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Biogeosciences
Discussions

Interactive comment on "Coupling of surface *p*CO_2 and dissolved oxygen in the northern South China Sea: impacts of contrasting coastal processes" *by* W. Zhai et al.

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Response to Review #1 (by Fiz F. Pérez)

In our paper we examined the relationship between CO_2 partial pressure (*p*CO_2) and dissolved oxygen (DO) based on a cruise conducted in July 2004 to the northern South China Sea, spanning from estuarine plume, coastal upwelling and deep basin areas. Distinct relationships between *p*CO_2 and DO saturation were identified in different regimes. This study reveals that a combination of high-resolution CO_2 and O_2 measurements may provide valuable information regarding net metabolic status in marine ecosystems under different physical and biogeochemical conditions. We have demon-

strated a simple procedure to evaluate the community metabolic status based on these surface *p*CO_2 and DO measurements, which may have applicability in other coastal systems with a large gradient of changes in their physical and biogeochemical conditions.

We agree with the reviewer's comment about the air-sea gas exchange. The important role of air-sea gas exchanges in the *p*CO_2 - DO relationship in those low productive areas is one of the key issues of this study. In the modified MS, we have added more discussion on this issue.

We appreciate that the reviewer pointed out the bubble effect coefficient issue, and agree that the bubble effect on surface DO may vary depending on factors such as breaking surface waves. Our observed surface DO was mostly in the range of 103% -107% (Fig. 2f) at Transect S, the trend of which was consistent with the chl-a (from <0.1 to 0.2 μg L^{-1}, Fig. 1) although the area is generally very low in biological production. As such, we justified that the 2.5% supersaturation we adopted from Broecker and Peng (1982) and Stigebrandt (1991) should be reasonable to be applied to the study area. However, this supersaturation might be subject to variations given the regional heterogeneity in terms of surface wave field. Thus, using a fixed supersaturation rate to characterize the bubble effect may have resulted in uncertainties. For example, under the condition of a same DO concentration of close to air-equilibrium (200 μmol kg^{-1}), one site having the bubble effect of 2.5% DO super-saturation and another with the bubble effect of 5.0% DO super-saturation, the calculated excess O_2 would show a difference of 5 μmol kg^{-1}. Unfortunately, there is no way that we could make the correction for individual data point. Therefore, we have made clear notes of this potential bias in the revised MS both in the method section where the excess O_2 was defined and the caption of Fig. 5 (of the original MS), where results of excess O_2 were presented. However, we must also point out that such uncertainties (<5 μmol kg^{-1}) should be minor, given the fact that the ranges of DO spatial variations were as high as 80 μmol kg^{-1} in nearshore areas (Fig. 5a in the original MS) and 50 μmol kg^{-1} in the PRE (Fig. 5c in the original MS). Most importantly, such uncertainties would not affect the approach we are using to examine the community metabolic status based on surface pCO_2 and DO measurements, nor the general conclusion of this study.

The reviewer suggested that we added more information in section 4.1 to compare modeling results (Eq. 2) with field-measured data. We have adopted the suggestion. We should also point out that Eq. (2) shows the relationship between temperature normalized pCO_2 versus excess O_2, which is different from the slope between pCO_2@SST versus DO% presented in Fig. 4 of the original MS. However, with an inclusion of the temperature effect on pCO_2 and DO%, we can obtain a similar slope between -135 and -160 μatm/%DO. Note that we have added clarification in the revised MS.

As for the comment about the CO_2:O_2 ratio, we agree that CO_2:O_2 ratio could also be generated from water mixing. However, here we used measured CO_2:O_2 ratios as an initial condition of calculation, rather than as a result of air-sea exchanges. We also partially agree with the reviewer's following assertion. If the CO_2 gradients are large enough to balance the O_2 air-sea flux, the CO_2 and O_2 exchanges would maintain at a stable slope over time. This study, however, demonstrated that there are cases that the air-sea CO_2 gradients may not balance the O_2 air-sea fluxes.

Per the reviewer's suggestion, we have also added more discussion to justify the proposed effect of air-sea exchanges on the pCO_2 - DO relationship. Basically, we have modeled the evolution of pCO_2 - DO relationship after a long-term air-sea exchange. The modeling results indeed shows that the high slope can be obtained after the re-equilibration with air of >15 days. We believe that by adopting the reviewer's suggestion we have strengthened our argument on this matter.

For the last comment, the 70-m isobath was used to distinguish the nearshore dataset from the offshore in this study with no special implications. It only happens around the 70-m isobath. In the modified MS, we have attempted to avoid emphasis of the 70-m isobath.

References:

Broecker, W.S. and Peng, T.H.: Tracers in the sea, Eldigio Press, Palisades, New York, 690pp, 1982.

Stigebrandt, A.: Computations of oxygen fluxes through the sea surface and the net production of organic matter with application to the Baltic and adjacent seas, Limnol. Oceanogr., 36, 444-454, 1991.

Interactive comment on Biogeosciences Discuss., 6, 6249, 2009.