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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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RC: Review of the paper "InïňĆuence 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 for publication to Biogeosciences. The paper "InïňĆuence of seasonal monsoons on net primary production and CO2 in subtropical Hong Kong coastal waters" by Yuan and coworkers reports a detailed study with seasonal resolution of the CO2 system and controlling factors in subtropical Hong Kong coastal waters. The paper provides relevant and exciting mechanistic understanding on the ramiïňĄcations of physical and biological processes controlling

CO2 and oxygen air-sea ïňĆuxes. The paper is on the concise side of publications, which I personally appreciate. In general, and beside some minor points, given below, the paper is appropriate for publication in Biogeosciences, except for two major points, of which I think, that they can be easily addressed:

RC: 1: There is no discussion at all about alkalinity and its potential inïňĆuence on CO2 air sea ïňĆuxes. The regions is under inïňĆuences of a larger river, and it can be assumed that alkalinity is released to the coastal system by the Pearl River. Further, the area is shallow, under inïňĆuence of sewage/nutrient inputs and subjected to upwelling, all setting the stage for alkalinity generation within the system (see for example Thomas et al., 2009 in Biogeosciences). It is difïňĄcult for me to see whether and to what extend this might play a role, but it certainly needs to be addressed/discussed here, since alkalinity does affect CO2 ïňĆuxes directly, but not the oxygen ïňĆuxes.

Response: We added a paragraph to discuss alkalinity: Alkalinity inputs could also influence the CO2 fluxes in Hong Kong waters, as alkalinity fluxes into the South China Sea was estimated to be 462 x 109 mol yr-1 from the PRE (Guo et al., 2008). In addition, alkalinity inputs may also come from anaerobic alkalinity generationin in coastal sediments (Thomas et al., 2009), as well as year-round sewage discharges and upwelling in Hong Kong waters. These alkalinity inputs might increase the CO2 buffer capacity in Hong Kong waters, but oxygen ïňĆuxes were not affected by alkalinity and showed no seasonal variations at all areas ($p > 0.05$, Fig. 4A). However, dispite of the alkalinity inputs, surface pCO2 and air-sea fluxes of CO2 near the PRE were higher in the wet season than the dry season ($p < 0.05$, Fig. 4C), suggesting that the Pearl River was a strong source of atmospheric CO2.

RC: 2: In my view there is massive confusion with signs of the ïňĆuxes throughout the manuscript, this to a degree that I have difïňĄculties to follow the line of argument at times. I assume that authors overlook the consistency of their arguments, I have had problems with it. Based on the authors' ïňĆux equations (eqs. 2+3), a negative partial pressure difference of CO2 or oxygen, which is an undersaturation of the surface

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waters, leads to a positive ïňĆux according to Fick's law. This is also the convention since a positive ïňĆux increases the DIC/oxygen pools of the ocean. For example the statement in the abstract (as representative for many others): "...and inïňĆux of O2 (-100+-60 mmol O2 m-2 d-1)..." is simply contradictory. Similarly, if production terms (NPP, IPP) are positive, this means uptake of carbon (CO2), i.e., a reduction of the CO2 pool. Negative production terms are deïňĄned as respiration. Also, the respiration term is ïňĄgure 7 and elsewhere needs to be positive (positive respiration release DIC and consumes oxygen). It is also unclear to me why the authors invert the signs while setting up the carbon balance in ïňĄgure 7? A negative (!) CO2 ïňĆux, i.e., outgassing, does reduce the DIC pool of the water column, the sign thus gives in this case the right reïňĆection of the process. NPP is indeed different, and one option would be to draw a box around the NPP ïňĄgures in ïňĄgure seven, which would indicate the organic pool. Adding an arrow could then provide the direction of the CO2 ïňĆux into or out of the organic box, for example. If the authors wish to "adjust" the sign of NPP to the DIC pool perspective in the text, they might add redundant statements such as: a NPP of 3 mol C m-2 d-1, corresponding to a change of the water column DIC -3mol C m-2 d-1, would lead to...... A redundancy here is better that a misunderstanding. Again, the authors have to thoroughly check the consistency of their arguments throughout the paper to reconïňĄrm that their arguments are not affected by the confusion of the signs.

Response: We agree that negative values represent CO2 losses into the atmosphere and positive values are CO2 flux into water column in Figures, with the exception of NPP as positive NPP coincided with CO2 losses.

RC: Minor comments: Abstract, p5622, line 15: ... high positive NPP corresponding..... In my feeling a better word would be "paralleled" or "coinciding with", because high NPP corresponds to an increase uptake of CO2 / decreased CO2 release. I thought the point the authors wanted to make and wanted to later explain is this discrepancy?

Response: We use the word "coinciding with".

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RC: Introduction, p 5623, lines10-15: A good reference to refer to would be Lendt et al., 2003, in JGR.

Response: We cited Lendt et al., (2003) in revised ms.

RC: Methods: p562: Please give details whether there were single or repeated occupations of the stations. Please also brieïňĆy discuss tidal effects and variability shorter that seasonal, if information is available.

Response: We added the tidal effects: In Hong Kong coastal waters, tides are mixed and predominantly semi-diurnal, and the mean tidal range is 1.7 m with a range from 1 m during neap tides to up to 2 m during spring tides with little seasonal tidal variation (Lee et al., 2006).

RC: P5626, lines 5-15: please give uncertainty of the computed pCO2 and the related ïňĆuxes.

Response: 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: P5627: line9: What is estimated solubility? Please provide information on how the solubility was computed.

