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Biogeosciences Discussions

Interactive comment on "Satellite observations reveal high variability and a decreasing trend in CO_2 fluxes on the Scotian Shelf" *by* E. H. Shadwick et al.

E. H. Shadwick et al.

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All three reviewers made insightful and constructive comments. We very much appreciate this feedback and have made the suggested changes. It is our belief that the manuscript has been considerably strengthened as a result of these changes, which included expanding some sections and modifying and shortening others. The validation of the multiple linear regression used to compute pCO2 has been expanded and 3 independent sets of observed pCO2 have now been used, including data from the CARINA database. Our response to each of the reviewer comments is below.

Response to Reviewer #1

C1: The manuscript contains a long, but not much in depth, discussion on the seasonal variation of surface water pCO2 on the Scotian Shelf. It is not clear whether there is overlap between Shadwick et al. (2010) and the current manuscript (section 3.2, section 4, figure 8). Any significant overlap should be removed. I recommend shortening of the current text on seasonal variation and / or a more in depth discussion of this topic.

R1: Any overlap between this manuscript and Shadwick et al. (2010) has been removed. Figure 8 has been removed from the revised manuscript. The discussion of seasonal variation has been revised.

C2: The article would benefit from a more in depth discussion of the spatial and interannual variation in surface water pCO2 on the shelf and of why CO2 fluxes here differ from those elsewhere, as touched on on pages 10-11.

R2:A new discussion section ('The Scotian Shelf in the Larger Global Context') has been added to the revised version of the manuscript. The air-sea CO2 fluxes in this region are compared with other coastal regions and the behavior of the Scotian Shelf is discussed in the context of other systems.

C3:A presentation of the hydrographic setting and circulation on the Scotian shelf might inform on the processes governing the seasonal, spatial and interannual variation of pCO2. E.g. how important is tidal mixing (not mentioned in the current text)? What is the importance of the outflow of the St. Lawrence? Does the Labrador Sea Current from the north dominate water on the shelf or is there any/much influence from the Gulf Stream? What processes govern upwelling on the shelf, when does it take place and how frequent is it (line 352)? How common is convective mixing (line 352)? The hydrographic setting will not be familiar to overseas colleagues.

R3:A new section ('Oceanographic Setting', Section 2 in the revised manuscript) has been added. The hydrography of the Scotian Shelf, including upwelling and convection, is discussed. A new figure (Fig. 1 in the revised version) has been added and includes

the geographic and hydrographic setting.

C4:The comparison with the East China Sea (lines 338-345) is very interesting. How is the Scotian Shelf situated with respect to the Gulf Stream ('a northward flowing boundary current' – line 340)? Could the difference between CO2 fluxes in the East China Sea (CO2 sink) relative to the Scotian Shelf (CO2 source) result from the position relative to the northward flowing boundary current and the southward flowing current (Labrador Sea Current). Could the CO2 outgassing on the Scotian Shelf result from the net warming of Labrador Sea water as it flows south or are other processes responsible?

R4:The role of the Labrador Current and the Gulf Stream are discussed in the comparison of the Scotian Shelf with the East China Sea (Section 5.1 in the revised manuscript). The warming of waters as they transit south and the potential contribution to the outgassing observed on the Scotian Shelf is discussed.

C5:There is no strict separation between methods, results and discussion. E.g. equation 1 (line 76) is both a method and a result. While I do not object to equation 1 as part of the methods, its early introduction leads to a rather bitty description of equation 1 (section 2), its meaning (equations 5-7, section 3) and interpretation (section 3, 4). This is confusing, in particular as the error estimate of pCO2 estimated with equation 1 is hiding in the caption of figure 3, while most figure captions provide poor information on the contents of the figures.

R5:The manuscript has been re-organized and there is now a strict separation between methods and results. The multiple linear regression, the discussion of the coefficients, and the spatial extrapolation and validation are now in the results section. The description of Eq. 1 and the error associated with the regression has been expanded. The figure captions have been revised to included more information about the figure content.

C6:On the pCO2 fit (equation 1, line 76, lines 170-205): It is not clear whether the three

terms in the pCO2 fit (equation 1, line 76) are all essential. How much does the gas transfer velocity improve the fit? Is the gas transfer velocity essential in the fit? How does inclusion of a third term reduce the error of the pCO2 estimates? The errors in the pCO2 fit are not discussed, when the equation is presented (line 76). The caption of figure 3 mentions an error of 16 _atm, but without an indication of how the error has been determined. An error of 16 _atm is substantial and it is important to determine the effect of this error on the flux estimates.

R6: The choice of input parameters in the derivation of Eq. 1 has now been discussed in more detail. The error associated with the regression, and the improvement of the fit with the inclusion of the gas transfer velocity is now discussed as suggested. The error in the computed pCO2 is now given. The caption of Figure 3 has been revised.

C7 :The uncertainty related to spatial extrapolation of the fit should be discussed. The application assumes that the CO2 dynamics of the mooring site also apply in boxes 1-7. How realistic is this?

