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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.

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I. General Comments: RC: This manuscript was generally well organized and written, and the figures were of high quality. However, the methods in terms of calculations were not presented in a detailed and precise fashion in this study (See my specific comments), so that the calculated results seem to be not very convincible. I suggest that the authors should carefully deal with the calculations and thoroughly justify the uncertainties in these calculations before this manuscript can be considered for publication at Biogeosciences.

II. Specific comments: RC: P5625 L1-3: "Monthly data on salinity, temperature, primary

production, dark community respiration, DO, DIC and pCO2 are found in Ho (2007) and Yuan et al. (2010)". Does this sentence mean that all the data used in this study have been published elsewhere?

Response: We now added a paragraph to clarify: Data on salinity, temperature, primary production, DO, DIC and pCO2 at stations 1 to 8 were presented in Ho et al. (2008 and 2010) and Yuan et al. (2010a). In this study, these data along with wind, respiration and gaseous air-sea fluxes were grouped into three main regions (the PRE, VH and EW) in seasonal pattern. The average values of all seasonal parameters (e.g. salinity, temperature, primary production and DIC etc.) were calculated by averaging data from April to October for the wet season, and November to March for the dry season.

2.3 RC: P5626 L18-19: The method in pCO2 calculation should be given in a more detailed fashion. For instance, the pH scale and the dissociation constants of carbonic acid used in the computation should be specified. Furthermore, a careful error estimate on the calculated pCO2 is definitely needed, since the error could be quite large based on the reported precisions in DIC and pH measurements.

Response: We now added the details: pH was measured with an Orion Ross combination glass electrode (Dickson and Goyet, 1994), and a tris buffer at salinity 35 and three NBS pH buffers (pH=4, 7, 10) were used to derive a seawater pH scale and calibrate pH measurements. pCO2 was calculated from measured pH values and DIC concentration for estuarine and coastal waters using the equation (Cai and Wang, 1998): (1) where CT is the DIC value, {H}=10-pH, KH is the solubility constant (Weiss 1974), and K1 and K2 are the constants of carbonic acid (Roy et al., 1993). The 0.01 pH error will result in the uncertainties of $\pm$3% pCO2 (ca. 15 $\pm$ 6 μatm CO2) and $\pm$10% CO2 fluxes (ca. 3 $\pm$ 2 mmol C m-2 d-1), which does not considerably affect our conclusion due to high pCO2 in Hong Kong waters.

RC: P5626 L19-24: Delete these sentences, since I did not find any pCO2 mean SST being used throughout the manuscript.

Response: We agree and delete these sentences.

RC: P5627 L6-12: The flux calculations of CO2 and O2 also need to be presented in a more detailed fashion. For instance, the formulae used in calculating the solubility of CO2 and saturated O2, and the wind speed data used (daily or monthly?) in parameterizing gas transfer velocity should be specified.

Response: We add those details: The CO2 solubility coefïňĄcient was formulated by Weiss (1974). pCO2w and pCO2a represent the partial pressure of CO2 in surface water and overlying air, respectively. [O2] and [O2]S represent the measured concentrations and estimated oxygen solubility, respectively. DO solubility was calculated according to Benson and Krause (1984). The gas transfer velocity (k) was empirically estimated from the daily wind speed at 10 m (Wanninkhof, 1992), which was obtained from the Hong Kong observatory (http://www.weather.gov.hk/). We also mentioned how to calculate gas transfer velocity.

2.4 RC: P5627 L12-13: The adoption of atmospheric pCO2 of 370 ïAËŻ atm may be inadequate. Considering the sampling site is very close to a mega city, it is very likely subject to land mass influence as reported in many other near-shore environments (e.g. Borges and Frankignoulle 2001 and references therein). Therefore, I suggest the authors should try to find other more representative atmospheric pCO2 data.

