them in the electronic version, but problem arises with a paper version of MS.

9) Figure 12 needs some legend with scale for NCP results. I suggest to bright up the background of bathymetry distribution.

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