Answers to reviewer #1

Over the past 10 to 15 years, there have been great advances in data collection and synthesis of air-water CO2 fluxes at the land-ocean interfaces. While global synthesis based on extrapolations of known systems have been presented using various approaches, we often do not know such information at regional scales. For example, while I have compared global air-water CO2 fluxes in estuaries and shelves with that of the open ocean to emphasize the importance of estuaries and shelves despite their small areas (Bauer et al., 2013; Cai, 2011), I cannot give such a comparison for the North America east coasts as, among other reasons, there is no pCO2 data from a few largest estuaries in that regions (e.g. the Chesapeake Bay). Furthermore, while spatial and seasonal distributions from a few systems are available and a general pattern of global spatial distribution such as mid-high latitude vs. low latitudes is known, overall we do not have a good sense on spatial and temporal distributions.
The paper authored by Laruelle et al. synthesizes the spatial and seasonal variability of CO2 fluxes at the air–water interface for the entire North East American Land–Ocean Aquatic Continuum, from streams to the shelf break. This is the first of its kind done at the sub-continental scale. The paper is well written and easy to follow. The paper can be accepted after a moderate refinement. Most importantly, I feel the uncertainty of estuarine flux should be fully appreciated. As mentioned above we have no data from the region's largest estuaries such as the main stem of the Chesapeake Bay and the Delaware Bay (Long Island Sound and New York Bight?). The estuarine degassing flux could be much lower if these large estuaries, some of them are highly eutrophic with likely low pCO2, are included. This fact must be clearly pointed out and the associated uncertainty should be assessed or at least mentioned.

We are grateful for the reviewer's positive comments. We agree that more emphasis could be drawn onto the estuaries and the uncertainties related to their emission rates. The small number of available estimates of estuarine outgassing in the region obviously is a major limitation and it certainly is an issue that the two largest estuarine systems in the region (Chesapeake and Delaware Bays) are not included in the set of estuarine systems for which yearly FCO_2 estimates are available. We believe that, considering the available data, our method is the most appropriate to derive a 'first order' picture of the CO_2 dynamics in the estuaries of COSCAT 827 but the problem of the representativeness of our average outgassing rate should be addressed in the manuscript. As the reviewer points out, the trophic status of the Chesapeake and Delaware Bays suggests that they could be characterized by relatively low pCO_2 values and would thus reduce our regional FCO_2 estimate if included into our calculations. It should be noted, however, that the average emission rate of 50 gC m^{-2} yr^{-1} we calculate for COSCAT 827 is already relatively low compared to other regional rates calculated in similar fashion for tidal estuaries, for which the global average is 218 gC m^{-2} yr^{-1} (Laruelle et al., 2013). While a qualitative discussion of the uncertainty associated to the representativeness of the studies used to derive our regional average needs to be included in our manuscript, it is difficult to effectively quantify this uncertainty. In response to the reviewer's remark, we introduced several sentences in the discussion, in which we first discuss the potential role of the Chesapeake and Delaware Bays in the regional estuarine carbon budget and compare our emission rate to global averages.

"It should be noted that our estimate of the estuarine outgassing is derived from a limited number of local studies, none of which were performed in the two largest systems of COSCAT827, that are, the Chesapeake and Delaware Bays (>80 % of the total estuarine surface area in COSCAT827). These estuaries are highly eutrophic (Cai, 2011), which suggests that they might be characterized by lower pCO_2 values and subsequent CO_2 exchange than the other systems in the region. On the other hand our regional outgassing of 50 gC m^{-2} yr^{-1} is already well below the global average of 218 gC m^{-2} yr^{-1} calculated using the same approach by Laruelle et al. (2013) for tidal estuaries."

Bauer, J.E., Cai, W.-J., Raymond, P.A., Bianchi, T.S., Hopkinson, C.S. and Regnier, P.A.G., 2013. The changing carbon cycle of the coastal ocean. Nature, 504(7478): 61-70.

Cai, W.-J., 2011. Estuarine and coastal ocean carbon paradox: CO2 sinks or sites of terrestrial carbon incineration? Annual Review of Marine Science, 3(1): 123-145

Answers to reviewer #2

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^3s^{-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.

