The study quantified the air-water CO2 exchange rates in rivers, estuaries, and continental shelves of the US Northeast region using existing data and various interpolation and extrapolation techniques. These CO2 flux estimates are very useful to construct the regional C budget. The seasonality and spatial variability in CO2 fluxes in the region, especially in rivers, are particularly interesting. In general, the paper is well written, but there are a few concerns/comments that I would like to share with the authors:

We thank the reviewer for his positive and constructive comments. We are glad that our study was regarded as useful and answered the reviewer's comments, point by point, to the best of our abilities.

Riverine pCO2 calculation – The pCO2 values in the paper were calculated from pH and alkalinity (alk) measurements. It is known that non-carbonate alkalinity (non-calk) can introduce large calculation uncertainty in pH-alk calculation of pCO2, most likely overestimate of pCO2. The study in Maine rivers (Hunt et al.) show the calculation can be 10 – >60% over estimate. I think the uncertainty may be even higher than this, as that particular study only focused on the main stems near river mouths, and upper streams of the rivers may be even more organic rich and their water may contain more non-calk. I won't be surprised in some places calculated pCO2 may be >100% off the real value. This issue was not dealt with in the paper, not even mentioned. I think the strategy here may be to find some existing data, where three of the 4 CO2 parameters are available to give an estimate of calculation errors or better yet try to minimize the overestimate in flux calculation.

Hunt et al. 2013 report pCO_2 values for two Maine rivers, the Kennebec River and Androscoggin River. These values were calculated from measured DIC and pH. They provide the range and mean values of the pCO_2 and pH as well as the mean values of Alkalinity and DOC for both rivers (see table below). By comparing DIC and Alkalinity, they found that, on average, 40% of the alkalinity is non-carbonate alkalinity. A calculation of pCO_2 solely based on pH and alkalinity would thus overestimate the actual pCO_2 by the same amount. Three of the sampling stations used in our study are also located in these two rivers (see table below). However, although we calculated the pCO_2 based on pH and alkalinity, our pCO_2 values are on average lower than those reported by Hunt et al. (2013).

Location	pCO_2	pH	Titrable Alkalinity	DOC	Study
Kennebec and Androscoggin rivers	3064 (1231-6703)	6.6 (4.9-7.0)	284	412	Hunt et al. 2013
Kennebec R. At Bingham (45.05°N, -69.89°E)	2409 (1208-4475)[a]	6.6 (6.4-6.8)	187	638	This study
Kennebec R. At North Sidney (44.47°N, -69.69°E)	901 (636-1127)[a]	7.2 (6.7-7.8)	306	519	This study
Androscoggin R. at Bruinswick, ME (43.92°N, -69.97°E)	1703 (1243-5085)[a]	6.9 (6.5-7.4)	272	683	This study
Lower Hudson R.	1014				Cole and Caraco 2011
Hudson R. at Green Island (42.75°N, -73.69°E)	1400 (761-2802)[a]	7.3	997	566	This study

Note that the range of values reported in the Table is based on median values per month. A range based on single values would be significantly larger and would reflect single (sometimes erroneous) extreme values. It is worth pointing out that a shift in pH by 0.1 unit leads to a difference in calculated pCO_2 of about 20%.

In contrast to the Maine Rivers, the pCO_2 values calculated here for the lower Hudson River are on average substantially higher than the one reported by Cole and Caraco (2011). Their value is based on a 8 year time-series of weekly direct pCO_2 measurements, and can thus be considered as a highly representative measurement devoid of any artifacts introduced by the alkalinity definition. A Comparison with our values indicates that we might overestimate the pCO_2 by ca. 40%.

We added a few lines in the discussion section about the possible bias introduced by the calculation of pCO_2 from pH and alkalinity.

