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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 first 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.

It would nice if the integrated primary production, respiration, air-sea CO2 fluxes, 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 meta-analysis (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 figures of the paper.

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).

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 figures).

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

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

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.

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 re-

compute 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).

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) ?

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.

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

L 336: As far as I understand this equation is not necessary to understand the computations 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.

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

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.

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.

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.

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 environments and magnitude of those fluxes. So nowadays things are not that "controversial" as stated.

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

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 upwelling 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 data-sets 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.

Minor comments

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).

L 243 : DOC abbreviation not defined

L 258: bacterial respiration => BR

L 266: replace heterotrophy by trophic status

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 CO2 fluxes in the Galician upwelling system.

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