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Biogeosciences Discussions

Interactive comment on "Distributions of surface water CO_2 and air-sea flux of CO_2 in coastal regions of the Canadian Beaufort Sea in late summer" *by* A. Murata et al.

Anonymous Referee #2

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Comments on “Distributions of surface water CO2 and air-sea flux of CO2 in coastal regions of the Canadian Beaufort Sea in late summer” by Murata et al.

Overall comment:

The manuscript reports a valuable data set of pCO2, TCO2, and the calculated TAlk in late summer in the Canadian Beaufort Sea, and presents the sea-to-air CO2 flux in the study area. It is nice to the new data set in a marginal sea of high latitude, where becomes more and more important on estimating the global oceanic CO2 uptake. Overall, the data quality is good, but I feel that the data analysis could be pushed further. My editorial corrections/suggestions and some concerns on data interpretation are detailed in the following.

Specific comments:

Abstract

(1) Lines 7-9: “…, which is the driving force of the air-sea exchange of CO2 and is calculated from differences in pCO2 between the sea surface and the overlying air,…” This statement is too detailed for an Abstract. I suggest deleting it.

(2) In line 12, negative values of flux (-15.0 and -16.8) indicate a sink status, but a positive value of flux (10.2ś7.7) in line 16 indicate a sink status, too. This would make the readers get confused. I suggest using only one way to express CO2 flux throughout the manuscript.

Introduction

p. 5095, lines 1-3: About the methods for estimating the uptake of anthropogenic CO2 in the ocean, the back calculation method pioneered by Brewer (1978) and Chen and Millero (1979) should be noted.

Brewer, P. G., Direct observation of the oceanic CO2 increase, Geophys. Res. Lett., 5, 997– 1000, 1978.

Chen, C.-T. A., and F. J. Millero, Gradual increase of oceanic CO2, Nature, 277, 205– 206, 1979.

2.1 Data

(1) p. 5096, lines 22-23: In addition to SST, salinity data are also needed to calculate the saturated water vapor pressure after Weiss and Price (1980).

2.2 Calculation of Talk

(1) p. 5097, lines 17-18: “within ś30 min of hours” and “TAlk

BGD

5, S3021–S3026, 2009

(μmol kg-1) on hours” are unclear, please rephrase.

(2) p. 5097, line 18: Delete (μmol kg-1)

(3) p. 5097, line 20: Delete equation (1)

(4) Merge the two sentences below into one.

“TAlk (μmol kg-1) on hours was calculated from…” in p. 5097, lines 18-22 and “The actual calculations were done using…” in p. 5098, lines 1-2.→ “TAlk was calculated from pCO2, TCO2, SST, and SSS by using the program CO2SYS of Lewis and Wallace (1998).

(5) p. 5098, line 6: “The data set for pCO2, TCO2, and TAlk was used examine” → “The data set of pCO2, TCO2, and TAlk was used to examine”

2.3 Calculation of the air-sea flux of CO2

(1) p. 5098, line 11: the transfer velocity → the gas transfer velocity

(2) p. 5098, line 24: w10 → u10

(3) Provide all the formulas used to calculate k or delete the formulation of Wanninkhof (1992)

(4) Try to replace the instantaneous shipboard wind speed data (Were they measured at 10m height?) by daily mean wind speed data, e.g. QuikSCAT satellite data (http://www.remss.com/qscat/qscat_browse.html), to calculate k.

3.1 Distributions of surface water pCO2 and related properties

(1) p. 5099, line 21: Fig. 2b and d → Figs. 2b and d (here and elsewhere in the manuscript, e.g. p. 5100, line 4).

(2) p. 5099, lines 21-26: The plots of pCO2 vs. TCO2, SST, and SSS would be a

BGD

5, S3021–S3026, 2009

Interactive Comment

simpler and more useful approach to demonstrate these correlations.

(3) p. 5100, lines 16-17: Please rephrase because pCO2 itself can not be undersaturated only a water mass can be undersaturated with respect to fCO2.

