

We wish to thank all three anonymous referees for their constructive comments. Almost all of the comments and suggestions have been incorporated into the revised manuscript, which we believe is significantly improved as a result. We have provided a response to each of the comments below in [blue text](#).

Referee #1

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 convincing. 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.

[We have added a subsection \(section 3.3 in the revised version\) to the Methods section, which clearly describes the method used to estimate NCP from carbon and nutrient data. This section includes a description of the assumptions used and an estimate of the uncertainty associated with the computation of NCP. The carbon budget has been removed from the revised text.](#)

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.

[We have added a section \(section 2 in the revised text\) called ‘Oceanographic Setting’ in which the general circulation in the region is described. A figure \(Fig. 1 in the revised text\) has also been added that indicates the dominant, long-term, mean circulation in the region.](#)

(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. $\Delta nDIC_{obs} = \Delta nDIC_{ncp}$

(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. $\Delta nDIC_{obs} = \Delta nDIC_{ncp} + \Delta nDIC_{cas}$

(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. $\Delta nDIC_{obs} = \Delta nDIC_{ncp} + \Delta nDIC_{cas} + \Delta nDIC_{cve}$.

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.

$\Delta nDIC_{obs} = \Delta nDIC_{ncp} + \Delta nDIC_{cas} + \Delta nDIC_{cve} + \Delta nDIC_{cha}$

$\Delta nDIC_{obs}$: the observed nDIC difference between April and September

DeltanDIC_{ncp}: nDIC change caused by net community production
DeltanDIC_{cas}: nDIC change caused by air-sea CO₂ exchange
DeltanDIC_{ve}: nDIC change caused by vertical entrainment
DeltanDIC_{cha}: 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 NCP_{as} 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 NCP_{as, 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, NCP_{as} and NCP_{as, 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.

Based on these comments, for which we are grateful, we have adopted a different method of estimating NCP that relies on the difference between surface concentrations and (assumed) winter concentrations, determined from seasonal profiles of NO₃. This method avoids the problems identified by the reviewer stated above, and yields a change in DIC that includes air-sea flux (addressed in the methods section in the estimate of uncertainty associated with NCP, and also in the discussion of results) and ‘mixing’. We have also estimated NCP from the difference between salinity-normalised ‘winter’ and surface concentrations, which yields an estimate of NCP that includes the air-sea flux but not the ‘mixing’ term. These methods and associated uncertainties have been described in the revised text (Section 3.3 and 5.1).

(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.

As described above, a new section has been included in the revised text (Section 3.3) describing the methods used to compute NCP and the associated uncertainty. The seasonal budget has been removed from the revised version.

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 uatm?

The uncertainty on the underway pCO₂ measurement is based on the standard deviation of the measurement on a sample of air (or calibration gas) with constant, known pCO₂. In this case the standard deviation was less than 1 uatm. An explanation has been added in the methods section of the revised text.

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.

The discussion of changes in the slope of the relationship between TA and salinity has been removed from the revised text.

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

This particular reference to Fig. 5 has been removed, but we thank you for the correction.

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.

The reviewer is correct that organic matter respiration in autumn should result in an increase in subsurface DIC. The statement was intended to indicate that comparison of the in-situ profiles along this section did not clearly indicate such an increase, not that an increase did not occur. This statement has been removed from the revised text and the description of seasonal changes in subsurface refocused on xy-plots, rather than the contour diagrams (which have been relocated to the Appendix).

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 significantly 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.

The revised text includes profiles of pH at 25C, and not in-situ temperature as suggested. Indeed, the (biological) increases in pH between April and September are associated with increases in Omega and decreases in DIC as stated by the reviewer. This has been clarified in the text (Section 4 in the revised version).

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.

The description of the method used to compute NCP has been moved to Section 3.3 as suggested.

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

A new Figure 1 has been included which indicates geographic locations referred to in the text as well as a general circulation.

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).

The axis label has been corrected, however we have elected to show the pCO₂ in the same way.

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.