Response: Incorporating the reviewer's concern, we now added a discussion on the uncertainty of unknown atmospheric pCO2 value: The atmospheric pCO2 has been reported to be in the range of 349 to 372 μatm in inner shelf/coastal areas adjacent to the Pearl River plume (Zhai et al., 2005), and $\sim$358 μatm in offshore waters (Zhai et al., 2009). Since our sampling sites are very close to a mega city (Hong Kong), the land mass influence may result in higher atmospheric pCO2, especially in the dry season when northeast winds were dominant. A large range of the atmospheric pCO2 (349 to 460 μatm, and averaged 400 μatm) was reported in Randers Fjord, Scheldt, and Thames (Borges et al. 2004), where sampling sites were also close to anthropogenic

influences. The average atmospheric pCO2 (400 μatm) is used for the calculation of the air-sea flux of CO2 in our studies. The variations in atmospheric pCO2 (349 to 460 μatm) would quantitatively result in the estimates of average CO2 effluxes (-20 mmol C m-2 d-1) varying from -12 to -25 mmol C m-2 d-1.

4.2 RC: P5631 L15 – P5632 L14: The plots of DO saturation level vs. IPP and ïAËŻDËĞ pCO2 (the difference between surface water and air pCO2) vs. IPP would be helpful to clearly demonstrate the relationship between O2/CO2 and the trophic state (net biologically metabolic balance). Additionally, two recent publications (Chen and Borges, 2009; Chou et al., 2009) regarding to this issue should be mentioned.

Response: We added a table which described the CO2 fluxes vs NCP which is helpful to demonstrate the the relationship between CO2 and the trophic state. This two recent publications were cited

4.3 RC: P5632 L24: Coriolis effect ! Ekman transport would be a better term.

Response: We agree Ekman transport would be a better term

RC: P5634 L3: The term of Rbenthic (benthic respiration) should appear in Eq. (4).

Response: According to editor's comments, we only calculated the DIC variations in mixed layer and deleted benthic respiration.

RC: P5634 L4: oxygen input ! DIC input

Response: Yes, should be "DIC".

RC: P5634 L5-6: Are "total ecosystem respiration" and "gross primary production" equal to "DCR" and "IPP", respectively? If yes, please use the same terminology; if no, it should note the difference between these definitions, and explain how you got the values of "total ecosystem respiration" and "gross primary production" in your calculation.

Response: All are revised as net community production (NCP) according to editor's suggestion.

RC: P5634 L7: This equation is mathematically incorrect. It should be something like "DIC(mixing)= dDIC/dt + DIC(airôĂĂĂsea fluxes)–DIC(pelagic NPP)–DIC(benthic respiration)"

Response: Revised as: DICmixing = - (DICNCP + Fair-sea)

Figures RC: P5643 Fig. 3 should be enlarged.

Response: enlarged

RC: P5644 Indicating saturated DO concentrations on Fig. 4(A) and atmospheric pCO2 levels on Fig. 4(B) would be helpful.

Response: We added saturated DO concentrations on Fig. 4(A) and atmospheric pCO2 levels on Fig. 4(B)

RC: P5645 Please explain why there are 260 data points on Fig. 5. (8 (stations) x 7 (cruises) = 56?)

Response: We take the samples at 1 m, 4 m and 2 m above the bottom. Triplicate was taken in first two cruises. Since the editor comments mentioned that air-sea flux of CO2 only affect mixed layer (see interactive comments), we revised the Fig and only included the data in mixed layers (Table 2).

RC: P5647 Please explain how to obtain all the numbers for dry and wet seasons (take average of summer and fall for wet season, and average of spring and summer for dry seaon? Or : : :).

Response: We will mention it in the section of Methods. The average values of all seasonal parameters (e.g. salinity, temperature, primary production and DIC etc.) were calculated by averaging data from April to October for the wet season, and November to March for the dry season.

Interactive comment on Biogeosciences Discuss., 7, 5621, 2010.