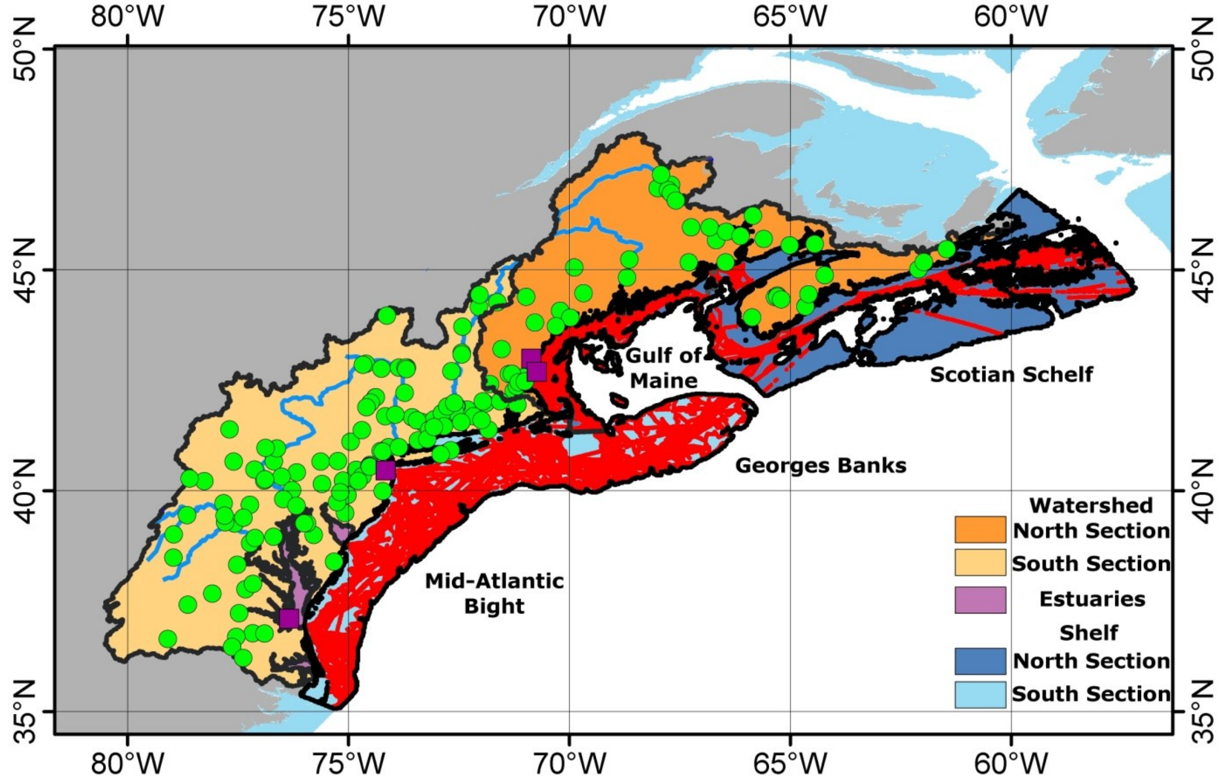
The first paragraph of the methods introduces COSCAT 827 as the study area and provided a brief description of what the COSCAT segmentation is. We re-wrote the first few sentences of this section to make sure the acronym COSCAT was spelled out after its first occurrence and expended the description of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

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

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

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

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

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

Equation 6

$$\ln(v\,[\mathrm{m\ s^{-1}}]) = -1.64 + 0.285 \cdot \ln(Q_{ann}[\mathrm{m^3 s^{-1}}]) \qquad \text{(after Raymond et al., 2012)}$$

Equation 7

$$\ln(v\,[\mathrm{m\ s^{-1}}]) = -1.06 + 0.12 \cdot \ln(Q_{ann}\,[\mathrm{m^3 s^{-1}}]) \qquad \text{(after Raymond et al., 2013)}$$

Equation 8

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

Equation 9

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

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).”

Answers to reviewer #3

General Comments:
The manuscript addresses the important problem of estimating the CO2 exchange at the air-water interface across the land-ocean boundary, from streams to the coast and shelf region, ranging from Cape Hatteras to the Scotian Shelf. The methodology is sound and the results provide useful insights into the processes that control the CO2 budget. I believe that the manuscript should be published with some minor changes, primarily to provide more details of the methodology and expanding the discussion of results.

We thank the reviewer for his positive comments and answered his remarks, point by point, to the best of our abilities. Some of the reviewer's requests regarding the clarification of some methodological aspects of our study were also formulated by reviewer #2 and we occasionally refer to those answers in our replies.

Specific Comments:
Page 11989, sentence starting on line 9, "The riverine data…". Here the authors should be more specific on how the data, riverine pH and alkalinity and shelf SOCAT pCO2 are used to derive the air-water CO2 exchange. Specifically, details on how was the pCO2 derived from pH and alkalinity.

The riverine pCO_2 values were calculated from pH, alkalinity, water temperature, and, where available, major ion concentrations, using the hydrochemical modelling software PhreeqC v2 (Parkhurst & Appelo, 1999). This information has been added in the methods section of the manuscript.

"Lauerwald et al. (2013) calculated pCO_{2river} values from pH, alkalinity, water temperature, and, where available, major ion concentrations, using the hydrochemical modelling software PhreeqC v2 (Parkhurst & Appelo, 1999)."