R7:The uncertainty associated with the spatial extrapolation has now been discussed. We have assumed that the dynamics at the mooring site apply to the larger Scotian Shelf region. The validation of the computed pCO2 with 3 sets of independent pCO2 observations in all grid boxes (with the exception of box 6) indicates that the seasonal cycle of pCO2 is well reproduced in the region, and supports the assumption that the algorithm can be extrapolated spatially (Section 4.3 and Figs. 6 and 7 and Table 3 in the revised version).

C8: Equation 1 has a pCO2 dependence on the gas transfer velocity, which is later linked to mixed layer depth (figures 5, 6). The model in figure 8 does not contain a parameter for wind-driven mixing. This difference between the fit and the model is not discussed.

R8:Figure 8 and the discussion of the 1-D model have been removed from the revised manuscript.

C9: Figure 4: How well do the CARIOCA pCO2 data match underway pCO2 and calculated pCO2 (equation 1)?

R9: The CARIOCA data agree well with the underway and discrete pCO2. The model pCO2 is further compared with data from the CARINA database. A Table comparing the (monthly) values of all independent observations with the pCO2 computed (using Eq. 1) is now included (Table 3 in the revised version).

C10:On the annual increase in pCO2 (equation 2, line 77): The correction should be made relative an appropriate month in 2007-2008, as the pCO2 fit in equation 1 has been made for 2007-2008. Has the correction been applied correctly?

R10:The correction is made relative to the month of January 2008, and has been applied correctly.

C11:On equation 3. I do not understand this equation and the meaning of the Beta coefficients. Is the explanation correct (lines 91-94)?

R11:The text has been clarified. The beta coefficients are the normalized, dimensionless, regression coefficients. The weight of each of the input parameters (Chl, SST, k) is related to the magnitude of the corresponding beta coefficient.

C12:Section 3 (including section 3.1, pages 5-8) is long, and not always convincing, nor clear, nor particularly exciting. In particular I struggle with section 3.1 on pCO2 gains, part of the interpretation of frequencies in the power spectrum (0.02 hr _ 48 hours or 2 days) and much of page 8. I recommend shortening this text substantially.

R12:A reference for the time-series analysis has now been given. This section has also been shortened considerably, and the majority of the discussion moved to an Appendix.

C13:Figure 5 and main text. Tidal mixing is not mentioned in the article. Can tidal mixing be ignored on the Scotian shelf? How does tidal mixing affect the relationship between the gas transfer velocity and mixed layer depth? Would this relationship vary from one site to another (e.g. as a result of differences in tidal mixing)?

R13:We have assumed that on the monthly timescale the (net) effect of tidal mixing can be ignored. It is possible that the tidal mixing would have a (short-term) effect on the relationship between the mixed-layer and the gas transfer velocity, through a change in mixed-layer depth, but this should not be large enough to override the linear relationship shown.

C14:Figure 8. Line 250-267. The text is weak, not convincing and raises more questions than it answers. Neither the text nor figure caption 8 provide information on how the modelling was done. Has the total pCO2 been observed or modelled? If total pCO2 was observed, how was this done (where, by what instrument)? Is any of the signals a residual (difference)? It is not made clear why the model does not contain the mixed layer depth or gas transfer velocity (as does equation 1). For which location is the model? Is the model based on pCO2 and SST data? Remove 'since it is clearly ....spring bloom', as this comment does not make sense. Many details are missing in the methods section and need to be added (see further comments below).

R14:Much of the discussion regarding the 1-D modelling (Shadwick et al. 2010) has been removed. Figure 8 has been removed from the revised manuscript, and the time-series analysis relocated to an Appendix.

C15:The article needs polishing, with careful attention for detail. For example: Correct references to equations (line 191, 193); Correct links to references (Cai et al., 2006; Cai, 2006 line 33-34 versus Cai and Wang, 2006 in the references); Adequate statistics, e.g. on the error in estimates of fits (equation 1 in line 76, fit in figure 5); 0.02 hr-1 corresponds to 50 hours (not to 40 hours) (line 223 and elsewhere); Adequate information in the figure captions on the origin (spatial and temporal resolution, grid box or mooring site) of data; Figures should be introduced in chronological order. All figures should be referred to in the text. (Figure 11 is cited before Figure 9, while Figure 10 is not cited at all.)

R15:These corrections have been made. The discussion on the error associated with

the multiple linear regression has been expanded. The figure captions have been revised, and the order of the figures has been corrected.

Minor comments C1:The title: 'A decreasing trend in CO2 fluxes' is vague, as it is unclear if the waters are an overall sink or source for CO2. The word 'trend' is superfluous. The title needs strengthening.

R1:The title has been changed (Air-Sea CO2 fluxes on the Scotian Shelf: from seasonal to multi-annual variability), as suggested.

C2:Sections 2, 3. Check the use of subsections in Biogeosciences. Generally a section starts with subsection 2.1 (line 41) if a later subsection 2.2 (line 97) is used.

R2:The revised manuscript has been re-structured, and there is no longer a section containing only one sub-section.

