

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

Anonymous Referee #3

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General Comments

This manuscript reported new surface seawater CO₂ 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:

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(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.

(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.

Specific Comments

(1) P7278, line 7, ‘...four measurable carbonate system parameters...’: There are more than four parameters measurable now, e.g. CO₃²⁻ can be directly measured.

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- (2) P7279, line 9, '...using two approaches...': Sounds like 'two steps'.
- (3) P7279, line 11, 'using interpolation...' showed twice here.
- (4) P7279, line 22, 'Windspeed data and dpCO₂ values (Fig. 4)...': Fig. 4 does not show dpCO₂ values, please explain.
- (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?
- (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?
- (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.
- (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?
- (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?
- (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.
- (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

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production?

- (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). Mis-understanding?
- (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.
- (14) Table 1. Should be Takahashi et al. (2009), not (2002)?
- (15) Fig.2. What is the atmospheric mixing ratio?
- (16) Fig.3. There is no blue line in the bottom plot, but it is mentioned in the caption.
- (17) Fig. 6. Use uniform color scales for comparison between seasons.

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