Equation (1), the procedure to derive k should be more detailed as it requires an approach quite distinct from the gas transfer coefficient evaluation in the open ocean.

We agree with the reviewer and elaborated on our methodological approach. See our response to Reviewer #2 on the same query.

The discussion of methods for rivers, estuaries and continental shelves is a bit out of balance, with much longer description for rivers than for estuaries and shelves.

We agree with the reviewer's comment that the description for the rivers is significantly longer than for estuaries and the shelves. In answers to some of reviewer's 2 suggestions, additional information was provided to describe the calculations for the estuaries and the shelf but, overall, their descriptions remain shorter than that of the rivers. We carefully looked into this and we actually believe that this imbalance reflects the required amount of information that is needed to properly describe each method, which are quite distinct. Stated differently, the imbalance does not result from ignoring details in the estuary and shelf description, but rather because the river FCO_2 estimates require specific predictors for, e.g., surface area (and thus stream width), channel slope and current velocity (for the exchange coefficient k).

Also, the manuscript could improve with the use of a table summarizing the data sources used for each of the three regions.

The table requested by the reviewer has been added to the manuscript (as new table 1) and is also provided below.

Page 11993, the "Results and discussion" session has rivers, estuaries and continental shelf results blended together. It makes the interpretation somewhat difficult, especially taking in consideration that in the methods section they were treated separately.

We understand the reviewer's concern regarding the readability of the results and discussion section but we believe that our integrated vision of the LOAC should be reflected in the way the results and discussion is structured, with the simultaneous analysis of all connected compartments to provide an overall understanding of the regional biogeochemical dynamics. We thus feel that dissociating each compartment in the discussion would weaken the message we are trying to convey. However, to ease the interpretation of our ms. the "Results and discussion" section was restructured in such a way that long paragraphs now appear as smaller sections, dedicated specifically to the rivers or the continental shelf. We hope this improves the readability of our ms. while preserving the integrated view.

Nine lines of conclusions seem a bit short.

We agree that our conclusion was particularly synthetic. We re-wrote the section to include the temporal dynamics of the CO_2 exchange with the atmosphere and to better explain to role of estuaries in the overall carbon balance.

"Our data driven spatially and seasonally resolved budget analysis captures the main characteristics of the air-water CO_2 exchange along the LOAC of COSCAT 827. It evidences the contrasting dynamics of the North and South section of the study area and an overall gradual shift from a strong source in small streams oversaturated in CO_2 towards a net sink in continental shelf waters. Our study reveals that ice and snow cover are important controlling factors of the seasonal dynamics of CO_2 outgassing in streams and rivers and account for a large part of the difference between the North and South section. The close simultaneity of the snow melts on land and of the phytoplankton bloom on the continental shelf leads to opposite temporal dynamics in FCO_2 in these two compartments of the LOAC. In addition, our results reveal that estuaries filter significant amounts of terrestrial carbon inputs, thereby influencing the continental shelf carbon uptake. Although this process likely operates in conjunction with other regional physical processes, it is proposed that the much stronger estuarine carbon filter in the South section contributes to a strengthening of the CO_2 sink in the adjacent continental shelf waters."

Technical Corrections:
Page 11988, line 24, change region to regions.

Done

Page 11990, equation (1), give units of all variables used, for instance, units for FCO2 and k are not given.

Ok

Page 11992, “uncertainty of the yearly FCO2s”? “yearly estimates of FCO2” reads better.

Agreed, the text was modified accordingly.

Page 11993, line 17, change “Scotian shelves” to “Scotian Shelf”. Check other occurrences in the text as well

Done (3 occurrences)

Table 1: Summary of the data used for the FCO_2 calculations in compartment of the LOAC.

Compartment	Parameter	Description	Source	Reference
Rivers	*pCO2*	CO_2 partial pressure	GLORICH	Hartmann et al., 2014; Lauerwald et al., 2013
	-	River network, digital elevation model (DEM)	Hydrosheds 15s	Lehner et al., 2008
	-	Runoff	UNH/GRDC	Fekete et al., 2002
	T	Air-temperature	-	Hijmans et al., 2005
		Lake surface area	Global Lake and Wetland Database	Lehner and Döll, 2004
Estuaries	*As*	Surface Area	SRTM water body data set	NASA/NGA, 2003
	-	CO_2 exchange rate	Average of local estimates	Raymond et al., 1997; Raymond et al., 2000; Raymond and Hopkinson, 2003; Hunt et al., 2010
Shelves	*As*	Surface area	COSCAT/MARCATS Segmentation	Laruelle et al., 2013
	ΔpCO_2	pCO_2 gradient at the air-water interface	SOCAT database	Bakker et al., 2014
	k	calculated using wind Speed	CCMP database	Altas et al., 2011
	K'_0	Solubility, calculated using salinity, water temperature	SOCAT database	Bakker et al., 2014