C3:Line 44. Will you make the pCO2 observations public via CDIAC?

R3:The authors certainly appreciate that Biogeosciences promotes the full availability of data sets corresponding to the papers it publishes. The work presented here is part of a Canadian Foundation for Climate and Atmospheric Sciences (CFCAS) project, and is therefore subject to the regulations agreed upon by all project participants. The data will become available to the public in due course according to the applicable CFCAS regulations.

C4:Line 57. At what depth were the shipboard samples taken? How were shipboard chlorophyll samples analysed? (type of analysis, etc).

R4:The ship board samples were collected at 3, 5, or 10 meters depth. Chlorophyll-a concentration was determined fluorometrically in a Turner Designs fluorometer using the acid ratio technique (Strickland and Parsons, 1972). This has been added to the text.

C5:Line 67. Recent work by Sweeney et al. (2007) suggests that the Wanninkhof

(1992) equations may overestimate the gas transfer velocity. It would be good to acknowledge this possible overestimation of the gas transfer velocity and CO2 air-sea flux estimates. Alternatively you might use the Sweeney et al. equation or the Nightingale et al. (2000) equation.

R5:The possible overestimate of the gas transfer velocity using the parameterization of Wanninkhof (1992) has now been mentioned. The error on the flux computation has been estimated by comparing the fluxes computed using Wanninkhof (1992) with those computed using Nightingale et al. (2000).

C6:Line 100. Dissolved organic matter complicates remote sensing of ocean colour in near-shore waters. How did you address this problem for your research area?

R6:The shortcomings of the SeaWiFS data in coastal regions due to CDOM and suspended particulate matter have now been mentioned in the text, and the impact of these shortcomings on the monthly mean values of ChlSAT have now been discussed.

C7:Line 102. Describe how the 9 km satellite chlorophyll and SST data were transferred to the 2 by 2 grid boxes (by averaging??) for e.g. figures 2, 3? How did you calculate fluxes for the grid boxes and in particular at what stage did you average SST and chlorophyll (e.g. at the end)? Did you use the individual 9 km data for figure 11?

R7:The data were transferred to the grid boxes by averaging. The SST and Chl values used in the computation of pCO2 are the average values for the grid box. One value of pCO2 was computed per month using the monthly mean Chl, SST, and the monthly-integrated k. The fluxes were computed using hourly winds, the monthly mean SST and pCO2, and with monthly mean salinity from a climatology of the Scotian Shelf. These hourly fluxes were integrated over a month. Figure 11 was generated using annual values, computed with monthly mean values, for each of the grid boxes.

C8:Line 104 and elsewhere. Does ChlSAT always refer to a specific spatial scale (e.g. for a grid box)? Or does it sometimes refer to the pixel(s) near the mooring and

sometimes to a grid box?

R8:The ChlSAT in the text always refers to the spatial scale corresponding to a grid box, except in the discussion of the spatial extrapolation and the standard deviation of the satellite chlorophyll data. In this case it refers to the standard deviation associated with the pixels used to compute the average for the grid box 1.

C9:Line 104-106. Sentences 104 to 106 are confusing. What is the location of the ChlSAT in the regression (average for grid box 1 or co-location to mooring site)? What do you mean with scaling? Why did the data need scaling? Provide information on the scaling factor.

R9:The regression was based not on ChlSAT, but on ChlF, i.e. calibrated fluorescence measured by the CARIOCA bouy. The scaling was done to account for the low (monthly mean) values of ChlSAT in grid box 1 compared to ChlF, and is now discussed in the text. The scaling factor was equal to 0.979 (i.e. ChlSAT = 0.989ChlF).

C10:Line 105, line 177. It seems unusual to test the significance of a correlation (p-value).

R10This has been removed.

C11:Line 108-109. What data is this standard deviation for, e.g. for the average of monthly data in a grid box?

R11:The standard deviation is associated with the pixels used to compute the monthly and spatially averaged ChlSAT in the grid boxes.

C12:Line 115. Provide a description of the underway pCO2 system or a reference an earlier publication for your system. Which of the systems in Kortzinger et al. is it? Where was it built? What type of equilibrator does it have? How did you calibrate the analyser? Etc.

R12:The description of the underway system has been expanded as suggested. The

calibration and data correction are also described.

C13:Line 121. Which set of carbonate system equations did you use? How well do the shipboard pCO2 data and the estimated pCO2 data compare to the CARIOCA pCO2 data?

R13:We used the equilibrium constants of Mehrbach et al. (1973) refit by Dickson and Millero (1987). This has now been included in the text along with an estimate of the uncertainty associated with the measurements of DIC and TA, and with the computation of pCO2. A comparison of shipboard, and computed pCO2 is now given (Table 3 in the revised version).

C14:Line 159-160. The text 'allowing the effects to .... distinguished' does not make sense. Remove or clarify.

R14:This has been removed.

C15:Line 163. Add space in 'south of'.

R15:This has been corrected.

C16:Line 163-169. This paragraph needs clarification. In particular, how do you derive 115 mg C/mg Chl-a? Did you use a buffer factor?

