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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 #1

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General comments: Bates and co-workers deal with distributions of pCO2, ΔpCO2 and air-sea fluxes of CO2 in the Bering Sea shelf from data obtained by observations in 2008 and subsequent data analyses. The study area is one of the areas, for which few studies on oceanic carbonate system have been made so far. In this sense, I believe that this study can contribute to adding new information on coastal CO2 budget. However, I found some points, which should be revised and re-considered. After the revision, this manuscript would be suitable for publication in Biogeosciences.

Specific comments: (1) The authors cite Murata and Takizawa (2002) wrongly. At line 19 of page 7273, it is stated that pCO2 observations by Murata and Takizawa (2002) were made outside the Bering Sea. But their study was made for the eastern Bering Sea shelf, as found from the title. Cite the reference in a more appropriate manner. The same thing at line 26 on page 7290. I recommend to refer to a paper: Murata (2006), Global Biogeochemical Cycles, GB4006, doi:10.1029/2005GB002615.

(2) In the manuscript, a word "observed pCO2" is used. But actually, it is calculated from DIC and TA. In the CO2 community, seawater pCO2 is usually observed directly with instruments. Thus using the word "observed pCO2" reminds us direct observations. Use "calculated pCO2" or others.

(3) The authors point out a drawback of Takahashi et al. (2009), because Takahashi et al. bases on data from only one cruise. But the authors also use data only from one year. Do they have representativity enough? Furthermore, the authors attempt to have a climatological view of pCO2 conditions using a MLR method. But the MLR is constructed from the one-year data. With this, is it possible to have a climatological view? Data from 500 m deep seem to be too deep for estimating properties on shallow shelf waters.

(4) At line 22 on page 7288, that sea-ice melt and river runoff were minor is stated. However, salinities smaller than 33.0 are usually observed in this region, which are probably caused by mixing between open-ocean waters and river discharge/ice melting waters. River discharge impacts carbonate system properties in a coastal region considerably, because it is rich in DIC and TA. Thus normalization by a constant salinity does not necessarily work well. In a seasonal march from spring to summer, it is likely that these are influential in CO2 system properties. Check this point.

Technical comments: (1) The authors cite the study of Mathis et al. (2010b), but I could not find the publication in AGU site. Check the reference.

(2) At line 11 on page 7286, "(Mathis et al., 2010)", Is this 2010a or 2010b?

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