This figure has been removed from the revised text. However, the negative correlation of nDIC with salinity is due to the normalization to salinity 35, which is higher than the vast majority of observations in the region. Thus the high salinity values are changed relatively little under the normalization, but the low-salinity waters are augmented to indicate what DIC they would have at salinity 35, causing a large increase in DIC at low salinity and almost no change at high salinity, and generating the negative relationship.

Response to Referee #2

Overall quality of the discussion paper

1. A paper prepared to a low standard with respect to the reader being able to make their own judgement on the data.
2. A paper which is not sufficiently quantitative in its approach.
3. A paper which does not consider the hydrographic setting in sufficient depth.

The revised manuscript includes a detailed description of the quantitative method used to estimate NCP, the assumptions used, and the uncertainty associated with the computation. A new section (Section 2) has been added to the revised version describing the physical oceanographic setting. A new figure (Fig. 1 in the revised version) has been included which shows the dominant, long-term, mean circulation in the region.

Specific comments

1. Does the paper address relevant scientific questions within the scope of BG? Yes. The magnitude of the impact of shelf sea systems on the global carbon cycle is still an open question because of the lack of measurements over most of the globe.

2. Does the paper present novel concepts, ideas, tools, or data? The data is novel but poorly described. The data set is partial because it does not include the appropriate nutrient and chlorophyll or oxygen data. The tools used to derive budget are poorly described. The concepts and ideas included in the conclusions provide nothing new relative to assertions made in the introduction.

The revised text includes nutrient data, both in terms of a description of seasonal changes in water column nitrate, and also as a tool to estimate the depth of 'winter' nutrient, and carbon concentrations, in order to estimate NCP. The carbon budget has been removed from the revised version which now focuses on the spatial distribution of the carbonate system properties and NCP.

3. Are substantial conclusions reached?

No new conclusions are reached.

To the best of our knowledge this is the first such assessment of the carbonate system in the Scotian Shelf, which we consider a new contribution. Furthermore the comparison of NCP based on nutrients, with that based on changes in DIC indicates that there is significant biological production occurring after the exhaustion of nutrients throughout the region. The conclusion has been re-written in order to highlight this aspect of the work.

4. Are the scientific methods and assumptions valid and clearly outlined? No. See below.

We have added as description of the method used to compute NCP (section 3.3 in the revised version) to the Methods section, which includes assumptions made and uncertainty associated with the computation in order to address this weakness.

5. Are the results sufficient to support the interpretations and conclusions? No. The paper is weak on clearly describing the annual hydrographic cycle in the waters being studied.

We have added a section (section 2 in the revised text) called 'Oceanographic Setting' in which the general circulation in the region is described and a new figure (Fig. 1 in the revised text) has also been added that indicates the dominant, long-term, mean circulation in the region.

6. Is the description of experiments and calculations sufficiently complete and precise to

allow their reproduction by fellow scientists (traceability of results)? No. The way the data is presented in the form of contour diagrams makes the data difficult to assimilate quantitatively. The data should be presented as simple x/y depth versus property plots. Throughout, the data is described as being “high” or “low” and rarely are any numbers provided for the reader to judge what is meant by “high” or “low”.

The contour plots have been relocated to the Appendix and seasonal (xy) profiles of DIC, pH, Omega, NO₃, and salinity-normalised DIC have presented as suggested. The description of the data has been re-written to clarify the changes quantitatively as suggested.

8. Does the title clearly reflect the contents of the paper? No. It should mention the assessment of export.

Since the assessment of export has been removed the paper the title has not been changed.

9. Does the abstract provide a concise and complete summary? No. It contains no quantitative information.

The abstract has been revised accordingly and now includes the magnitude of seasonal changes in DIC as well as regional mean results of the NCP computations.

10. Is the overall presentation well structured and clear? No.

(1) A section of say 300 words giving information on the hydrographic setting and seasonal changes is required. Questions such as:- How well homogenised is the water at the end of winter on and off shelf? What is the direction of the flow of the St Lawrence plume? How much does the flow change seasonally in terms of volume?

