

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-

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surements 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

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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° x 1° 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 ΔpCO₂ values (Fig. 4). . .': Fig. 4 does not show ΔpCO₂ 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° by 2.5°, but pCO₂ has a resolution of 1° by 1°, 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° x 1° box using the NNR data that those areas fall within. Where 1° x 1° 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

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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.

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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.

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