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Response

We would like to thank Dr. Cai for his 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 RC2 to denote referee comment and AR response.

RC2. General comments: As a result of climate change, the Bering Sea has experience rapid ecosystem and biogeochemical changes. There are several previous studies reporting air-sea CO_2 flux in part of the Bering Sea. This paper reports air-sea CO_2 flux based on 1) pCO_2 calculated from very high accuracy DIC and TA data collected in the East Bering Sea shelf in spring and summer 2008, and 2) the whole Bering Sea climatological pCO_2 based on Multiple Linear Regression (MRL) approach using the area-limited 2008 DIC and TA data. The paper also synthesizes previous works and made good comparison with them. This work is valuable in evaluating the air-sea CO_2 flux in this changing environment and will have a good impact on the research community. The paper is well-written and easy to follow. I will support the publication of this paper with some relatively easy modifications and if one critical point (MRL approach) can be clarified.

RC2. I am not totally convinced that the MRL approach used here produces reliable DIC in the open ocean areas, in particular in the western Bering Sea area. There are two issues. 1. From Fig. 4c, it seems there are large uncertainties in some of the summer DIC data although the overall std.dev is not bad. Is there a pattern in the deviation of prediction vs. observation (i.e., if the deviation has a random distribution)? What do these larger DIC deviations (often exceed 100 umol/kg) translate into pCO_2 errors (assuming TA does not have the same exact errors as seen in Fig.4d)? The authors should provide their MRL model parameters (in Table 2) for independent evaluation by others.

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 felt that it was better to restrict the analysis in this paper to the shelf areas, and not report results of the MLR for the open-ocean areas of the Bering Sea. 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 deviations between prediction and observation are random and do not appear to have a temporal or spatial pattern. In Fig 4c, some of the extreme deviations between MLR prediction and observation do have large pCO_2 errors (50 to +100 μatm), but with reinforced statements on the caveats of use, the MLR approach gives the "mean" or "average" view of DIC.TA, pCO_2 variability. As such, the Takahashi pCO_2 climatology, this and other MLR based studies cannot capture year-to-year variability and mesoscale variability. Thus, for example, the Takahashi pCO_2 climatology report the mean condition for each 4° x 5° box with large observed pCO_2 ranges (50 to +100 μatm) within the box. In the revised paper, we have added a Table 2 that report the MLR model parameters so that others readers can use the equations.

RC2. 2. The “training data” (i.e., data used to derive the parameters) are all from the Eastern shelf (shallower than 100-200 meters). The paper didn’t discuss whether water mass mixing in the other areas are similar to that of the western shelf. Fig. 4 doesn’t give me enough confidence that it will work outside the “training data area.” I also went to read the cited Lee papers and several other papers. I think MRL approach is used in a different way in other papers. It is used in open ocean scenarios in most other papers (and largely under the mixed layer depth). In the Lee et al. (2000), in particular, many different equations were developed for various surface waters in various regions. Are there other $p\mathrm{CO}_2$ (or DIC data) available for comparison at specific locations outside the eastern shelf (for example, the original data from the Takahashi database)?

AR. We agreed with the comments of the reviewer. 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. Unfortunately, we have not found any other high quality data available at present for comparison.

RC2. Also, a good explanation for the predicted summertime $p\mathrm{CO}_2$ as high as 550 μatm at 52N/175W has not been given in the paper. The current explanation that the climatology results and 2008 observations do not necessarily have to agree is probably not enough. Therefore, I feel the authors should clarify the above issue or reduce the scope of this paper to the data-based eastern shelf.

AR. We agree with the comments of the reviewer. 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.

RC2. In addition, using shipboard wind is not appropriate (see my specific comments).

AR. In the revised paper, we clarified the text to state that we used the NNR windspeeds and the 0.39 U/flux parameterizations. We retained Figure 3, to show the variability of windspeed in order to reinforce the caveat that the NNR are time and space averaged within the 2° x 2° grids, for example.

RC2. Finally, while the authors did a very good comparison of their results with earlier data, it will be preferable (though I am not sure if it is possible) to discuss or even speculate how much of the difference is due to climate (real) change and how much is due up-scaling or other technical issues.

AR. We agree with the comments of the reviewer. With one year of data, it would not be appropriate to speculate about how much the differences can be ascribed to climate change, natural year-to-year variability or for example, mesoscale variability.

RC2. Specific comments Refs: Where is the Goyet et al. (2000) reference (cited in p.7280, line 21)? Is Lee (2001, LO) really the reference the authors intended to cite or is Lee et al. (2000, GBC) the right one?

AR. In the revised paper, we cite several papers that use the MLR approach (including the two Lee papers).

RC2. Abstract: Maybe it’s more actual to modify the title to “. . .on the eastern Bering Sea shelf.”

Otherwise, the abstract ". . .the Bering Sea shelf which is the largest US coastal shelf sea" is not accurate as part of the Bering Sea (the west part) is not US. (this comment was made before I read the rest part).

AR. Yes, we clarified the paper. Most of the Bering Sea shelf area is within the US EEZ for example.

RC2. Considering the fact that two other papers (Mathis 2010 a and b) have already been published from the same dataset, the introduction part, in particular 2.1, can be shorter and more closely linked to pCO_2 and air-sea exchange of CO_2 gas. In equation 2, you used the coefficient (0.39), which is based on long-term average wind. Thus you need to modify this sentence: Here, gas transfer velocity-wind speed relationships for short- term and long-term wind conditions based on a quadratic (U2) dependency between wind speed and k (i.e., Wanninkhof, 1992) were used to determine air-sea CO_2 fluxes.

AR. In the revised paper, we reduced the introductory statements and modified the gas exchange statement.

RC2. p.7279, line 21, you said "Synoptic meteorological data (including windspeed) was collected from the USCGC Healy during the cruises (Fig. 3)." Shipboard wind speed should not be used. Rather monthly satellite wind should be used with equation 2. Alternatively, instantaneous mooring data should be used (then change 0.39 to 0.31). The reason is this. If point A and point B have the same pCO_2 and are 5 hours apart during the survey (though could be a small distance apart). In reality, wind speed at A and B are probably the same (at the same time), but the shipboard weather station may have a much high wind speed at point B than at A because of time difference. Thus your calculated CO_2 flux will be higher at B than at A, which is not reasonable. See Jiang et al. 2008 (JGR) on calculation of the coefficient, though using 0.39 with monthly mean wind is acceptable.

AR. In the original and revised paper we did not use the shipboard measurements for calculating air-sea CO_2 fluxes, and rather used the NNR and long-term 0.39 coefficient. We retained Figure 3 and added a statement to show the variability of windspeed in order to reinforce the caveat that the NNR are time and space averaged within the 2° x 2° grids, for example.

RC2. p.7276, The 2nd paragraph on ecosystem changes is just too long and can appear in any of the previous accompany paper. I do not see this information to be that closely relevant. Also some of this is already in the Introduction.

AR. We have revised the statement.

RC2. p.7288, equation 6. After correcting for temperature, one may assign the rest effect to biology. This might be a standard approach in open oceans, but how well it works in the coastal ocean is questionable. The authors talked about other possibilities earlier on, but when coming to this part, it seems all others disappeared. Some discussion of nearshore influences is warranted.

AR. In the revised paper, we have added statements on the other processes at play.

RC2. p.7291, line 26, says the Takahashi et al. (2002) up scaled flux is 36 TgC/yr, but Table 1 RC2. says 37. Fix it.

AR. In the revised paper, we have corrected the Table.