A section describing the hydrographic setting has been added as suggested (Section 2 in the revised text). It includes a description of seasonal changes in temperature and salinity as well as a description of the dominant currents and the seasonal change in transport via this current. A description of the delivery of freshwater via the Gulf of St. Lawrence, and the annual amount has also been included.

(2) Thought should be given as to how the differences between the hydrographic sections could be more clearly presented. This would allow section 3.2 to be substantially shortened and make it more useful and informative.

The hydrographic sections themselves have been relocated to the Appendix and the description of seasonal changes has been re-focused and shortened accordingly.

(3) A problem this paper probably suffers from is that the first April cruise should have actually sailed and sampled before the spring bloom. Page 12019 line 3 states “In April ... representative of winter conditions” but the bloom was well underway by the time they had sailed. I would have expected that they would have tried to examine their profiles to identify winter mixed water containing pre-bloom concentrations of DIC. Was this not possible?

The statement identified in in this comment refers to a statement in the text that the April observations are representative of winter conditions **with respect to the physical system**, not the biological system, which is true when one looks at temperature and salinity in April. However, we have used a new method in the revised version that relies on the identification of ‘winter’ (pre-bloom) NO₃ and DIC concentrations, as suggested, in the estimate of biological draw down in both April and September.

(4) I find the section on the estimation of net community production confusing. At the top of page 12028 mention is made of nitrate data for the first time (but it is “not shown”). Given the wealth of historical nitrate data I would have thought must exist for this region, could not the early season production be estimated from difference between their observed April values and winter nitrates from a climatically comparable year? This would solve the problem they admit to around line 25 on page 12028, that substantial production had occurred before their April cruise.

The NO₃ data has been included in the revised version, and used, as suggested, to define the depth at which the pre-bloom DIC (and NO₃) concentrations are found.

(5) Section 4.2 on the budget seems a bit of jumble to me. What is needed is simple step by step description how each of the numbers appearing in Figure 14 is arrived at.

This section has been removed from the revised text. However a step-by-step description of the methods used to compute NPC has been included.

(6) Other points

What is the carbonate concentration in the St Lawrence? How does the carbonate content change with flow? This important information is not presented. (Cai et al (2010) J. Geophys. Res. 115: C08014, doi:10.1029/2009JC005482. Raymond and Cole (2003) Science 301: 88–91; give some indication it would be worth a mention).

From our own data – included in this manuscript, and in Shadwick et al, 2011 in Marine Chemistry – we developed a linear relationship between DIC (and TA) and salinity. The y-intercept values of these relationships ($R^2=0.92$ for TA and $R^2=0.74$ for DIC, $n = 784$, $p<0.001$) indicate that the carbonate concentrations in the St. Lawrence are DIC = 546 $\mu\text{mol/kg}$ and TA = 805 $\mu\text{mol/kg}$, which are in line with values obtained for the Gulf of Maine, consistent with observations in Cai et al. 2010 (now cited in the revised text). We do not have a good sense of how these values change with flow given the relatively limited (seasonal) observations.

They followed the Friis normalisation method but where did they get the appropriate end member from? Having made case that normalising DIC is of value then changes in DIC would be easier to see in the diagrams if nDIC were plotted.

The Friis normalization equation has been added to the text, and the end member used is the one described above, also included in the revised version. The profiles of nDIC (as xy plots) have been included in the revised text (Fig. 9).

11. Is the language fluent and precise? The language is fluent but the paper is poorly structured and long winded. Substantial editing and restructuring are required.

The paper has been re-structured and shortened substantially as suggested.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? In sufficient such information is given. For example the Friis adjustment equation and factor and source of the fresh water factor should be given. Presentation of the equations may help explain the working used to derive Figure 13.

The Friis normalization has now been included along with the freshwater end-member DIC and TA values (Section 3.3 in the revised text).

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Figure 12 is unnecessary given Table 1. It could however be merged with Figure 1 and flow lines indicating the major currents could be added.