"The higher outgassing rates in the North are a consequence of higher ΔCO_2 values since average k values are similar in both sections. In rivers with Q_{ann}<10 $m^3 s^{-1}$, the ΔCO_2 is about twice as high in the North than in the South from April to August (Table 2). The calculation of pCO_2 from alkalinity and pH presumes however that all alkalinity originates from carbonate ions and thus tends to overestimate pCO_2 because non-carbonate contributions to alkalinity, in particular organic acids, are ignored in this approach. The rivers in Maine and New Brunswick, which drain most of the Northern part of COSCAT 827, are characterized by relatively low mineralized, low pH waters rich in organic matter. In these rivers, the overestimation in pCO_2 calculated from the carbonate alkalinity only was reported to be in the range 13%-66% (Hunt et al., 2011). Considering that rivers in the Southern Part of COSCAT827 have lower DOC concentrations and higher DIC concentration, the higher FCO_2 rates per surface water area reported in the Northern part could party be due to an overestimation of their pCO_2 values. However, a direct comparison of average pCO_2's does not confirm this hypothesis. For the two Maine rivers (Kennebec and Androscoggin Rivers), Hunt et al. (2014) report an average pCO_2 calculated from pH and DIC of 3064 µatm. In our data set, three sampling stations are also located in these rivers and present lower median pCO_2 values of 2409, 901 and 1703 µatm for Kennebec River at Bingham and North Sidney and for Androscoggin River at Brunswick, respectively. A probable reason for the discrepancy could be that we report median values per month while Hunt et al. (2014) report arithmetic means, which are typically higher."

Abstract: '…estuarine surface area are identified as important…factors…'. It is a bit confusing. Surface area is one factor of many in estuaries that can affect CO2 flux. As the authors mentioned, decomposition of terrestrial C in estuaries is one very important factor, at least as important as surface area.

Our use of the word factor when referring to estuarine surface area might be misleading. Indeed, the surface area does not represent a biogeochemical process. As pointed out by the reviewer, it is the decomposition of terrestrial C in estuaries and subsequent outgassing that affects the dynamics of carbon of the continental shelf. What we meant was that the filtering capacity of estuaries in the North section is much less than in the South section because of the difference in number and size of estuaries between both regions. We re-wrote the sentence as follows:

“Significant differences in flux intensity and their seasonal response to climate variations are observed between the North and South sections of the study area, both in rivers and coastal waters. Ice cover, snow melt and carbon removal efficiency through the estuarine filter are identified as important control factors of the observed spatio-temporal variability in CO_2 exchange along the LOAC.”

I am not an expert of the language, but is ‘North East’ should be one word? This applies to the whole paper.

The reviewer is correct and the text has been modified accordingly (4 occurrences including one in the title).

P11988, L16, COSCAT 827 first appeared in the paper. Should give the full name and give some description on what is it. Many people are not familiar with the term. There are other acronyms in the paper that authors did not first describe and give the full names. May want to give a thorough check and add descriptions if necessary.

