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Biogeosciences Discussions

Interactive comment on "Influence of seasonal monsoons on net primary production and CO_2 in subtropical Hong Kong coastal waters" *by* X. C. Yuan et al.

Anonymous Referee #2

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General comments:

The manuscript by Yuan et al. BGD deals with the control of air-sea CO2 fluxes and net primary production during seasonal monsoons in subtropical waters off Hong-Kong. The area studied here is largely impacted by anthropogenic inputs of nutrients and organic matter. The authors discuss the link between the trophic state of the ecosystem and the direction of the air-sea CO2 fluxes by considering the impact of the physical mixing during dry and wet seasons. They show that the trophic state of the coastal waters shifted from heterotrophic to autotrophic from the dry to the wet season. For the autotrophic period the authors argue that upwelling of DIC rich waters during the

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wet season maintained the efflux of CO_2 from the surface waters to the atmosphere. Thus, they address the issue of using air-sea CO_2 fluxes as an indicator of the trophic status of subtropical coastal ecosystems. However, their results are limited to a rather small area and the authors do not convincingly extend their findings to other subtropical ecosystems. Given the broad impact of papers published in Biogeosciences, I would suggest that the authors consider a different journal, which focuses more specifically on coastal ecosystems dynamics, to submit their manuscript. Below, several additional comments that would need to be address to improve the manuscript for future submission.

Specific comments:

Overall, the figures are of good quality, figure 3 should be enlarged in future submission. The manuscript is well written except for specific sections mentioned below.

Abstract: The abstract summarizes well the manuscript.

Introduction: Introduces well the topic and summarizes well the work previously done in the area. The data presented here were partly presented in Yuan et al. (2010) AME, particularly the air-sea CO_2 fluxes.

Section 2.3. The pCO_2 data were calculated using DIC and pH measurements. Since the pH measurements were made using an electrode calibrated with NBS buffers, their precision is limited (0.01). Thus the precision and accuracy of the pCO_2 calculated values should be discussed in more details. This is important since, for example, the authors rely on statistical values to test the seasonal variability for pCO_2 in regions VH and EW (section 3.2).

Section 2.4: For air-sea CO_2 calculations, the authors should consider using atmospheric pCO_2 values measured in the vicinity of their stations. Indeed, the dominant northeast winds during the dry season might transport some air enriched in atmospheric pCO_2 above Hong-Kong, thus impacting the air-sea CO_2 fluxes computations.

BGD

7, C2087–C2090, 2010

Interactive Comment

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In general, the impact of the low precision of the pCO2 measurements and the highly variable atmospheric pCO2 values encountered in this coastal area on the overall computed air-sea CO2 fluxes should be discussed in more details.

Section 3.2: Rephrase last sentence, not clear.

Section 4.2: In the last paragraph, the authors explain that besides biological control, mixing should be considered as a major process controlling dynamics of pCO2 and O2. This paragraph should be rewritten to clarify their idea.

Section 4.3:

My main concern about the assessment of the monsoonal influence on the NPP and air-sea CO2 fluxes is about the mixing term during the wet season. Page 5634, the authors argue that "The negative mixing contribution to CO2 in the wet season suggested that bottom offshore waters increased DIC concentrations due to upwelling in Hong Kong waters". Since the water column is stratified during the wet season, shouldn't this mixing term be considered as a "diffusion" term at the halocline and thermocline? If so and given the short residence time of the water mass (2 days), could diffusion alone be responsible for a 230 mmol C m-2 d-1 increase in DIC? Given the strong halocline shown on figure 3, the author should also consider the lateral inputs of rich DIC waters directly from the Pearl river estuary in their mixing term for stations 1 to 6. Once quantified, this lateral "mixing" term should be included to figure 7b.

Page 5634, line 12-14: rephrase sentence, not clear.

Section 4.4: Last sentence of the first paragraph. "In contrast to the relative consensus…". This statement should be revised in view of the recent work by Chen and Borges (2009) who introduced the concept of inner coastal ecosystems (estuaries, mangroves, etc…) as source of CO2 and continental shelf seas as sink of CO2 for the atmosphere. Note that this concept seemed to be supported by the results presented in the manuscript for the inner stations 1 to 7.

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7, C2087–C2090, 2010