Response: ïĄć (Bunsen coefficient) is the solubility of CO2 at a given temperature and salinity. The CO2 solubility coefïňĄcient was formulated by Weiss (1974). DO solubility was calculated according to Benson and Krause (1984).

RC: Results: p5929, l23: coastal

Response: Added

RC: Discussion: p5631, line 12: please give oxygen saturation value.

Response: We added: the dissolved oxygen (DO) remained >150 μM with a solubility

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ranging from 210 to 240 μM (Fig. 4A)

RC: P5632: A detailed discussion of this issue is given by Thomas et al., 2005 (Biogeosciences 2, 87-96). In summary I do not think this is a paradox, since the coastal ocean is far away from being an ideal 1-box system.

Response: We revised it as a “discrepancy” as you suggested above.

P5633, l16-17. Isn’t this the trap the authors wanted to eliminate? NPP does not necessarily reverse the ïňĆuxes, (positive) NPP does reduce CO2 release to the atmosphere, or strengthens CO2 uptake, thus, NPP shifts the ïňĆux toward more positive values. This may correspond to a change in the direction of the ïňĆux, but does not have to. Further, I think one point to mention here is the length of the integration time scales (observations, processes, computations). In principle, NPP does change the pCO2 and thus the ïňĆux, as NPP is active. The response of the DIC/pCO2 is immediate. Only the ïňĆux itself is slow and thus the response of the water column DIC inventory to the ïňĆux delayed relative to NPP.

Response: We are studying seasonal variations, and hence the time lag between NPP and the flux was not obvious. But we agree that the statement that NPP does reduce CO2 release to the atmosphere, or strengthens CO2 uptake, thus, NPP shifts the ïňĆux toward more positive values. Revised as: However, the air-sea exchange directions of O2 and CO2 were not reversed, although the high IPP and NPP could shift the ïňĆuxes of CO2 toward more positive values.

RC: P5633, l20: DIC unit is mikro mol kg-1. Or do you mean pCO2 rather than DIC?

Response: Yes, DIC should be μM. We revised it.

RC: P5634, l4: carbon rather than oxygen??

Response: Yes, it should be “DIC input”

RC: p5635, l21: latitudes?

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Response: Yes, it should be "latitudes"

RC: P5635: Again, I think that Lendt et al., 2003, and also Borges et al., 2008, Biogeosciences 5(1), 141-155, would be appropriate to discuss here. Further, one other point to mention here is the possibility of different nutrient to carbon ratios in sinking organic matter and upwelled waters.

Response: We added a discussion: Borges et al. (2008) reported that enhanced upwelling may result in a decrease of the CO2 sink during positive phase of the Southern Annular Mode (SAM) in high latitude areas of the Southern Ocean Southern and an increase of the CO2 sink in the low latitude. The seasonal variations in the mixed layer depth was also reported by Lendt et al., (2003), in which mixed layer depth was 25 m before on set of southwest monsoon and less than 10 m when the upwelling was developed in the northwestern Arabian Sea. We also discussed the DON/DOP ratio: The seasonal variations in water circulation likely affected nutrient composition, as Wu et al, (2003) observed that N:P ratios in inorganic and organic matter were 1 to 10:1 (DIN/DIP) and 28:1 (DON/DOP) at the surface, and increased to 14:1 and >40:1 respectively below 200 m in a non-upwelling offshore region of the South China Sea. Therefore, the vertical mixing can easily alter N:P ratios at the surface. In contrast to Arabian Sea where the upwelling does not directly supply nitrate (Lendt et al., 2003), high nitrogen relative to phosphorus is brought up from the bottom in upwelled waters. In-situ experiments showed that BP was limited by phosphorus in the northern South China Sea, as bacterial demand of phosphorus was higher than phytoplankton (Yuan et al. submitted). In southern Hong Kong waters which was influence by both upwelling and lateral mixing of the PRE, BR was also limited by phosphorus (Yuan et al. 2010b), which partially reduced the decomposition of organic matter.

Figures:

RC: Fig 3: Please add sampling depths / locations to the panels.

Response: It is added.

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RC: Fig 4a, c: Please adjust y-axis. There is no need to start a zero, if all values are much larger. In Fig 4c, please also indicate the atmospheric CO2 value.

Response: We agree and adjust y-axis and indicate the atmospheric CO2 range

RC: Fig 5. There is not only a negative coefïňĄcient in the relationship, also CO2 and oxygen ïňĆuxes differ by an order of magnitude. Please discuss this in the paper.

Response: We added a discussion: The air-sea fluxes of CO2 varied from -3 mmol C m-2 d-1 in the eastern water to -40 mmol C m-2 d-1 near the Pearl River estuary (Fig. 4), while Zhai et al. (2005) reported that air-sea fluxes of CO2 was 2 to -18 mmol C m-2 d-1 in inner/coastal shelf waters. The slope (0.17) between the air-sea fluxes of CO2 and O2 indicated that O2 will re-equilibrate to atmospheric pressure more quickly than pCO2 (Fig. 5), which was due to the fact that the ratio of fractional change in seawater pCO2 to the fractional change in total DIC is $\sim$10 (Revelle ratio) in the northern South China Sea (Zhai et al., 2009). Carrillo et al., (2004) reported that oxygen concentrations approach atmospheric equilibrium in approximately 30 days, while CO2 only changed by approximately 12% during the same time span.

RC: In my print out there appear two boxes in the neg/neg and pos/pos quadrants. Is this a printer error or intentional? If intentional, please clarify.

Response: The figure was deleted now, and the regression was included in Table 2.

RC: Fig 7: As discussed above this ïňĄgure remains a miracle, unless you adjust signs.

Response: We change all those signs.

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