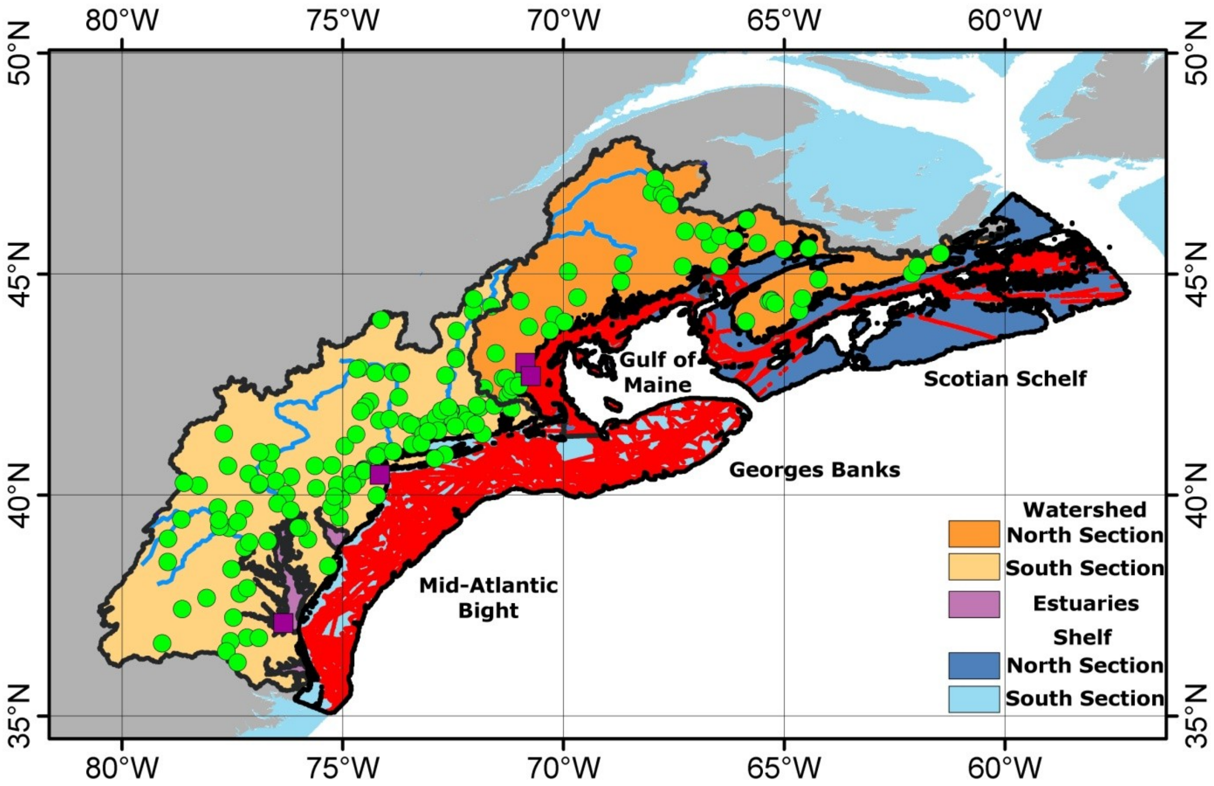
The first paragraph of the methods introduces COSCAT 827 as the study area and provided a brief description of what the COSCAT segmentation is. We re-wrote the first few sentences of this section to make sure the acronym COSCAT was spelled out after its first occurrence and expended the description of the COSCAT segmentation:

“Our study area is located along the Atlantic coast of the Northern US and Southern Canada and extends from the Albemarie Sound in the South to the Eastern tip of Nova Scotia in the North. It corresponds to COSCAT 827 (for Coastal Segmentation and related CATchments) in the global coastal segmentation defined for continental land masses by Meybeck et al. (2006) and extrapolated to continental shelf waters by Laruelle et al. (2013). COSCATs are homogenous geographical units that divide the global coastline into homogeneous segments according to lithological, morphological, climatic and hydrological properties.”

Additionally, we carefully went through the manuscript and made sure to explicit the other acronyms on their first occurrence (i.e. SOCAT and GLORICH).

Figure 1. The boundary of the North-South region is not clearly labeled and showed, and no legend for it.

Following the reviewer’s comment, we decided to use slightly different colors to characterize the watersheds and continental shelf waters of the North and South sections. We updated the legend accordingly. For the sake of readability, we also increased the fonts on that particular figure.



P11989, last paragraph, It would be useful and more clear to list the equations of Aeff or have a table to show how it is defined. The equations of Raymond et al. 2012, 2013 may also be useful to show here. I found it is a bit difficult to follow the text.

The surface water area *A* was calculated from stream length *L* and stream width *B* for each 15'' cell of the hydrological routing scheme Hydrosheds. *L* was derived from the stream network (i.e. from the size of the considered 15'' cell and the flow direction, i.e. whether the stream crosses the cell in horizontal, vertical or diagonal direction). The stream width *B* was calculated from the average annual discharge Q_{ann} using the equations of Raymond et al. (2012, 2013) (Eqs. 2, 3). The effective stream surface area A_{eff} for each month was calculated from *A* after setting all values of *A* to 0 in the 15'' cells for which the estimated water temperature for the corresponding month was below zero (see also response to following comment and Eq. 4)

Equation 1

$A\ [m^2] = L\ [m] * B\ [m]$

Equation 2

$\ln(B\ [m]) = 2.56 + 0.423 \cdot \ln(Q_{ann}\ [m^3 s^{-1}])$ **(after Raymond et al., 2012)**

Equation 3

$\ln(B\ [m]) = 1.86 + 0.51 \cdot \ln(Q_{ann}\ [m^3 s^{-1}])$ (after Raymond et al., 2013)

with
L stream length
B stream width
Q_{ann} annual average discharge

As suggested by the reviewer, we added equations 2 and 3 in the ms. and referenced them in the text.