R16: The 115 mg C/ mg Chl-a was derived using the Revelle Factor. For every mg/m3 of Chl there is a change in pCO2 of 24.6 μatm. We used a mean annual pCO2 of 420 μatm, a mean annual DIC of 1950 μmol/kg and a Revelle factor of 13. This has been added to the text.

C17:Line 175. What criterium did you use for estimating the mixed layer depth? A density increase, a temperature increase or by eye balling the depth?

R17:The mixed-layer depth was estimated from monthly profiles of temperature and defined as a difference in temperature between depths of greater than 0.5°C. This has been added to the text.

C18:Lines 186-188. ' to the areal ..... CO2 inventory'. What is the statement based on? What does it mean? Remove?

R18:This has been removed.

C19:Line 191. Correct to 'equations 5-7'.

R19:This has been corrected.

C20:Line 193. Correct to 'equation 5' (???).

R20:This has been corrected.

C21:Line 201, line 256, and elsewhere. What evidence do you have of non-Redfield production on the Scotia shelf? Why is this relevant for this article? I suggest removing the mention of non-Redfield production, unless it is critical for your argument, in which case it needs considerable clarification.

R21:This has been removed.

C22:Line 224 and elsewhere. Figure 7 has little evidence of the gain in pCO2 approaching 4.6 matm/C; 8 matm/C more realistic. Your statement comes across as wishful thinking.

R22:This has been corrected. The majority of the discussion of the SST regression coefficient has been relocated to an Appendix.

C23:Line 226-227. Why do you subtract the 24 hour daily mean, rather than a running 24 hour mean? I wonder if this explains some of the peaks at 0.02 hr-1 and the similar gains for 0.04 hr-1 and 0.02 hr-1, which are both multiples of 24 hours. Section 3.1. Figure 7. What method was used for the frequency analysis? Please, explain the meaning of the gain of a power spectrum, or at least provide a reference to the method. Lines 238-249. The interpretation is not convincing and appears subjective. Shorten, remove or strengthen text.

R23:Subtracting a running 24-hr mean does not cause the peaks at 0.02 hr-1 0.04 hr-1, despite these being multiples of 24 hours. We have therefore left the analysis using the 24-hr mean. The method used in the time-series analysis has now been referenced in the text. This section has been revised and shortened. Most of the discussion of the SST regression coefficient, including the time-series analysis, has now been moved to an appendix.

C24:Line 258. Why is the spring and summer pCO2 drawdown not resolved by the chlorophyll record? This seems bizarre. The chlorophyll record has large peaks in April. Is there a subsurface chlorophyll maximum?

R24:There is evidence of post-bloom biological production (carbon uptake) in the absence of large increases in surface chlorophyll concentration on the Scotian Shelf (Shadwick et al. 2010). Losses of surface particles due to both sinking and grazing can be significant (up to 45% of the surface waters being cleared of particles in a given day, e.g. Banse 1994, 1995). These losses diminish the chl-a concentration, thus leading to lower observed values, although DIC has been consumed.

C25:Section 4. line 268. Section 3 contains results. Thus 'results' should be removed from the header of section 4 ('Results and Discussion').

R25:The manuscript has been restructured so that the results, including the presentation of the regression and the extrapolation and validation are now presented in the 'Results' section. The 'Discussion' section has been expanded to include a broader discussion of the CO2 system in the Scotian compared to other regions.

C26:Line 290. Change 'cold temperature minimum' to a 'low temperature minimum'.

R26:This has been corrected.

C27:Line 317-335. The spatial extrapolation of the fluxes is very interesting, and you might strengthen this part of the article, e.g. by also showing maps of pCO2, chlorophyll and SST for the grid boxes.

R27:The spatial extrapolation has been expanded as suggested and is now in a section called 'Shelf-Wide CO2 Fluxes'. Maps of annual mean pCO2, SST, ChlSAT have also been included and discussed in the revised version.

C28:Line 324. Clarify or remove 'freezing point' (of freshwater °C??). At present the term is confusing, given the lower freezing point of seawater.

R28:This has been corrected.

C29:Line 329-333. The link to the NAO is pretty vague. You might strengthen and clarify this.

R29:This has been expanded and clarified as suggested.

C30:Line 333-335. What do you mean? The text on the long-term change is vague. Consider removing this.

R30:This has been removed as suggested.

C31:Line 338-340. Add a verb to the sentence 'The East China Sea ....boundary current'.

R31:This has been corrected.

C32:Line 362-363. What does 'this observation-based approach' refer to? To this manuscript? Or to Shadwick et al., 2010? Clarify.

R32:This has been clarified as suggested.

C33:Line 363. In which way do the results conflict with (which?) modelling studies? Clarify or remove.

R33:The results of this study, and of Shadwick et al. (2010), indicate that the Scotian Shelf acts as a source of CO2 to the atmosphere at the annual scale. The modeling studies of Fennel et al. (2008) and Previdi et al. (2009) suggest that the region acts net sink for atmospheric CO2 on the annual scale. This has been clarified as suggested.

