Interactive comment on “Air-sea CO₂ fluxes on the Bering Sea shelf” by N. R. Bates et al.

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Response

We would like to thank the anonymous reviewer for very helpful and thoughtful comments that have improved the paper. In the revised paper, we have addressed all the concerns of the reviewer. In this response, we have interspersed our responses to reviewer comments below (in blue, Helvetica 11 font in the supplemental file) and revised the paper accordingly. In the online version of our response, we have added the RC3 to denote referee comment and AR response. Please see attached pdf for formatted response.

RC3. General Comments. This manuscript reported new surface seawater CO₂ mea-
measurements obtained from two BEST cruises in the Bering Sea, with the aims to deriving annual CO2 flux and identifying controlling factors that are responsible for observed CO2 source/sink on this shelf region. The new data can also help to establish baseline conditions of the carbonate chemistry in the Bering Sea. The paper was well structured, and the authors thoroughly reviewed previously reported work. This work is much anticipated in a long time for this important shelf region. The authors thoroughly discussed biological factors that shape the CO2 fluxes in the Bering Sea, and highlighted the CO2 sink in the ‘green belt’. The authors conclude that the Bering Sea serves as a significant CO2 sink to the atmosphere on an annual basis.

I have two concerns on the authors’ data analyses and interpretation:

RC3. (1) Apparently there are only two research cruises have been conducted, and the obtained CO2 data are probably only representative for two seasons. There seems to be no underway pCO2 measurements, and all CO2 data were derived from DIC/TA bottle measurements. This affects the spatial coverage of CO2 measurements. Given this limitation and that the coastal ocean is highly heterogeneous in space and time, annual CO2 flux derived from limited spatial and temporal coverage is less confident. The annual CO2 flux of 157 Tg C is a high value. I think this may be partially due to biased CO2 sink towards to summer, when the data were collected. There have been lessons in the past in other shelf area. Interpolating CO2 data using MLR may have estimable errors, but just looking in Fig. 4, the errors are not small. Extrapolating data is even more risky due to potential large errors from limited temporal and spatial coverage. Therefore, cautions should be taken to make any conclusion concerning annual CO2 flux. AR. We have incorporated additional statements in the revised paper to clarify the points raised by the reviewer. The reviewer is correct that pCO2 is calculated from TA and DIC, and in the revised paper, we have replaced “observed” with “calculated”. The flux value is high driven by the low seawater pCO2 conditions that develop during the sea-ice free periods of spring and summer. Like other sea-ice covered shelves, there’s an asymmetry in flux since sea-ice cover dampens/blocks gas
exchange. There’s currently a debate about whether there is significant gas exchange through sea-ice. In the revised paper we restricted the MLR analysis in this paper to the shelf areas (<200 m deep), and did not report results of the MLR for the open-ocean areas of the Bering Sea. The MLR approach has been used for water-column and mixed-layer studies. The MLR fits below the mixed layer tend to have smaller standard deviations and for example, have been used for GLODAP climatology, climatologies of Goyet et al., and often for crossover analysis for comparisons of data from different cruises. The MLR fits for the surface/mixed layer have larger standard deviations and used by Lee et al., 2002, Bates et al., 2006, for example. The MLR fits should improve with more data that hopefully captures all the physical and biological processes that influence inorganic carbon cycle variability. In addition to comparing the 2008 BEST data with the Takahashi et al., 2008 climatology, the MLR approach offers an additional way to compare to the BEST dataset.

RC3. (2) Partitioning total CO2 change into temperature and biology changes (eq. 6) is too simple and too coarse for a coastal region. We just do not have much confidence to say how much of this ‘biology’ term is really due to biological activities. Physical processes, such as mixing and advection, all affect pCO2 signal, and yet these processes are all lumped into ‘biology’. This seems to be inappropriate for coastal ocean. What are the assumptions in such partitioning? A more thorough discussion of these assumptions should be presented. This method may be OK for open-ocean condition, but it is hardly the case in coastal ocean. It may be useful to use other data (such as O2 and nutrients) to derive this biology term, and separate it from the total CO2 change. Before the authors have a more concrete handle on the biological term, it may be pre-mature to say biology is a dominant term, even though I think this may still be the case at the end. AR. We believe this approach is still an appropriate way to try to decipher the relative importance of processes in an empirical and simple way. In the revised paper, we have added a few additional statements to clarify these points and underlying assumptions (and the caveats to this approach). Yes, the “biology” term will incorporate vertical mixing and entrainment, and advection. However, the
Spring-summer period is mostly a period of detrainment (without upward flux of CO2), vertical diffusion rate are a very minor component to the term, and the advection term influences a small nest of $1^\circ \times 1^\circ$ grid boxes between spring and summer (so there's “continuity” between boxes. Those areas we show large “biology” terms do coincide with those regions exhibiting high rates of NCP from spring to summer (calculated from DIC and O2) that are reported in a companion paper (Mathis et al., 2010).