Why did the authors choose -4.8C as the ice cover temp? Is there a logic/reason here, reference?

The choice of an air temperature of -4.8° as ice cover temperature is based on an empirical equation between average monthly water temperature T_{water} and average monthly air temperature T_{air} (Eq 4). This equation was derived using a linear regression on $498 \cdot 10^3$ pairs of observed monthly T_{water} and T_{air} values at the water sampling location (GloRiCh data base [Hartmann et al., 2014])

Equation 4

$T_{water}\ [°C] = 3.941 \pm 0.007 + 0.818 \pm 0.0004 \cdot T_{air}\ [°C]$ ($R^2=0.88$)

According to this empirical equation, T_{water} is below 0°C when T_{air} is below -4.8°C and this is the reason why we chose this threshold value. It is also close to the value of -4°C used by Raymond et al., 2013. Eq. 4 and the derived ice cover temperature were taken from the ms. by Lauerwald et al., (Global Biogeochemical Cycles, under revision) and this paper is now referenced in the revised ms.

P11990, 2nd paragraph, I think it would be very useful to list how k is calculated in equations. The k constant is a key parameter for CO2 flux calculation. I don't see what k-parameterization (reference) was used here. A more careful discussion is needed here. Also in this paragraph, it mentioned that only annual averages for V and k600 could be calculated, then how can monthly k be calculated?

The standardized gas exchange velocity k_{600} was estimated from stream flow velocity v and stream channel slope S_{chan} using the equation from Raymond et al., 2012 (Eq. 5). The stream flow velocity was estimated from the mean annual discharge Q_{ann}. Stream flow velocity of a river usually increases with discharge. However, the empirical equation from Raymond et al. (2012, 2013) (Eqs. 6, 7) are not applicable to estimate temporal changes in stream flow velocity v from discharge. They are only valid for an annual average Q_{ann}, just like the empirical equations for stream width B and stream depth. That means that the equations can be used to estimate the different average flowing velocities at different sites, but not the temporal variability of flowing velocities at one site.
The actual gas exchange velocity k is also dependent on water temperature. The standardized gas exchange velocity k_{600} is valid for CO_2 at a water temperature of 20°C (which corresponds to a Schmidt number SC of 600). We calculated for each 15s cell and month the water temperature based on equation 4 (see comment

above), and used this value to correct k_{600} (Eqs. 8, 9). This is the reason why the gas exchange velocity is different for each month of the year.

Equation 5

k_{600} [m d^{-1}] = v [m s^{-1}] · S_{chan} [1] · 2841 + 2.02 (after Raymond et al., 2012)

Equation 6

ln(v [m s^{-1}]) = –1.64 + 0.285 · ln(Q_{ann}[m^{3}s^{-1}]) (after Raymond et al., 2012)

Equation 7

ln(v [m s^{-1}]) = –1.06 + 0.12 · ln(Q_{ann} [m^{3}s^{-1}]) (after Raymond et al., 2013)

Equation 8

k [m d^{-1}] = k_{600} [m d^{-1}] · $\left(\frac{SC}{600}\right)^{-0.5}$ (see Raymond et al., 2012)

Equation 9

$SC = 1911 - 118.11 \cdot T_{water} + 3.453 \cdot T_{water}^2 - 0.0413 \cdot T_{water}^3$ [Wanninkhof, 1992]

with
k_{600} Standardized gas exchange velocity for CO_2 at 20°C water temperature
k Gas exchange velocity
Q_{ann} annual average discharge
v stream flow velocity
S_{chan} channel slope
SC Schmidt number
T_{water} Water temperature

We added eqs. 5-7 in the ms. and referenced them in the text. For the calculation of actual k values for each month we referred to the publication of Raymond et al. (2012) which describes the procedure in more detail and also includes equations 8 and 9.

