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Biogeosciences Discussions

Interactive comment on "Air-sea CO_2 fluxes on the Bering Sea shelf" *by* N. R. Bates et al.

Anonymous Referee #4

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General comments: The paper presents surface water pCO2 distribution on the Bering Sea shelf calculated from measured TA and DIC collected in the spring and summer of 2008. The surface water pCO2 fields are then also extended into the Bering Sea by the use of MLR. The annual air-sea flux of CO2 is calculated and compared to previous results. The authors conclude that the Bering Sea shelf is a stronger sink for CO2 than shown previously. The strong sink during summer is attributed to a large biological drawdown over much of the shelf, only partly countered by warming. The paper is well written and structured and adds to the limited amount of studies of surface water pCO2 and CO2 fluxes on the Bering Sea shelf. However, some issues needs to be addressed before publication.

Specific comments:

(1) I am missing a discussion on the importance of surface water circulation and mixing

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of different water masses in the studied area with regards to surface pCO2. How far out does the shelf water region extend? Also, it is not quite clear what the importance of the riverine input is. Is it insignificant as the authors use salinity normalised TA in the discussion?

(2) Questions regarding the MLRs: The observed pCO2 values showed a span of 180-520 uatm and the MLR model maps showed 350-450 uatm. I wonder if there was an investigation on the more “extreme” pCO2 values that were not captured by the MLRs. Did they belong to a specific region or time, could they be linked to a specific process not captured with the parameters in the MLR? Could you see an “extreme” value in any of the supporting parameters in these cases? Do you know if it was the calculated TA or calculated DIC that “missed”? Are you reporting the R-sq or the adjusted R-sq for the MLR-results? Were they similar? Did you use your MLR equations on the Takahashi data set to compare the results? How far off the shelf are you confident that your approach works? You state that there is an associated unique pCO2 error for every point; were there any particular regions that seemed to work better/worse?

(3) When talking about sinks, any thoughts about where this carbon is likely to end up? Burial in the sediments, off-shelf transport, outgassing.. which would be the dominant process?

Technical comments:

Repeated text on line 10-11 on p. 7279

Table 1: In the header it reads: Bering Sea annual flux flux

Fig 3. The figure text states that the original hourly wind data is shown in blue in each plot; however in the bottom plot it is red.

Fig. 4. The red text in the plot is somewhat blurry; it would be nice to have a different colour and sharper (if it is necessary at all, since it is mentioned in the figure text).

Fig. 5 and Fig 7. Larger text on the axes of the plots would be an improvement. The

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Interactive Comment

ice-% captions in figure 7 definitely needs to be larger. For figure 5, perhaps it isn't necessary to repeat "at each hydrocast station during. . ."?

Fig. 12 The figure text is really, really long. . . One suggestion is to remove the non-corrected comparison plots and instead plot the results for the different areas in separate plots (and then the areas would not need to be mentioned in the figure text, but are stated inside each plot).

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