RC3. Specific Comments

RC3. (1) P7278, line 7, ‘...four measurable carbonate system parameters...’: There are more than four parameters measurable now, e.g. CO32- can be directly measured. AR. We have modified the statement in the revised paper.

RC3. (2) P7279, line 9, ‘. . .using two approaches. . .’: Sounds like ‘two steps’. AR. We have modified the statement in the revised paper that it is clear there are two different methods.

RC3. (3) P7279, line 11, ‘using interpolation. . .’ showed twice here. AR. We have corrected the typo in the revised paper.

RC3. (4) P7279, line 22, ‘Windspeed data and $\Delta pCO2$ values (Fig. 4). . .’: Fig. 4 does not show $\Delta pCO2$ values, please explain. AR. This is corrected in the revised paper.

RC3. (5) P7280, line 1-2: NNR data model has a resolution of $2.5^\circ \times 2.5^\circ$, but pCO2 has a resolution of $1^\circ \times 1^\circ$, is this a mismatch for CO2 flux calculation? AR. This is not a mismatch of the datasets but the point is clarified in the revised paper. In Fig 6, fluxes are calculated for each $1^\circ \times 1^\circ$ box using the NNR data that those areas fall within. Where $1^\circ \times 1^\circ$ box overlap NNR fields, an average of both is used. In Fig. 7, fluxes are calculated for each station using windspeed datasets from NNR that spans each respective area on the shelf.

RC3. (6) P7281, eq. 3 and 4: Since different processes control DIC and TA distributions (decoupled controlling) and it also involve different seasons, it seems to be difficult to
believe that DIC and TA have similar parameterizations (different coefficients but same factors) in these equations. Are there special considerations that DIC and TA equations are similar in structure? AR. The best fits for the MLR for both DIC and TA are a function of the parameters listed.

RC3. (7) P7282-7283, error analysis: The simulation gives an error estimate of interpolation, which may be much less than error from extrapolation. Any thought on what error of extrapolation might be? I think this limits the confidence for the CO2 flux estimate. AR. In the revised paper, there are a couple of additional statements about potential extrapolation errors.

RC3. (8) P7284, 2nd paragraph: It seems to be a dilemma that the authors say that 2008 is not a typical year when discussing the difference between observed and modeled pCO2, yet they are using 2008 data to interpolate and extrapolate to obtain the model. Any explanation? AR. In the revised paper, we have clarified the point. The MLR extrapolation is based on World Ocean Atlas climatology data for T,S, etc. In 2008, the observed physical variability was different to the WOA climatology.

RC3. (9) P7286, Fig. 6: Is the sea-ice condition considered in spring CO2 flux calculation? If sea-ice occupied large area in spring, CO2 flux then should be much less in Fig. 6? AR. Yes. In the revised paper, we have clarified the point and flux estimates.

RC3. (10) P7286, line 10-11: The authors say river run-off areas ‘tend to’ have high pCO2 value, but many river plumes show strong CO2 sinks. AR. Yes, this is correct. However, for Arctic rivers (Salisbury et al., 2008) rivers tend to have high pCO2, and Mathis et al., (in press) also report high pCO2 for the Yukon River.

RC3. (11) P7287, 1st paragraph: there is bit confusion and overlapping on the terms used to describe the factors controlling CO2 exchange. For example, primary production is included in net autotrophy/heterotrophy, so why put them together; what is export production? AR. This is clarified in the revised paper.
RC3. (12) P7291, line 15: ‘. . .we conclude. . .annual CO2 sink. . . 3.4 Tg. . .’ I thought this is not the author’s conclusion, but by Walsh and Dieterle (1994). Misunderstanding? AR. This is clarified in the revised paper.

RC3. (13) P7292, line 22: Can not say NCP dominates. . .drawdown of CO2, since no NCP is reported here (reference?), plus there may be other processes in ‘biology’ term that are really physical processes. AR. NCP estimates were calculated in a companion paper (Mathis et al., 2010). In the revised paper, we have added the references and clarified where the NCP estimates are derived from.

RC3. (14) Table 1. Should be Takahashi et al. (2009), not (2002)? AR. Yes, this is corrected in the revised paper.

RC3. (15) Fig.2. What is the atmospheric mixing ratio? AR. This term is clarified in the revised paper.

RC3. (16) Fig.3. There is no blue line in the bottom plot, but it is mentioned in the caption. AR. The plot is corrected in the revised paper.

RC3. (17) Fig. 6. Use uniform color scales for comparison between seasons. AR. The figure is corrected in the revised paper.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C4427/2010/bgd-7-C4427-2010-supplement.pdf

Interactive comment on Biogeosciences Discuss., 7, 7271, 2010.