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Biogeosciences Discussions

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Interactive Comment

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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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 for publication to Biogeosciences.

The paper "Influence 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 ramifications of physical and biological processes controlling CO2 and oxygen air-sea fluxes. 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:

1: There is no discussion at all about alkalinity and its potential influence on CO2 air-sea fluxes. The regions is under influences 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 influence 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 difficult 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 fluxes directly, but not the oxygen fluxes.

2: In my view there is massive confusion with signs of the fluxes throughout the manuscript, this to a degree that I have difficulties 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' flux equations (eqs. 2+3), a negative partial pressure difference of CO2 or oxygen, which is an undersaturation of the surface waters, leads to a positive flux according to Fick's law. This is also the convention since a positive flux increases the DIC/oxygen pools of the ocean. For example the statement in the abstract (as representative for many others): "...and influx 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 defined as respiration. Also, the respiration term is figure 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 figure 7? A negative

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(!) CO2 flux, i.e., outgassing, does reduce the DIC pool of the water column, the sign thus gives in this case the right reflection of the process.

NPP is indeed different, and one option would be to draw a box around the NPP figures in figure seven, which would indicate the organic pool. Adding an arrow could then provide the direction of the CO2 flux 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 reconfirm that their arguments are not affected by the confusion of the signs.

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?

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

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

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

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

Results: p5929, l23: coastal

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

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.

P5633, l16-17. Isn't this the trap the authors wanted to eliminate? NPP does not necessarily reverse the fluxes, (positive) NPP does reduce CO2 release to the atmosphere, or strengthens CO2 uptake, thus, NPP shifts the flux toward more positive values. This may correspond to a change in the direction of the flux, 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 flux, as NPP is active. The response of the DIC/pCO2 is immediate. Only the flux itself is slow and thus the response of the water column DIC inventory to the flux delayed relative to NPP.

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

P5634, l4: carbon rather than oxygen??

p5635, l21: latitudes?

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.

Figures:

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

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.

Fig 5. There is not only a negative coefficient in the relationship, also CO2 and oxygen

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fluxes differ by an order of magnitude. Please discuss this in the paper. 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.

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

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

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