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***Interactive comment on* “Influence of seasonal monsoons on net primary production and CO₂ in subtropical Hong Kong coastal waters” by X. C. Yuan et al.**

Anonymous Referee #5

Received and published: 10 September 2010

Review of the paper Influence of seasonal monsoons on net primary production and CO₂ in subtropical Hong Kong coastal waters by X. C. Yuan, K. D. Yin, W.-J. Cai, A. Y. T. Ho, J. Xu, and P. J. Harrison submitted to Biogeosciences for possible publication.

General comments: This work is aimed at providing a description of the factors controlling the seasonal variations of both the air-sea CO₂ exchange and primary production in the subtropical coastal waters of Hong Kong. The study is based on field data obtained during the development of seven cruises that temporally cover the shift that in the monsoonal wind regime occurs in the region. According to the results attained, the authors conclude that the trophic status of the system changed from heterotrophic

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during the winter dry season to autotrophic in the summer wet season, with the area acting generally as a source of CO₂ to the atmosphere. Although the study analyses a relatively small area, it apparently contains several contrasting environments in terms of hydrodynamics, their biogeochemical water properties and anthropogenic forcing, which is expected to influence the dynamics of the carbon systems. However, it seems that the contribution of each partial system on the global trend observed is not properly addressed and the underlying mechanisms behind the observed patterns are not entirely explained. Moreover, the methods used to calculate the O₂ and pCO₂ fluxes seem somehow confusing, which introduces certain doubts about the accuracy of the data. Taking into account these considerations, the conclusions drawn from this study should not be generalised to a larger geographic region or extended to other subtropical coastal areas, as it is implicit in some parts of the manuscript. Therefore, some substantial modifications should be made in the manuscript in order to fulfil the requirements needed to be considered for publication in Biogeosciences.

Specific comments: Methods.- Page 5625, lines 2-3: Differences between data presented in Ho (2007) and Yuan et al. (2010) and those contained in the current work should be stated. Page 5626, line 18: As pCO₂ was computed from the pH and DIC measurements, the uncertainty associated to the calculations should be provided. Also, it is assumed that the dissociation constants used are those given by Cai and Wang (1998) but a more comprehensive explanation of the procedure would be desirable. Page 5626, line 22: The temperature effect on pCO₂ does not seem to be addressed in the study. Hence, either more results are added or equation 1 should be deleted. Page 5627, line 11: Which is the frequency of the wind speed data? Also, the atmospheric pCO₂ value of 370 microatm might not be entirely correct.

Results.- Page 5628, lines 3-4: A considerable part of the results (and the subsequent seasonal patterns of the carbon system properties considered) is explained based on the influence of the freshwater discharge from the Pearl River. However, no data of such a riverine input is provided. I would suggest to add a figure showing the seasonal

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river discharge to the coastal fringe. Similarly, although slightly mentioned in the discussion and published in Ho et al., (2008), the seasonal patterns of nutrient distribution would also help to explain the river influence on biological productivity in the area and the concomitant effect on DIC dynamics. In addition, it would be helpful to mention how the average values of salinity and temperature for both seasons are obtained (same for the rest of variables and parameters considered). Page 5629, line 9: The air-sea CO₂ fluxes seem to be very high and specially in relation to the O₂ fluxes. Are they correct? Wind speed data should be also included in Figure 4.

Discussion.- Page 5632, line 24: It would be more appropriate to indicate that downwelling conditions are due to Ekman transport rather than to the Coriolis effect. Page 5634: I don't quite understand how the effect of mixing on carbon dynamics is addressed here. Please clarify the equation and the contribution of each term in the vertical profile. Page 5635, lines 7-9: The work by Borges and Chen (2009) reconciling opposing views on carbon cycling in the coastal ocean should be mentioned in this part and overall, considered in the entire section 4.4 for discussion. Page 5636, lines 14-16: Conclusions drawn from this study can not be generalised to other coastal areas, as in fact, this geographical region seems to be very particular in terms of hydrodynamics and human forcing.

Figures: -Please make Fig. 3 bigger. -Please add the atmospheric pCO₂ value in on Fig. 4 along with the wind speed data.

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

BGD

7, C2866–C2868, 2010

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