Review comments of “Air-sea CO_2 fluxes on the Bering Sea shelf” by Bates et al.

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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.

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.

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 pCO_2 (or DIC data) available for comparison at specific locations outside the eastern shelf (for example, the original data from the Takahashi database)?

Also, a good explanation for the predicted summertime pCO_2 as high as 550 uatm 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.

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

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.

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?

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).

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 (*U*2) dependency between wind speed and *k* (i.e., Wanninkhof, 1992) were used to determine air-sea CO_2 fluxes.

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.

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.

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.

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