P11990, last line, what is the inverse distance weighted interpolation? More description would be useful.

This method is an interpolation technique creating a regular grid of values based on a set of scattered points with observed values. To predict a value for each unobserved point x in the grid, the N nearest points x_i with observed values are used (Eq. 10). In our interpolation, we used the 4 nearest points. The predicted

value is derived as the weighted average of those observed values. The weight applied is the inverse of the squared distance between each point x_i with measured values and point x for which the missing value is predicted (Eq. 11). Accordingly, observed values from closer points have a higher weight in the prediction than the more distant points. We used the software ArcGIS (ESRI™) and its "Spatial Analyst" extension to perform this interpolation.

Equation 10

$$\hat{u}(x) = \frac{\sum_{i=1}^{N} w_i(x) \cdot u_i}{\sum_{i=1}^{N} w_i(x)}$$

Equation 11

$$w_i(x) = \frac{1}{d(x, x_i)^2}$$

With
$\hat{u}(x)$ estimate value at point x
u_i observed value at point x_i
$w_i(x)$ weight applied to value of neighboring point x_i
$d(x,x_i)$ distance between point x and point x_i

The above method is now briefly described in the revised ms:

"These median values per sampling location and month were then used to calculate maps of $\Delta[CO_2]$ at a 15s resolution. To this end, an inverse distance weighted interpolation was applied. This method allows predicting a value for each grid cell from observed values at the four closest sampling locations, using the inverse of the squared distance between the position on the grid and each sampling locations as weighting factors."

P11991, L9, '…relative to the terrestrial surface area per…'. Not sure how this has been done and what meaning it has. Please clarify.

This statement means that we report the flux relative to the terrestrial surface area (i.e. in g C m^{-2} yr^{-1}). The terrestrial surface area is comprised of 'dry' land and inland water areas. However, for the maps we proceeded slightly differently. We combined fluxes of water-air CO_2 exchange on inland waters and in the shelf sea. For that, we calculated for each 0.25° cell first the total FCO_2, and then divided it by the total area of the cell, as long as it falls within our study area. At the coastline, the FCO_2 is a combination of riverine and shelf FCO_2. We will correct this passage and explain it in a more comprehensible way.

"The results were then aggregated to a 0.25° resolution and three-month period and reported as area specific values referring to the total surface area of the grid cell. At the outer boundaries, only the proportions of the cell covered by our study area are taken into account. "

P11991, L11, Is there any justification why the equations of Raymond et al. 2012 and 2013 can be used for the estimate of the uncertainty?

Here, we essentially follow Raymond et al. (2013). In this study, it was found that the equations of Raymond et al. (2012) tend to overestimate the width of rivers, particularly the small ones. This resulted in an overestimation of river surface area and, thus, *FCO_2*. On the other hand, the equations for k and stream width of Raymond et al. (2013) tend to underestimate stream width, river surface areas and *FCO_2*. Thus, these authors used these two estimates of *k* and *A_{eff}* to calculate the confidence interval. A similar approach can be used here since we used the same predictive equations.

This has been clarified in the revised ms:

"This method is consistent with the approach of Raymond et al. (2013), which used two distinct sets of equations for k and A to estimate the uncertainty in these parameters and their combined effect on the estimated FCO_2. "

P11991, 2nd paragraph, Again, what k-parameterization was used for estuarine CO2 flux calculations? It would be very useful to list key equations and have some discussion of k errors.