3.2 Air-sea exchanges of CO2

(1) p. 5100, lines 20-21: Delete “the driving force…from the former)”, since fCO2 has been defined in Section 2.3.

(2) p. 5100, lines 25-26: The time-series plot of pCO2 (i.e. pCO2 vs. sequential day) would be helpful to show the result.

(3) p. 5101, line 4: Delete “in the”.

(4) p. 5101, lines 12-13: I would suggest revise the sentence “In summary, the area overall acted as a moderate sink for atmospheric CO2, although sea-to-air CO2 fluxes were observed locally.” → “In summary, the study area overall acted as a moderate sink for atmospheric CO2 in September 2000 and 2002.”

4.1 Biological processes

The arguments made by the authors in this section could be problematic for the reasons below:

(1) The observed phenomenon that “nTCO2 in the surface layer … the TML in the study area” (p. 5102, line 22-24) is most likely resulted from the artificial variance created by the method of normalization used. As discussed in Friis et al. (2003), the traditional normalization concept, i.e. transformation of a measured TCO2 to some reference salinity based on the measured salinity, could be over-corrected, mainly due to the ignorance of the influence from reverie input. This potentially artificial effect would be serious in this study, since the study area was in a coastal region where salinity varies in a wide range. I suggest re-calculating the nTCO2 by using the

BGD

5, S3021–S3026, 2009

Interactive Comment

approach proposed by Frris et al. (2003).

Friis, K., A. Körtzinger, and D.W.R. Wallace (2003), The salinity normalization of marine inorganic carbon chemistry data, Geophys. Res. Lett., 30, 1085, doi:10.1029/2002GL015898.

(2) TCO2/TAlk ratio is generally higher in fresher water than that in seawater. Therefore, the physical mixing effect may play a much more important role than the biological effect to result in the observed negative correlation between TCO2/TA ratio and salinity.

4.2 Water mixing

I have some concerns on the discussions in this section:

(1) Based on the T/S diagram, I agree that two mixing types can generally be found in the study area. However, I was not convinced that type B mixing was associated to ice melting, because ice melting not only lead to decrease in temperature but also in salinity. Therefore, more evidences should be provided to support this argument.

(2) To account for two mixing types, at least three end-members are needed. However, only two end-members were identified in this study.

(3) The changes of temperature, salinity, TCO2, and TAlk were calculated to be capable of contributing 19, 18, -80, and 35 atm, respectively, to pCO2 variation (p. 5105, lines 6-8). If the study area is a physical-mixing dominant system, the contribution of changes in TCO2 and TAlk to the pCO2 variation would be very close. However, the calculated result showed that pCO2 variation caused by TCO2 change is much larger than TAlk, suggesting that a significant role played by biological effect, which did not agree the conclusion of Section 4.1.

4.3.1 Differences between 2000 and 2002

I suggest using additional wind speed data set to do this analysis, because shipboard wind speed represents an instantaneous condition that may be very different from the averaged condition.

4.3.2 Differences between the areas east and west of 150°W

I suggest that the comparison of pCO2 between the areas east and west of 150°W should also be made, which would be easier to make a meaningful comparison, because CO2 flux depends on wind speed, too.

4.3.3 Air-to-sea CO2 fluxes and comparison with previous studies As suggestion I made for 4.3.2, pCO2 should also be compared between the present study and the previous studies, since the choice of wind speed data set would significantly affect the calculated result of CO2 flux.

4.4 Capacity of the Beaufort Sea as a CO2 sink

I suggest that the authors calculate the buffer factor (Revelle factor) directly by using the program CO2SYS of Lewis and Wallace (1998), which was used to compute TAlk from TCO2 and pCO2 by the authors, rather than using ln(pCO2) vs. ln(nTCO2) plot, because the method of normalization used could be problematic as mentioned earlier.

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BGD

5, S3021–S3026, 2009

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