C34:Table 1. Expand the descriptions, such that they can be understood without reference to the text. (e.g. add ' by the buoy' for FChl. Add 'shipboard' and the analytical technique for Chl-a. Add 'buoy values calibrated to shipboard Chl-a' for ChlF.

R34:The descriptions in the Table have been expanded as discussed.

C35:Table 3. Use a constant number of decimals for the fluxes in box 1. The shelf-wide flux for 2006 is -0.02 mol C/m2/yr in the table, but 0.01 mol C/m2/yr in the text. Correct 'gird' to 'grid'.

R35:This has been corrected as suggested.

C36:Figure 1. Is it possible to improve the resolution of the coast line? The Canadian /US mainland coast looks bizarre. The figure should specify the oceanographic regions mentioned in the text, e.g. the St. Lawrence, Cabot Strait, the Gulf of Maine, the Gulf of St. Lawrence. Ideally the depth of the 200 m contour should be mentioned in the caption. The location of the Sable Island meteorological station should be indicated. Ideally Nova Scotia should be identified. It would furthermore be useful if the main ocean currents are indicated (e.g. Labrador Sea Current, St. Lawrence outflow). Is the Scotian shelf part of or a neighbour of the Grand Banks?

R36:A new figure (Fig. 1 in the revised version) includes the regions mentioned in the text and the location of Sable Island has been indicated as suggested. Nova Scotia, Cabot Strait, and the Gulf of St. Lawrence have been labeled as requested. The Labrador Current has also been shown schematically. The Scotian Shelf is NOT part of the neighboring Grand Banks.

C37:Figure 2. The gas transfer velocities are presumably from the weather station (e.g. Fig 3c), while other data are monthly averages per grid box (Fig 2a, 2b, 2d). The text on the inset with shipboard chlorophyll in Fig. 2a is confusing, as it breaks the description of the other parameters.

R37:The caption has been revised as suggested, and the inset has been removed.

C38:Figure 3. What SST data are shown in Fig. 3a (monthly satellite SST for box 1?). Are the fluxes in Fig 3c for box 1? What are the bars on the annual fluxes (Fig. 3c)?

R38:The SST data shown in Fig. 3a are the satellite SST for grid box 1. The fluxes in Fig. 3c are also for grid box 1. The error on the annual flux is the 20% uncertainty associated with the flux computation.

C39:Figure 6. Are the values for the mooring site or for grid box 1? Did you calculate or observe the total pCO2 values? For which period are the values (2007-2008?). Relative to which month are the values (January 2008)? What values were used for SST (buoy, satellite?). The caption should make it clear how these data were calculated and/or refer to the relevant text (equations 5-7).

R39:The values shown in Fig. 6 are the CARIOCA measurements (of SST, and ChlF) at the mooring location (April 2007 – March 2008), the k values are from the Sable Island Station. This caption has been clarified.

C40:Figure 7. Briefly explain pCO2* and SST* in the caption and refer to the relevant section in the text (equation 9). The insets are rather small. 0.02 hr-1 corresponds to 50 hours (or roughly 48 hours).

R40:The figure caption has been modified as suggested, and the time-series analysis has been relocated in an Appendix.

C41:Figure 9. The figure caption is not clear. Should one interpret Wind + as a year with lots of wind in winter AND spring? Figure 9b is not clear at all and should be improved or removed. Figure 9a is not essential for the article.

R41:This figure has been removed.

Response to Reviewer #2

C1: A paragraph should be included briefly outlining the physical, biological and chemical oceanography of the study area, and Figure 1 should be modified to include the

geographical features mentioned in the text. The location of the Sable Island Meteorological Station where the atmospheric CO2 and wind speed were measured should also be indicated on Figure 1, and the distance from the CARIOCA deployment site to Sable Island should be mentioned in the text.

R1: A new section ('Oceanographic Setting', Section 2 in the revised version) has been added to the manuscript, and a brief description of the physical system on the Scotian Shelf is now included. The geographic features mentioned in the text, along with the location of the Sable Island Meteorological Station, are now labeled in Fig. 1 as suggested.

C2: Local time should be included as well as UTC (page 5272 line 11).

R2: Local time is now included in addition to UTC as requested.

C3: Monthly integrals of the gas transfer velocity were determined from hourly wind speed data. A measure of the variation in the wind speed and the resulting gas transfer velocity occurring in this integration process should be given.

R3: These values have now been included as requested.

C4: Equation (1) is a key component of this work. More information needs to be given on the contribution of each term to the equation, the error associated with each coefficient and the improvement of the fit when the additional terms are included. This information could be included as an extension to Table 2. Why is k included in Eqn (1) and not wind speed?

R4: The discussion of the error associated with Eq. 1 has been expanded, and the improvement of the fit with the inclusion of the gas transfer velocity is now discussed as suggested. We used k and not wind speed since the latter, which is representative of wind stress, is the quantity of interest in the computation of air-sea CO2 flux. This justification has now been included in the text.