Our calculation of the CO_2 flux for estuaries only consists in making an average of local estimates of FCO_2 calculated by other authors in the region. We thus did not use any parameterization of the CO_2 flux for estuaries ourselves. It is true, however, that the formulation of the CO_2 exchange at the air-water interface is not the same in each of the 5 studies we refer to and this information is now provided in the manuscript. We believe however that providing formulations of k that we did not use ourselves might be confusing. The following clarification has been introduced in the methods section:

"It should be noted that the methods used to estimates the CO_2 emission rates differ from one study to the other (i.e. different relationships relating wind speed to the gas transfer coefficient). However, in the absence of consistent and substantial estuarine pCO_2 database for the region, we believe that our method is the only one which allows deriving a regional data driven estimate for the CO_2 outgassing from estuaries. Similar approaches have been used in the past to produce global estuarine CO_2 budgets (Borges et al., 2005; Laruelle et al., 2010; Cai, 2011; Chen et al., 2013; Laruelle et al., 2013)."

P11992, 2nd paragraph, it there any extrapolation that has been done to cover non-sampled grid cells? Please clarify. 'Monthly FCO2 for the North and South… water surface area and weighted rate for each cell,…' It is not very clear how this has been done, may want to list some equations and have more description.

Indeed, the average monthly gas exchange rate calculated for each section based on the cells containing data is then extrapolated to the cells devoid of data in order to obtain the entire flux for each region. The sentence pointed out by the reviewer has been replaced by a longer text explaining the procedure in more details.

“Average monthly CO_2 exchange rates were calculated for the North and South sections using the water surface area and weighted rate for each cell and those averages were then extrapolated to the entire surface area As of the corresponding section to produce FCO_2. In effect, this corresponds to applying the average exchange rate of the section to the cells devoid of data.”

We also introduced a reference to a recently published manuscript in Global Biogeochemical Cycles (Laruelle et al., 2014), which uses the same procedure for further information.
“A more detailed description of the methodology applied to continental shelf waters at the global scale is available in Laruelle et al. (2014).”

Results and Discussion, I like the estuarine filter discussion. But other mechanical drivers of CO2 fluxes along this continuum are not very well discussed. It would be useful to strengthen the discussion by examining the fluxes calculated from this study.

We agree with the reviewer that other processes than the estuarine filter alone are known to influence the shelf CO_2 dynamics and could potentially lead to a difference between the North and South section. The discussion regarding other potential factors has been improved (see last answer of this file). Following the reviewer’s comment, we added a few sentences in the text regarding one aspect of the budgets we constructed for the estuaries that was not referred to or discussed in the text: the ratio of inorganic to organic carbon and its difference between the North and South section are now further discussed.

“The ratio of organic to inorganic carbon in the river loads is about 1 in the North and 1.4 in the South. This difference stems mainly from a combination of different lithogenic characteristics in both sections and the comparatively higher occurrence of organic soils in the North (Hunt et al., 2013; Hossler and Bauer, 2013).”

P11993, 1st paragraph, it would be good to separate this paragraph to two, one for river, one for shelf.

Done

P11994, 1st line, ‘…in DOC and CO2, combined to increasing…respiration…’ CO2 can’t increase respiration.

We rephrased the sentence to clarify that the cause of the increase in respiration rates is warmer water temperature.
“The steep increase and FCO_2 maximum in spring can be related to the flushing of water from the thawing top-soils, which is rich in DOC and CO_2. Additionally, the temperature rise also induces an increase in respiration rates within the water streams (Jones and Mulholland, 1998; Striegl et al., 2007).”

P11994, 1st paragraph, ‘a close mirror behavior’, I think it is not a very close mirror here.

We agree with the reviewer. We thus rephrased the sentence to keep the idea of synchronized opposite trends without having to refer to the idea of a ‘close mirror behavior’.

“Rivers and the continental shelf in the North section present synchronized opposite behaviors from winter through spring. In the shelf, a mild carbon uptake takes place in January and February (-0.04 ± 0.25 TgC month^{-1}) followed by a maximum uptake rate in April (-0.50 ± 0.20 TgC month^{-1}).”

P11994, L25, ‘…one order of magnitude larger…’ I don’t see it is one order of magnitude larger here. Which number vs. which number?

