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Anonymous Referee #1
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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 RC1 to denote referee comment and AR response.

RC1. General comments: Bates and co-workers deal with distributions of pCO_2, ΔpCO_2 and air-sea fluxes of CO_2 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 CO_2 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.

RC1. Specific comments: (1) The authors cite Murata and Takizawa (2002) wrongly. At line 19 of page 7273, it is stated that pCO_2 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.

AR. The reviewer is correct. We have revised the statement to include the eastern Bering Sea shelf work of Murata and Takizawa 2002 paper, and added the Murata (2006) paper to the text and references.

RC1. (2) In the manuscript, a word “observed pCO_2” is used. But actually, it is calculated from DIC and TA. In the CO_2 community, seawater pCO_2 is usually observed directly with instruments. Thus using the word “observed ppCO_2” reminds us direct observations. Use “calculated pCO_2” or others.

AR. We have corrected the revised paper to calculated pCO_2.

RC1. (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 pCO_2 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.

AR. There were a few similar comments by the reviewers on the details of the MLR method. In the revised paper, we have reinforced statements about representativity and the caveats underlying using the MLR approach to compare to observed data. For the revised paper, we could retain the results of the MLR for the open-ocean areas of the Bering Sea but felt that it was better to restrict the analysis in this paper to the shelf areas. The open-ocean Bering Sea is beyond the scope of this paper but we can undertake a separate comparison of the limited observed data versus the MLR model results/Takahashi climatology in a separate paper. The

MLR fits would likely improve with more data, but a long-time decadal time-series is needed to perhaps resolve natural variability imparted by PDO/ENSO/AO and secular change forcing.

RC1. (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 CO_2 system properties.

AR. This is correct. In a separate paper by Mathis et al., TA/DIC from Alaskan rivers is discussed and we observed minor influences of river runoff in the northern inner shelf. In the revised paper, we strengthen the caveat statements about using salinity normalization. D18-O measurements are particularly helpful in resolving contributions of seawater, sea-ice melt and river runoff (which we have undertaken before for the Chukchi Sea an Canada Basin) but unfortunately these measurements were not undertaken for these cruises.

RC1. 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?

AR. In the revised paper, we have corrected the reference list for this in press paper, and defined 2010a or b.