C5: It appears that the monthly mean CARIOCA pCO2 data were used in the derivation

of equation (1) (page 5272 line 25), then values computed using equation (1) were compared with pCO2 measured by the CARIOCA buoy (page 5274 line 23), this is a circular argument. It should be made clear which data is used in the derivation of equation (1), and which data is used as verification. Obviously these should not be the same data set. The use of shipboard measurements and values calculated from alkalinity and DIC for verification of Eqn (1), as was also done, is independent.

R5: The validation of the model (Eq. 1) has been expanded. The computed pCO2 are now compared with 3 sets of independent shipboard observations, and not with the data from the CARIOCA which was used in the derivation of Eq. 1. These values are now presented in a table (Table 3 in the revised version).

C6: The SST and ΔpCO2 anomalies have been defined (Page 5274 line 21) however these values are not referred to again in the manuscript. The anomalies should be presented and commented on, as this is the measure of how good Eqn (1) is at predicting pCO2.

R6: The definition of the SST and ΔpCO2 anomalies is now presented at the beginning of the discussion regarding the change in pCO2 and SST over the 10-year period. The anomalies are now discussed in greater detail as suggested.

C7: The wind speed was measured only at the Sable Island site, therefore the same value of k was used in each grid box in the hindcast? Some comment on the wind speed variability should be made, particularly any land / coast effects.

R7: The same value of k was used in each grid box in the hindcast of air-sea CO2 flux. It has been shown that winds measured at Sable Island are representative of winds for the wider Scotian Shelf region (Petrie and Smith, 1977), and the use of observed winds at one location was deemed preferable to modelled winds throughout the region. A statement regarding the over/under estimate of wind speed near the coast and offshore by the Sable Island wind is now included in the text as suggested.

C8: The CARIOCA buoy was moored close to the coast, and the ChlF and ChlSat was “roughly” 1:1 (page 5274 line 15), however explicit mention should be made of the shortcomings of the SeaWIFS data in coastal regions due to CDOM and suspended particulate matter, and comment made on the impact this may have on the work presented here.

R8: The shortcomings of the SeaWiFS data in coastal regions due to CDOM and suspended particulate matter have now been mentioned in the text as suggested, and impact of these shortcomings on the monthly mean values of ChlSAT have now been discussed as requested.

C9: Which equilibrium constants were use in the calculation of pCO2 from DIC and TA (page 5275 line 7)?

R9: We used the equilibrium constants of Mehrbach et al. (1973) refit by Dickson and Millero (1987). This has now been included in the text along with an estimate of the uncertainty associated with the measurements of DIC and TA, and with the computation of pCO2.

C10: CO2 flux from the ocean into the atmosphere is defined on Page 5276 line 12 and in the table 3 caption as negative, however ΔpCO2 is defined as pCO2(ocean) – pCO2(atm) on Page 5282 line 18. A ΔpCO2 defined in this way would give a positive flux when the ocean is a source of atmospheric CO2. The usual convention (eg Takahashi et al DSR II 2009) is that ΔpCO2 = pCO2(ocean) – pCO2(atm) and flux from the ocean to the atmosphere is positive.

R10: We defined ΔpCO2 as pCO2(ocean) – pCO2(atm). Thus if the pCO2(ocean)=250 and pCO2(atm)=390, the ΔpCO2 = -150. The flux and the ΔpCO2 must have the opposite sign, yielding a positive flux (in spring). If pCO2(ocean) = 400, and pCO2(atm)=390, then the DpCO2=10, and the flux is negative (indicating outgassing).

C11: What does ": : : are compared with the same high precision, : : :" (Page 5276 line14) mean, and what statistical evidence do you have for this statement?

R11: This statement has been removed from the text.

C12: The sentence ".. The ChlF coefficient corresponds: : :" (Page 5277 line 1) refers to a range of 115 mgC (mg chla)-1. 115 is not a range.

R12: This statement has been corrected.

C13: What method was used to estimate the mixed layer depth (Page 5277 line 15 and Fig. 5)?

R13: The mixed-layer depth was estimated from monthly profiles of temperature and defined as a difference in temperature between depths of greater than 0.5°C. This has been added to the text.

C14: Page 5278 line 5 refers to equations (5) to (7) not (4) to (6), Page 5278 line 6, refers to equation (5) not equation (3).

R14: This has been corrected.

C15: The cross-spectrum method used to examine the relationship between pCO2 and SST should be referenced. I am not familiar with this technique, however it seems to be over dominant in this paper. I suggest that this section be shortened.

R15: The cross-spectrum method has been referenced as requested. This section has also been shortened considerably, and the majority of the discussion moved to an Appendix.

C16: The authors infer that the NAO could be influencing the CO2 dynamics in the Scotian Shelf area, however the reasoning is vague. The NAO link is mentioned in the abstract, however the justification in the text does not warrant this. The section should be expanded.

R16: The reference to the NAO has been removed from the abstract, and the discussion regarding the NAO has been expanded in the text as suggested.

C17: More context on the East China Sea should be given, otherwise the comparison with the Scotian Shelf is irrelevant. The sentence introducing the East China Sea comparison (Page 5284 line 3) does not contain a verb, therefore makes no sense.

