

Interactive comment on “Seasonal variations of sea – air CO₂ fluxes in the largest tropical marginal sea (South China Sea) based on multiple-year underway measurements” by W. Zhai et al.

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Response to Referee #1

General Comment:

Zhai et al present an analysis of underway pCO₂ measurements for the South China Sea (SCS) collected during 14 cruises over a 5-year period. This effort builds on some of the authors' previous work within sub-regions of the SCS, however, provides new information by incorporating more data and taking a holistic approach to studying the SCS. It is specifically interesting how the authors sub-divided the SCS into 4 physical-biogeochemical domains for the analysis of variability in the fluxes. The manuscript describes important sources of variability in surface water pCO₂ and sea-air CO₂ fluxes within each domain, with a final revised estimate of sea-air CO₂ exchange for the SCS. The methods are sound and generally well described, and the paper is well written. This contribution clearly fits within the scope of Biogeosciences, and should be published following discussion over some points I have laid out below. Best of luck.

[Response: We thank Dr. Wiley Evans for his positive comments.](#)

Specific Comments:

My most significant comment is that there are many data points shown in Figure 2 that do not fall in any of the specified domains. This is particularly the case for data shown in panels a, b, d, e, and h of that figure. Many of these measurements are adjacent to the coast, with some data from areas with very high surface seawater pCO₂. Is there a justification for leaving these data out of the analysis? The domains were introduced in the 3rd paragraph of the introduction (page 7034), and the physical-biogeochemical reasoning for selecting the 4 domains makes good sense. This was expanded on in the 3rd paragraph of the Study area section (page 7036). However it is not clear how the exact dimensions of the domains were determined. If Domain A was extended to the coastline, Domain B was extended west to Hainan Island, Domain C was extended south and west, and Domain D extended south, all the data shown in the Figure would be within a domain. In addition, extending Domain A to the coastline would include the high near shore pCO₂ data and perhaps affect the final estimates presented in the paper given that Domain A was determined to be nearly in equilibrium with the atmosphere. I think it needs to be more clearly defined in the text how the borders of each domain were chosen such that some data were excluded.

[Response: As a typical marginal sea, the South China Sea \(SCS\) proper encompasses a variety of physical-biogeochemical domains. In this study, we focus on the majority of the SCS proper \(i.e. the four selected domains\), where the spatial coverage of our observational data is reasonably satisfactory at a temporal resolution of seasonal levels. Off these four selected domains, however, other physical-biogeochemical processes may dominate. For example, summer coastal upwelling induced CO₂ dynamics must be considered in the southwestern part of Taiwan Strait, in the eastern](#)

coast off the Hainan Island, and in the eastern coast off Vietnam. Although we have touched these areas during several cruises (e.g. Zhai et al., 2009; Cao et al., 2011; Jo et al., 2012), the spatial-temporal coverage is insufficient for reliable estimates of the sea-air CO₂ flux. Therefore, we exclude those data-insufficient zones in our “climatological” flux estimation. In the modified MS, we have clarified this issue.

Discussion/Concluding Remarks sections: Perhaps there should be some comparison with the estimates from the earlier studies that were based on more temporally or spatially limited data (i.e. Zhai et al., 2005)? How does the new holistic estimate from the SCS differ from that reported by the previous spatially limited study, and improve our understanding of the SCS? Some of these points are mentioned in the Introduction, but could be moved to the Discussion section.

Response: Done. We thank Dr. Wiley Evans for the constructive suggestion.

Concluding Remarks: Last paragraph: rather than, or in addition to, making the comparison to efflux from the equatorial belt of the global ocean, perhaps the SCS flux should be compared to the sink term for the global coastal ocean cited in the first paragraph of the introduction? The newest estimate of coastal ocean sea-air CO₂ exchange by Dai et al., 2013 uses the value for the SCS reported by Zhai et al., 2005. How would using the new SCS value adjust the coastal ocean sink term, if at all? I very much like the last paragraph of the introduction, and feel that these points could be expanded a bit in the Discussion or Concluding Remarks sections.

Response: We thank Dr. Wiley Evans for the constructive suggestion. We have changed the MS accordingly. We find that the SCS proper accounts for 24% of the CO₂ emission rate of the total 19 CO₂-releasing ocean margins (Dai et al., 2013), although it represents only 7% of the surface area of global coastal oceans. Considering this new result of this study, the Dai et al. (2013) compilation value of the CO₂ sink in the global coastal ocean is slightly lowered by 5% and updated to 0.34 Pg C yr⁻¹.

The terms “air-sea” and “sea-air” are both used throughout the manuscript. I would suggest sticking to “sea-air” because this is the term used in Tables 3-6 and in Figure 6.

Response: Modified accordingly.

Technical Corrections:

Abstract Line 18: sources of atmospheric CO₂?

Response: Modified accordingly.

Abstract Line 23: to the atmosphere?

Response: Changed into “to the atmospheric CO₂”.

Introduction Page 7033 Line 23: remove the word “such”?

Response: Done.

Introduction Page 7034 Line 2: replace “observable” with observed?

Response: Done.

Introduction Page 7034 Line 24: replace “variabilities of” with variability in?

Response: Modified accordingly.

Introduction Page 7036 Line 6: pCO₂ defined here but previously introduced on Page 7034 Line 25.

Response: Corrected.

Introduction Page 7036 Line 7: perhaps just cite the range instead of the exponential?

Response: Changed into “following a simple exponential equation of ...”.

Introduction Page 7036 Line 21: See comment above about defining the domains. Expanding them to include the other data not represented in the domain would increase the percent of total SCS area.

