Biogeosciences
Discussions

Interactive comment on "Air–sea CO$_2$ fluxes in the East China Sea based on multiple-year underway observations" *by* X.-H. Guo et al.

Anonymous Referee #1

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General comments

This paper reports an extensive set of seasonal underway pCO2 and ancillary data for the East China Sea for 24 cruises over 2006-2011. The authors use these data to constrain seasonal variability in pCO2 in five spatial zones and to calculate the total pCO2 sink for the East China Sea. This work is valuable, both in providing a robust estimate of the pCO2 sink for this large marginal sea in the context of its biogeochemistry and physical circulation, and in making a large contribution to the global set of pCO2 data for marginal seas.

The work was conducted using standard methods, and the conclusions are well supported by the data. The paper is also well written. I have only minor suggestions for improvement, mainly questions for clarification and suggestions for clearer presenta-

tion.

Specific comments and questions

p. 5133, line 16: "increasing trend" in air pCO2. Given the large seasonal variability and sporadic sampling, a five-year time series is not really long enough to demonstrate an increasing trend in atmospheric pCO2. The comparison with the Mauna Loa time series does draw the eye upward, but without it, it would be difficult to interpret a secular increase from these data. If I had only the red dots, I might draw a horizontal line through the data all the way to June 2009 and then another, higher one through the 2010 and 2011 data. This plot is not important for the rest of the interpretation in the paper, but perhaps it would be better to say that the data were "not inconsistent with the global increase in atmospheric pCO2," rather than that they showed an "increasing trend."

p.5134 lines 26-29. Why is the intraseasonal variation within Zone 1 so much smaller in fall than in the other seasons? Are there transitions within the other seasons that might have been sampled at different phases during different cruises, or is there predictable, domain-wide upwelling in autumn?

p.5137, line 2. Insert range of values observed by Zhai and Dai for comparison.

p.5137, lines 10-22. Leave most of these numbers in the tables and just report ranges in the text; it is hard to take in so many numbers in the middle of a paragraph. The values might be better presented as a bar graph.

p.5137, line 19: "more than twice the global average for ocean margins." Return to this point in the Discussion and explain (or speculate) why this sea takes up so much more CO2 than do other marginal seas.

p.5138, lines 3-13. Present your own new work first, before bringing in the work of other authors for context.

p.5138, lines17-24. Explain NpCO2 more clearly - its meaning, calculation and use.

p.5138, line 25 and Figures 8 and 9: "no trend with SST." It looks as if there might be a trend in some seasons, even if not for the dataset as a whole. This is worth exploring.

p.5141, lines13-14: "low in winter (2.1 fold), high in spring (2-3 fold)." These ratios are essentially the same. Why is one "low" and the other "high?"

p.5142-51243: anomalous results in October 2006 and December 2010. I think that the October 2006 results should be included in the average, since the anomalous values were the result of a natural bloom, which might reasonably be expected to occur again from time to time. In contrast, I agree with the authors' decision to exclude December 2010, since the anomalous values in that case likely resulted from a change in the timing of the winter cruise. That does call into question the use of 2010 as a reference year, however. Was December 2010 included in the calculation of the reference conditions?

p.5144, lines 8-12. Future work will probably not need to be so comprehensive as this study. Now that this study has illustrated the range of values and the degree of variability in different locations and at different seasons, future sampling could concentrate on those seasons and locations where the variability is greatest or the mechanisms controlling pCO2 the least understood. It would be worthwhile to say that here, instead of just saying that future work must reduce the error from undersampling.

Figure 1. Note non-linear depth scale for colour bar.

Figure 3. Darken zone boundary lines. They are hard to see, especially in the NW corner, where the colours change. Enlarge the whole figure to clarify labels.

Figure 4. Clarify the timescale. Add more month labels, or add vertical lines to mark every January 1 or in some other way make it easier for readers to associate the variations in SST and SSS with the month or season. Also, plot labels (A,B) are uppercase, while in the caption they are lowercase (a,b).

Figure 5. Same comment about the timescale as for Figure 4.

Figure 6. This is a very useful figure that captures both the mean and the variability. Explain in the caption why October 2006 and December 2010 are treated differently from the other seasons in the figure, and whether December 2010 was included in the calculation of the 2010 reference year.

Figures 8 and 9. See comments above about possible relationships with SSS and SST one season at a time.

Figure 10. It looks as if there could be a quantifiable relationship between NpCO2 and chl in zone 3 for all seasons at once. It seems odd that the chlorophyll is highest in fall in Zone 4 in one year. Is that value correct?

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