R17: The comparison of the Scotian Shelf and East China Sea systems has been expanded as suggested.

C18: The lack of agreement between the observations and the modelling studies (Page 5285 line 2) needs further comment.

R18: We have elected not to comment further on the lack of agreement between the observations (which we have presented here, and in a paper under revision) and modelling studies conducted by colleagues. While this disagreement is interesting from a scientific standpoint, we do not believe that it is our role to investigate the shortcomings of models. We would welcome discussion with colleagues working on models in this region, but think that an investigation of the model-observation discrepancy should be initiated from their side, since they are much better positioned to identify reasons for disagreement.

C19: Fig. 6 shows the change in pCO2 for each of the contributing factors , how was the initial pCO2 determined, and why isn't it the same for each contribution? Similarly, for Fig. 8)

R19: The initial pCO2 was determined using Eqs. 5-7 and should be the same for each contribution. This figure has been revised accordingly. Figure 8 has been removed.

C20: Figure 9 is not necessary. If the authors decide to retain Fig. 9 then the caption should be expanded to explain the meaning of Wind + and Wind -.

R20: Figure 9 has been removed from the revised manuscript.

Response to Reviewer 3

General Comment:

Authors use in-situ data (pCO2, fluo, SST, etc...) collected over more than one year and satellite data (Chla etc...) over 10 years to extrapolate in space and time pCO2 and evaluate the interannual variability of the air-sea CO2 fluxes in the Scotian shelf. Such approach is not new, but it is the first time it is applied in this region. This is a nice try to extrapolate annual observations in space (7 boxes) and time (10 years) but authors should justify more clearly both the space and temporal extrapolations. The validation of the "model", expressed by Equation (1), is not really discussed; neither the sensitivity analysis depending on the error in model parameters and/or forcing fields. In this context, I strongly suggest to explore historical datasets that could be used to evaluate the extrapolation back to 1999 in order to better discuss the decadal pCO2 trends. Based on the model results, authors also derive interesting conclusions (link with NAO, pCO2 trends...) but, as also identified by other reviewers, more analysis and discussions are needed on these topics. At that stage, I do not recommend publication of this manuscript.

Response: The validation and spatial extrapolation sections of the manuscript have been expanded. The discussion of the model error, with respect to the parameters used in the regression has also been expanded. We have explored historical datasets as suggested, but have only found data back to the year 2004 in the region.

Specific comments:

C1: Title: Should be changed; the variability of air-sea CO2 fluxes is not revealed by satellite observations but by the method used in the analysis. The same applies for the "Decreasing trend in CO2 fluxes" in the title.

R1: The title has been changed (Air-Sea CO2 fluxes on the Scotian Shelf: from seasonal to multi-annual variability), as suggested.

C2: Page 5270, line 24. delete reference Boutin and Merlivat 2009: as opposed to Takahashi et al (e.g. CDIAC) or Watson et al., the data published in Boutin and Merlivat are not available.

R2: We have elected not to delete the reference, since the availability of data (to the larger community, via CDIAC for example) is not a prerequisite for the citation of a publication. We have instead rephrased the sentence so that the emphasis is on the increase in the measurement of surface ocean pCO2 in the open ocean, and not on the increased access to data.

C3: Page 5271, line 5: As opposed to all other references, Etcheto et al did not investigate the north atlantic. Suggestion here, add reference to Ullman et al 2009 (see below).

R3: The Etcheto et al. reference has been removed, and the Ullman et al., reference has been included as suggested.

C4: In historical data (e.g. 2003, 2006 2007), several cruises show pCO2 well below 300 _atm in this region whereas the extrapolation presented in this analysis leads to spring values much higher. Would it be possible that the data (Carioca) used to evaluate the model (Eq 1) were conducted during an anomalous year, implying high pCO2 values extrapolated in spring during the decade 1999-2008. This is an important point that should be discussed regarding all other results, processes analysis, variability of the fluxes (Figure 3c), reconstruction of pCO2 in different locations (figure 4) and pCO2 trends (Figure 10).

R4: We agree that there are historical data showing the (spring) pCO2 below 300 μatm in the region. Data from the CARINA database have now been included in the validation of the model (Figs. 6 and 7 and Table 3 in the revised manuscript).

With regard to the suggestion that the CARIOCA data indicate anomalously high spring values, the hourly CARIOCA data indicate values below 300 μatm during the spring

bloom in both April 2007 and April 2008 (see the figure below, and Fig. 3b in red), and we do not believe that these data were collected in anomalous years. However, the spring bloom on the Scotian Shelf lasts roughly 2 weeks (see hourly data in blue in the figure below), and the averaging of the hourly CARIOCA data to the monthly time scale results in an average April value of 275 μatm in 2007, and 335 μatm in 2008 (shown in Fig. 3b in red, and in the figure below). The model uses monthly mean values of Chl-a and SST that have also been averaged spatially within a $2^\circ \times 2^\circ$ grid box. The resulting pCO2 is therefore represents a monthly and spatially averaged quantity, and the minimum spring bloom value is overestimated compared to much shorter term observations, including those made by the CARIOCA buoy, and the underway pCO2 measurements used in the validation (see Fig. 4). However, there is good agreement between the modeled and observed pCO2 in the summer and autumn, and the seasonal cycle is well reproduced by the model. This discrepancy between short-term observations and monthly means, and the overestimate of the (spring) pCO2 minimum is discussed in Section 2.1 (see p. 5275 lines 9-29 and p. 5276 lines 1-3).