The order of magnitude of difference refers the difference between the surface area of estuaries (14.5 10^3km^2) and rivers (1.2 10^3km^2). We decided to add the value of the surface area of rivers between brackets and a reference to table 1 to clarify that the comparison refers to the surface areas:
“Estuaries emit 0.73 ± 0.45 TgC yr-1, because of their comparatively large surface area (14.5 10^3 km^2), about one order of magnitude larger than that of rivers (1.2 10^3 km^2, table 1).”

P11995, 2nd paragraph. Is there an explanation why rivers in the North have a higher areal rate of CO2 degassing than in the South in general?

The main reason is that the average pCO2 is way higher in the Northern part. Particularly for rivers with a Q_{ann}<10 m^{3}s^{-1}, from April to August, the pCO_2 is about 2 to 3 times that in the South. A reason for the high pCO_2 might be the higher abundance of organic rich wetland soils and thus the higher DOC concentrations in rivers in the Northern part (see also Hunt et al. 2011).

Also in this paragraph, it would be clearer to make two paragraphs, one for rivers and one for shelf.

Done

P11995, 2nd paragraph. It says that the shallowest depth interval is a CO2 source for the shelf, but Table 1 shows the South shelf S1 is a sink? Please check and change the discussion accordingly. It is a bit surprise that S1 is a sink? Do DeGrandpre and Signorini papers show nearshore CO2 sink in the MAB?

Indeed, the shallowest interval is only a CO_2 source in the North. In the South, it is a very moderate CO_2 sink and overall, the larger surface area of the shallow shelf (<20m) in the South leads to a net sink for the shallow shelf of the entire region. We modified the text in order to be more specific as to which section of the study area is a CO_2 source. The study of DeGrandpré et al. (2002) reports an increase in the intensity of the CO_2 sink from the inner to the mid-shelf (followed by a decrease again in the outer shelf, which is outside of the limits of our study area) and the maps produced by Signorini et al. (2013) reveal recurring high pCO_2 values near the coast. Additionally, another study by Chavez et al. (2007) also reports an increase of the intensity of the CO_2 sink away from the shore but using a relatively coarse resolution (1 degree). This trend from mild to stronger CO_2 sink as the distance away from the coast increases is what we were referring to in our sentence but we did not mean to imply that any of these authors reported an actual source of CO_2 in the nearshore. The sentence was thus modified to clarify this point.

“This trend along a depth transect, suggesting a more pronounced continental influence on near-shore waters and a strengthening of the CO_2 shelf sink away from the coast was already discussed in the regional analysis of Chavez et al. (2007) and by Jiang et al., (2013) specifically for the South Atlantic Bight.”

P11997, 1st paragraph. Although estuarine filters may be a reason that can explain the north-south difference, there may be other reasons as well. For example, the Gulf of Maine is a semi-closed system, which may promote shelf-derived OC decomposition. In the Scotia shelf, there is riverine influence from the St. Lawrence River, I think (please check). So careful discussion and wording here are necessary.
- Aleck Wang

We agree with the reviewer that a number of processes other than the estuarine filter are known to influence the shelf CO_2 dynamics and are also potential contributors to the difference between the North and South sections. This includes currents, climate and, as the reviewer suggested, the temporary intrusion of the river plume of a large river (St Lawrence). We already mentioned some of these factors in the text but we now significantly elaborated on this in the revised discussion.

“Naturally, other environmental and physical factors also influence the carbon dynamics in shelf waters and contribute to the difference in CO_2 uptake intensity between both sections. For instance, in the North, the Gulf of Maine is a semi-enclosed basin characterized by specific hydrological features and circulation patterns (Salisbury et al., 2008; Wang et al., 2013) which could result in longer water residence times promoting the degradation of shelf-derived organic carbon. Other potential factors include the plume of the Saint Lawrence estuary, which has also been shown to transiently expend over the Scotian Shelf (Kang et al., 2013), the strong temperature gradient and the heterogeneous nutrient availability along the region which may result in different phytoplankton responses (Vandemark et al., 2011; Shadwick et al., 2011).”