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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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Comments from handling editor (Alberto V. Borges) I read the ms and made my comments on the very ïňĄrst version of the ms, hence, Line numbers refer to those in that version and not to the line numbers in the BGD version published on-line.

RC: It would nice if the integrated primary production, respiration, air-sea CO2 ïňĆuxes, photic depth, mixed layer depth, SSS, SST, positions, dates, etc... are given in a tabular form as a supplement of the paper. Researchers that compile these data to carry out metaanalysis (e.g. Gattuso et al. 1998, Gazeau et al. 2004, Hopkinson and Smith 2005) will certainly be grateful to the authors to be able to access the raw data rather than having to scan them from ïňĄgures of the paper.

Response: a table is given as your suggestion, which include seasonal changes in sea surface salinity (SSS), temperature (SST), photic depth (1% light depth) (PD), mixing layer depth (MLD), dissolved inorganic nitrogen (DIN), PO4, SiO4, integrated dark community respiration (IDCR), primary production (IPP) and air-sea CO2 flux (FCO2) near the Pearl River estuary (PRE), Victoria Harbour (VH) and eastern waters (EW)

RC: The authors could attempt to establish correlations between the measured rates and environmental variables such as temperature, salinity, chlorophyll-a, or other variables. Such analysis can be enlightening (e.g., Hopkinson and Smith, 2005).

Response: CO2 flux rates were correlated with environmental variables. Regression analysis performed for each region separately (See table 2). We also discussed the table.

RC: L 27 : Here and everywhere in the ms. I assume that the net metabolic balance was computed as the difference between the integrated primary production values derived from the 14C incubations and the dark community respiration (DCR) (L133). If this is the case then there is problem in terminology : Net primary production (NPP) = gross primary production (GPP) – autotrophic respiration. What the authors computed is net community production (NCP): NCP = GPP - autotrophic respiration – heterotrophic respiration = GPP – community respiration (e.g. Gattuso et al. 1998). Please update the text everywhere (and ïňĄgures).

Response: We updated the text.

RC: L 50-54: Please note that besides organic carbon inputs, eutrophication also corresponds to enhanced nutrient ïňĆuxes. These tend to increase primary production. The balance of both will determine how the CO2 ïňĆux will evolve. Relevant references on the subject that could be added here are : Mackenzie et al. (2004), Gypens et al. (2009), Borges & Gypens (2010).

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Response: More discussion was added: The organic matter inputs from riverine outflow and domestic sewage effluent have increased the occurrences of hypoxia or anoxia as well as high CO2 release in some estuarine and coastal waters (Ducklow and McCallister, 2004; Diaz and Rosenberg, 2008; Borges et al., 2006), while the enhanced inorganic nutrient ïňĆuxes increase primary production and consequently oxygen production and CO2 sink (Mackenzie et al., 2004; Gypens et al., 2009). Hence the ratio of inputs of dissolved inorganic nutrient to labile organic matter will determine how the CO2 ïňĆux will evolve (Borges and Gypens, 2010).

RC: L 124-138 : Please specify if light proïňĄles were carried out and please specify which model was used to integrate vertically the primary production (e.g. Platt).

Response: Primary productivity at each depth was calculated according to Jassby and Platt (1976).

RC: L 124-138 : Here or elsewhere in the ms it needs to be mentioned that due to short incubation time (4h) the 14C rates are assumed to be representative of GPP and not net primary production (e.g. Gazeau et al. 2007). This then allows the computation of NCP as GPP – DCR.

Response: We added: The respiration of 14C labeled organic matter which partially depends on incubation time (Gazeau et al. 2007), and the 14C uptake rates are assumed to be representative of gross primary production with insignificant respiration of 14C labeled organic matter due to the short incubation time (4h) in this study.