Figure 12 has been removed. A new figure indicating the circulation has been added (Fig. 1 in the revised text).

Thought needs to be given to Figures 3 to 7 and 9. I think the information would be clearer as X/Y depth property plots. They should be grouped by property rather than the section. Plotting the normalised values for DIC and TA may be more appropriate than the simple concentrations. The contour diagrams in this submission could be retained in the supplementary data.

The contour plots have been relocated in the Appendix and xy depth-property plots are now presented as suggested. Profiles of salinity-normalised DIC (and NO₃) have also been included.

14. Are the number and quality of references appropriate? No. Better information is needed on the background hydrography and the seasonal cycle in productivity in the region based on nutrient and other studies.

Additional information regarding the hydrography and seasonal cycle in production, as well as the relevant citations, has been added to the introductory sections.

15. Is the amount and quality of supplementary material appropriate? The data set is sufficiently small it could be included in the supplementary information. This should also include the nutrient data. It is possible that examination of the nutrient ratios may also help distinguish the boundary between river influenced and ocean waters.

The carbonate data set will be submitted to the CDIAC data base and not included as a supplement; the nutrient data set is collected and maintained by the Bedford Institute of Oceanography and is subject to the internal rules for data distribution, and will thus not be included with this manuscript, but would likely be made available upon individual request.

The continuous records from the ship's CTD temperature and salinity profiles may help distinguish the position and depth of the winter mixed layer and hence pre-bloom concentrations of DIC.

We have identified the depth of pre-bloom DIC concentrations based on historical nutrient data as suggested and have modified the method used to compute NCP, and the description of this method in the text, accordingly.

Response to Referee #3

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

In the revised version NCP is computed on the basis of difference between surface NO₃ (and DIC) concentrations and pre-bloom, 'winter' values, allowing a comparison of the carbon and nutrient based approaches to be made. A description of the method used to compute NCP has been included in the Methods section (section 3.3 in the revised version), and in this description the assumptions made and associated uncertainties are stated.

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?

This figure has been removed from the revised text as a different approach has been adopted to compute NCP (see above).

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.

Information about the seasonal cycle of biological production has been added to the introduction (Section 2 in the revised text). We have also expanded the discussion of the NCP results to include information regarding (satellite) observations of chlorophyll-a and POC in the region.

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 CO₂ (or any other parameter describing it) concentrations in seawater. It is unclear how the nDIC changes caused by the CO₂ exchange with the atmosphere was distinguished from the nDIC changes caused by primary production and mineralization.

The description of the method used to compute NCP has been included in the methods section and all steps and uncertainties have been described. The description of results has been clarified to and includes discussion of changes due to air-sea flux and mixing (Section 3.3 and 5.1 in the revised text).

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 CO₂ outgassing or biological activity etc. However, these explanations are only the speculations and rough hypothesis since no statistical confirmation accompanies them.

The description about drivers of seasonal changes in DIC has been clarified.

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.

A new section has been added to the revised text (section 2) that describes the hydrographic setting, and figure has been included showing the dominant circulation in the region (Fig. 1 in the revised text).

Minor comments: 1) Page 12015, lines 3-7. Giving the example of the Baltic Sea as a basin acting as a sink for atmospheric CO₂ 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 CO₂.

The statement regarding the Baltic Sea has been removed.

2) Page 12020, line 15. The authors refer to fig. 3 and there is no pCO₂ data on that fig.

This has been corrected.

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

This has been corrected.

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.

This has been corrected/clarified in the text.

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

This has been corrected/clarified in the text.

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.

This has been corrected/clarified in the text. Additionally, in the revised version, Figure 1 includes the geographic locations mentioned in the text and the stations have all been labeled in Figure 2 and in corresponding profile plots.

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.

The depth range of subsurface layer has been defined in the text when referred to.

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.

This has been addressed. The contour plots have been replaced by x-y plots and are now found in the Appendix (in higher resolution).

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

This figure has been removed from the revised version.