

Interactive  
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***Interactive comment on “Satellite observations reveal high variability and a decreasing trend in CO<sub>2</sub> fluxes on the Scotian Shelf” by E. H. Shadwick et al.***

**Anonymous Referee #1**

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The authors present important new results of in situ pCO<sub>2</sub> measurements on the Scotian shelf and combine these with satellite data. The article contains important estimates of regional and temporal variation in the pCO<sub>2</sub> air-sea difference and CO<sub>2</sub> air-sea fluxes. The methods used are sound, but insufficient detail is provided in the description of some of the methods, the results and figure captions. It is a pity that the effort of creating the CO<sub>2</sub> air-sea flux maps is not accompanied by an in depth discussion of the spatial and temporal variation in pCO<sub>2</sub> and CO<sub>2</sub> fluxes and the processes governing these on the Scotian shelf.

The article is acceptable for publication pending many, many minor to moderate cor-

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Interactive Discussion

Discussion Paper



rections. In addition, I strongly recommend strengthening and expanding the scientific interpretation of the results (pages 10-11) in order to make the text more scientifically attractive and novel.

### Main comments

The manuscript contains a long, but not much in depth, discussion on the seasonal variation of surface water pCO<sub>2</sub> 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.

The article would benefit from a more in depth discussion of the spatial and interannual variation in surface water pCO<sub>2</sub> on the shelf and of why CO<sub>2</sub> fluxes here differ from those elsewhere, as touched on on pages 10-11. This is where the article gets really interesting and can move science forward. The link to the NAO is mentioned without any analysis for such a link between the NAO, SST, pCO<sub>2</sub> and CO<sub>2</sub> air-sea fluxes.

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 pCO<sub>2</sub>. 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.

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 CO<sub>2</sub> fluxes in the East China Sea (CO<sub>2</sub> sink) relative to the Scotian Shelf (CO<sub>2</sub> source) result from the position relative to

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7, C2110–C2118, 2010

Interactive  
Comment

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Interactive Discussion

Discussion Paper



the northward flowing boundary current and the southward flowing current (Labrador Sea Current). Could the CO<sub>2</sub> outgassing on the Scotian Shelf result from the net warming of Labrador Sea water as it flows south or are other processes responsible?

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 pCO<sub>2</sub> 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.

On the pCO<sub>2</sub> fit (equation 1, line 76, lines 170-205):  
• Line 76. It is not clear whether the three terms in the pCO<sub>2</sub> 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 pCO<sub>2</sub> estimates?  
• The errors in the pCO<sub>2</sub> fit are not discussed, when the equation is presented (line 76). The caption of figure 3 mentions an error of 16  $\mu\text{atm}$ , but without an indication of how the error has been determined. An error of 16  $\mu\text{atm}$  is substantial and it is important to determine the effect of this error on the flux estimates. The uncertainty related to spatial extrapolation of the fit should be discussed. The application assumes that the CO<sub>2</sub> dynamics of the mooring site also apply in boxes 1-7. How realistic is this?  
• Equation 1 has a pCO<sub>2</sub> 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.  
• How well does equation 1 work for satellite data?  
• Figure 4: How well do the CARIOCA pCO<sub>2</sub> data match underway pCO<sub>2</sub> and calculated pCO<sub>2</sub> (equation 1)?

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

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

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 pCO<sub>2</sub> 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.

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)?

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 pCO<sub>2</sub> been observed or modelled? If total pCO<sub>2</sub> 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 pCO<sub>2</sub> 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).

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<sup>-1</sup> 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

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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.)

Minor comments The title: 'A decreasing trend in CO<sub>2</sub> fluxes' is vague, as it is unclear if the waters are an overall sink or source for CO<sub>2</sub>. The word 'trend' is superfluous. The title needs strengthening.

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.

Line 44. Will you make the pCO<sub>2</sub> observations public via CDIAC?

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

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 CO<sub>2</sub> air-sea flux estimates. Alternatively you might use the Sweeney et al. equation or the Nightingale et al. (2000) equation.

Line 98 and figure 1. How did you delimit the boxes near land?

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?

Line 102. Describe how the 9 km satellite chlorophyll and SST data were transferred to the 2° x 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?

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?

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

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

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

Line 115. Provide a description of the underway pCO<sub>2</sub> system or a reference an earlier publication for your system. Which of the systems in Körtzinger et al. is it? Where was it built? What type of equilibrator does it have? How did you calibrate the analyser? Etc.

Line 121. Which set of carbonate system equations did you use? How well do the shipboard pCO<sub>2</sub> data and the estimated pCO<sub>2</sub> data compare to the CARIOCA pCO<sub>2</sub> data?

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

Line 163. Add space in 'south of'.

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?

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?

Lines 186-188. ' to the areal ..... CO<sub>2</sub> inventory'. What is the statement based on? What does it mean? Remove?

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

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Line 192. Are the data for the mooring site, for grid box 1 or elsewhere?

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

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.

Line 224 and elsewhere. Figure 7 has little evidence of the gain in pCO<sub>2</sub> approaching 4.6  $\mu\text{atm}/\text{i}^{\circ}\text{C}$ ; 8  $\mu\text{atm}/\text{i}^{\circ}\text{C}$  is more realistic. Your statement comes across as wishful thinking.

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<sup>-1</sup> and the similar gains for 0.04 hr<sup>-1</sup> and 0.02 hr<sup>-1</sup>, 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.

Line 258. Why is the spring and summer pCO<sub>2</sub> drawdown not resolved by the chlorophyll record? This seems bizarre. The chlorophyll record has large peaks in ~April. Is there a subsurface chlorophyll maximum?

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

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

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 pCO<sub>2</sub>, chlorophyll and

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7, C2110–C2118, 2010

Interactive  
Comment

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Printer-friendly Version

Interactive Discussion

Discussion Paper



SST for the grid boxes.

Line 324. Clarify or remove 'freezing point' (of freshwater, 0°C??). At present the term is confusing, given the lower freezing point of seawater.

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

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

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

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

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

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

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/m<sup>2</sup>/yr in the table, but 0.01 mol C/m<sup>2</sup>/yr in the text. Correct 'gird' to 'grid'.

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

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Interactive Discussion

Discussion Paper





Scotian shelf part of or a neighbour of the Grand Banks?

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 ship board chlorophyll in Fig. 2a is confusing, as it breaks the description of the other parameters.

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)?

Figure 6. Are the values for the mooring site or for grid box 1? Did you calculate or observe the total pCO<sub>2</sub> 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).

Figure 7. Briefly explain pCO<sub>2</sub>\* and SST\* in the caption and refer to the relevant section in the text (equation 9). The insets are rather small. 0.02 hr<sup>-1</sup> corresponds to 50 hours (or roughly 48 hours).

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.

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7, C2110–C2118, 2010

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Interactive Discussion

Discussion Paper

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