RC: L 152: Another way to normalize the data, that is thermodynamically more correct, is to compute total alkalinity (TA) from pCO2 at in-situ temperature and DIC, and recompute pCO2 from TA and DIC at the mean temperature. Indeed, the Takahashi et al. procedure assumes an isochemical water-mass which cannot be the case for the present study. Please note that the correct reference for this equation is Takahashi et al. (1993).

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Response: We agree that this method is more correct. But we deleted this equation as suggested by previous two reviewers.

RC: L 268: this statement is wrong, and the authors actually say it latter (and their data-sets illustrate it). So why make this statement? Why not directly say that trophic status and air-sea CO2 fluxes do not equate (based on what has already been published in literature) ?

Response: We agree and it is revised.

RC: L 278: the Borges et al. (2006) paper should be cited here, as it is one the first (if not the first) to compare trophic status and air-sea CO2 fluxes in coastal environments based on consistent data-sets (acquired at the same time) and across several ecosystems.

Response: We added a discussion: Borges et al., (2006) reported the discrepancy that a positive NCP (autotrophic status) was associated to a source of CO2 during a cruise in the Bay of Palma while a negative NCP (heterotrophic status) was related to a sink of atmospheric CO2 during a Randers Fjord cruise.

RC: L 296-297 : This statement is incorrect. Sarma et al. (2001) provide such a comparison for DIC, pH and pCO2.

Response: We added the discussion: The responses of O2 and CO2 fluxes to the seasonal monsoons have not been addressed in previous studies in Hong Kong waters, although the monsoonal influence on CO2 has been studied in other estuarine waters, such as in a tropical estuarine system, Goa, India (Sarma et al., 2001).

RC: L 336: As far as I understand this equation is not necessary to understand the compu-tations made (and saying that DICinput and DICoutput were computed from O2 is very confusing). Please only refer to equation given in Line 339 (that should be numbered) saying that the terms of the mass balance are ... and that DICmixing is computed as the closing term of the mass balance.

Response: We kept the equation in Line 339, and delete the one in Line 336

RC: L343 : I assume it's the “air-sea fluxes” of CO2 that were used ? please specify in text.

Response: We specify it as CO2

RC: L366-352: More details are needed to define the terms of the mass balance. As the text stands it's very difficult to follow what the authors did: First, specify if “deltaDIC(pelagic NPP)” (that need to be changed in “deltaDIC(pelagic NCP)”) corresponds to the values integrated in the photic depth ? or integrated in the mixed layer ? The problem is that the deltaDIC(air-sea fluxes) only affects the DIC in the mixed layer; if mixed layer is shallower than photic depth, then there is an inconsistency in the mass balance if deltaDIC(pelagic NCP) was integrated in the whole photic depth.

Response: NCP was in the photic depth and DCR in the whole water colomn in the previous version. We recalculated NCP in the mixed layer, respiration is also estimated in the mixed layer.

RC: Second, if the mass balance of DIC is looking at the DIC changes in the mixed layer, then there is no point in including the deltaDIC(benthic respiration) term in the mass balance. The closing term will correspond to the flux of DIC from depth including whatever DIC was added by benthic respiration to the upwelled water.

Response: We deleted benthic respiration and recalculate the mass balance of DIC in the mixed layer.

RC: Finally, the deltaDIC(mixing) term will include the vertical inputs but also the horizontal inputs. This needs to be mentioned and discussed. If the authors can find a way to work out the horizontal inputs of DIC, this would then allow to look at only the vertical fluxes of DIC.

Response: The horizontal inputs of DIC is mentioned and discussed. We added an estimates of the contributions of the influence of the Pearl river estuary with equation:

PPRE =(S-SPRE)/(Soceanic – SPRE), where PPRE is the proportion of water masses from the PRE, and S, SPRE and Soceanic are the salinities in studied, the Pearl River estuary and oceanic waters. Hence, lateral inputs of rich DIC were estimated.