Response: See our response above. In the modified MS, we compare the domain areas to the area of the SCS proper (approximately 2.5×10^6 km²), i.e. excluding the Gulf of Thailand and its adjacent shallow sea areas. So the four domains account for ~50% of the SCS proper.

Methods 3.1: The practical salinity scale is a conductivity ratio and therefore unit-less. PSU is not a unit.

Response: Agreed. All “psu” had been deleted from the modified manuscript.

Figure 2 is first introduced on page 7042, after Figures 3, 4 and 5 are introduced. Perhaps Figure 2 should be renamed Figure 5, and the other Figures moved up?

Response: In the modified MS, we introduce the data maps (Fig. 2) in the “Study area” (Section 2). And we mention them in the “Sampling and methods” (Section 3) to show our sampling sites.

Results 4.4 Page 7042 Lines 4 and 5: Perhaps this should read “increased along with decreasing latitude”?

Response: Agreed and modified accordingly.

Results 4.4 Page 7042 Line 20: Perhaps this should read “along with decreasing latitude”?

Response: Agreed and modified accordingly.

Results 4.4 Page 7043 Lines 17 to 28: the word “relatively” is repeated 6 times throughout these lines. Maybe use a different word or say explicitly what the value is relative to?

Response: Modified accordingly. Thank the reviewer for reminding us.

Results 4.5 Page 7044 Lines 9 and 10: Should the other studies be referenced? Where are these comparisons in the manuscript?

Response: Modified accordingly. Thank the reviewer for reminding us.

Results 4.5 Page 7044 Line 15: remove “variation in”.

Response: Modified accordingly.

Results 4.5 Page 7044 Line 16: Should this read “seen on the...”?

Response: We thank the reviewer and have modified it accordingly.

Results 4.5 Page 7044 Lines 22 to 24: Error and uncertainty is not the same thing. Using variation synonymously with uncertainty is also a bit confusing.

Response: Agreed. We have changed previous “variations/uncertainties” into “uncertainties”; and previous “uncertainty/variation” into “uncertainty”. The sentence has been changed into “these represent the two largest sources of uncertainties in estimating regional sea – air CO₂ fluxes, i.e. the uncertainty introduced by pCO₂ interpolation and/or extrapolation and the uncertainty introduced by environmental forcing parameters such as wind”.

Results 4.5 Page 7045 Lines 13 to 18: Perhaps add each annual value to the table legend for each respective domain?

Response: Modified accordingly.

Discussion 5.1 Page 7049 Line 14: replace “driven” with “that drove”?

Response: This sentence has been changed into “this winter sea-air CO₂ exchange was subject to large variability due to rapid CO₂ degassing and subsequent sea surface primary production driven by upwelled nutrients from the subsurface waters”.

Discussion 5.2 Page 7050 Line 3: insert the between “of above-”.

Response: Done.

Discussion 5.2 Page 7050 Line 3: I'm not sure climatological is the correct word here. Perhaps "typical" would be better? In general, the point made in this paragraph is very interesting.

Response: Agree. We change the "climatological" into "typical".

Concluding Remarks: Perhaps shift the last paragraph up to precede the first paragraph?

Response: In response to review#2, this paragraph has been deleted.

Concluding Remarks: The statement on Lines 11 through 15 is a bit anticlimactic.

Response: We have changed the final statement into "We demonstrated once more that the SCS as a whole serves as a weak to moderate source of atmospheric CO₂ (Zhai et al., 2005a; Chen et al., 2006; Dai et al., 2013), although the *p*CO₂ variability in the SCS proper was remarkable both in time and space, and the critical roles of wind speed variability and the gas transfer velocity in the annual sea – air CO₂ flux estimation were still unresolved. Considering the new result of this study, the CO₂ sink in the global coastal ocean should be slightly lowered by 5%."

Tables 3 to 6: Is the seasonal average flux the mean of survey fluxes calculated using the W92 or S07 relation? This isn't clear in the text which relation is used, only that both are used so that values can be compared to other studies. Perhaps add the annual mean estimate for the respective domain to the legend?

Response: Agreed. In the modified MS, we add the seasonal average flux using the W92 equation. Therefore both results using the W92 and S07 equations are clearly presented. And we have added each annual value to the table legend for each respective domain.

Figure 1: Stars are hard to see, especially the SEATS location star. Perhaps shade darker?

Response: Modified accordingly.

Figures 7-10: perhaps make marker edge colors the same as the marker face colors? This may make some of the markers easier to see.

Response: Updated.

References:

Chen, C.-T. A., Wang, S.-L., Chou, W.-C., and Sheu, D.-D.: Carbonate chemistry and projected future changes in pH and CaCO₃ saturation state of the South China Sea, *Mar. Chem.*, 101, 277-305, 2006.

Dai, M.-H., Cao, Z.-M., Guo, X.-H., Zhai, W.-D., Liu, Z.-Y., Yin, Z.-Q., Xu, Y.-P., Gan, J.-P., Hu, J.-Y., and Du, C.-J.: Why are some marginal seas sources of atmospheric CO₂? *Geophys. Res.*

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Cao, Z.-M., Dai, M.-H., Zheng, N., Wang, D.-L., Li, Q., Zhai, W.-D., Meng, F.-F., and Gan, J.-P.: Dynamics of the carbonate system in a large continental shelf system under the influence of both a river plume and coastal upwelling, *J. Geophys. Res.*, 116, G02010, doi:10.1029/2010JG001596, 2011.

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