C5: As usual, in such a publication that present new observations (depending the status of the paper in revision to Mar Chem), it is recommended to specify where the data are available (CDIAC ?, other Data center ?)

R5: The authors certainly appreciate that Biogeosciences promotes the full availability of data sets corresponding to the papers it publishes. The work presented here is part of a Canadian Foundation for Climate and Atmospheric Sciences (CFCAS) project, and is therefore subject to the regulations agreed upon by all project participants. The data will become available according to the applicable CFCAS regulations. In the meantime, we would, of course, welcome requests from individual researchers with a particular interest in this work.

C6: Table 2: specify unit of parameters

R6: The units of parameters in Table 2 have now been specified as suggested.

C7: Table 3: check spelling, ... containg, moorning, gassing, gird....

R7: Spelling and grammatical errors have been corrected in the text.

C8: Table3: fluxes are presented in molC/m2/yr; is it usefull to specify the area of box 1 and Scotian shelf region ?

R8: The area of grid box 1 (containing the CARIOCA mooring) and of the region are stated in the Figure caption to give the reader an idea of the range of the spatial extrapolation, and the size of the study area. We do feel that this is useful and this text remains in the figure caption.

C9: Figure 1: adding tracks of the cruises used to validate the extrapolation?

R9: We have elected not to include the cruise tracks on the figures. This addition complicates the figure, particularly as several cruises within the same small area were used in the validation, and does not add much to the discussion. We have shown the validation for each grid box individually, and have now split this into two figures so that the time series can been seen more clearly. Furthermore, we have referred the reader to the map for the location of the grid boxes, which we feel is adequate for the understanding of the validation of the spatial extrapolation.

C10: Figure 6: legend: specify the region investigated in this plot.

R10: This plot refers to grid box 1 (containing the CARIOCA mooring) and this information has been added to the figure caption as suggested.

C11: Figure 10: legend: specify the region investigated in this plot

R11: This plot refers to grid box 1 (containing the CARIOCA mooring) and this information has been added to the figure caption as suggested.

C12: Figure 10: legend: specify the year used as a reference to evaluate anomalies.

R12: The pCO2 and SST anomalies were defined as the deviation from the mean

seasonal cycle over the 10-year period (i.e. mean of all January values, mean of all February values, etc.), and not relative to a specific year (see p. 5274, lines 19-22)

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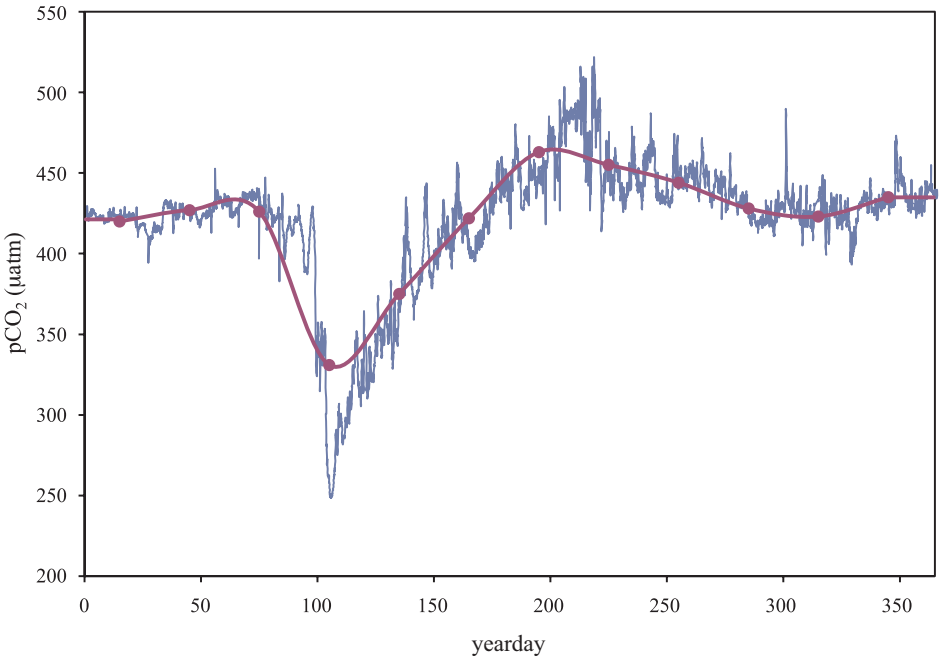

Fig. 1. Measurements of pCO2 from the CARIOCA buoy in 2008. Hourly data are shown in blue, and the monthly mean values shown in red.