RC: L 366: Since the Ducklow and McCallister (2004) work (6 years ago), several papers have addressed the air-sea CO2 fluxes in coastal environments (Borges 2005; Borges et al. 2005; Cai et al. 2006; Chen and Borges 2009; Laruelle et al. 2010). From these papers there is an emerging picture on the drivers of CO2 fluxes in coastal environ-ments and magnitude of those fluxes. So nowadays things are not that "controversial" as stated.

Response: We cite those studies to discuss this issue: more recent studies have addressed the air-sea CO2 fluxes in coastal environments (Borges et al., 2005; Cai et al., 2006; Chen and Borges, 2009; Laruelle et al., 2010). For example, the synthesis of worldwide measurements of the partial pressure of CO2 (pCO2) indicates that most inner estuaries and near-shore coastal areas are over-saturated with respect to atmospheric CO2 (Chen and Borges, 2009).

RC: L368: There are several recent papers on CO2 fluxes in coastal upwelling systems (see hereafter) that could be cited in this section.

Response: We cited more in this section for comparison

RC: L377-379: There is an alternative explanation, and actually the data reported by the present paper fits with that explanation. As developed by Borges (2010), coastal up-welling systems in the Atlantic are sinks for CO2 (Borges and Frankignoulle 2002; Huertas et al. 2006; González-Dávila et al. 2009), while those in the Pacific (Friederich et al. 2002; 2009) and Indian (Goyet et al. 1998) are sources of CO2 (based on datasets that adequately capture the seasonal cycle, hence excluding studies that only reports limited data during a single season). This relates to the conveyer belt. Denitrification removes NO3-while not affecting DIC. Hence, there is a relative enrichment of DIC compared to NO3-as the water masses age (i.e. travel from the Atlantic to the Pacific and Indian). Also, oxygen minimum zones are much more developed (and shallower) in the Indian and Pacific than in the Atlantic. Refer to Borges (2010) for an in depth discussion on this topic.

Response: We have updated the explanation: In addition, as the synthesized by Borges (2010), coastal upwelling areas associated to oxygen minimum zones (OMZ) are sources of atmospheric CO2 such as the Arabian Sea (Goyet et al.1998) and the Peruvian and Chilean coasts (Friederich et al. 2008; Paulmier et al. 2008). Coastal upwelling areas devoid of OMZ such as the Iberian coastal upwelling system (Borges and Frankignoulle 2002a) or with deep OMZ such as the Oregon coast (Hales et al. 2005) are sinks for atmospheric CO2. Several recent studies showed that coastal up-welling systems in the Atlantic are sinks for CO2 (Borges and Frankignoulle 2002; Huertas et al. 2006; González-Dávila et al. 2009), while those in the Pacific (Friederich et al. 2002; 2009) and Indian (Goyet et al. 1998) are sources of CO2. This is likely a consequence of the higher accumulation of DIC relative to NO3 due to denitrification along the Conveyer Belt (i.e. travel from the Atlantic to the Pacific and Indian), and more and shallower OMZ in the Indian and Pacific than in the Atlantic (Laruelle et al., 2010).

Minor comments

RC: Most abbreviations (NPP, DO, etc...) were defined several times in the ms. Please define an abbreviation only ONCE, when it is used for the FIRST TIME in the main text (abstract excluded).

Response: We will address it.

RC: L 243 : DOC abbreviation not defined

Response: Dissolved inorganic carbon (DIC)

RC: L 258: bacterial respiration => BR

Response: We agree.

RC: L 266: replace heterotrophy by trophic status

Response: We agree.

RC: L 435 : This reference could be replaced by Borges and Frankignoulle (2001) Distribution of surface carbon dioxide and air-sea exchange in the upwelling system off the Galician coast, Global Biogeochemical Cycles, 16: 4/1-4/14, doi:10.1029/2000GB001385, that is a more extensive discussion on air-sea CO_2 fluxes in the Galician upwelling system.

Response: We will replace it.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C3402/2010/bgd-7-C3402-2010-supplement.pdf

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