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Interactive comment on “Coupling of surface $p\text{CO}_2$ 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 #4 (by Anonymous Referee)

In our paper we examined the relationship between CO_2 partial pressure ($p\text{CO}_2$) 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 $p\text{CO}_2$ 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, demonstrating a

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simple procedure to evaluate the community metabolic status based on surface $p\text{CO}_2$ and DO measurements, which may have applicability in other coastal systems with large gradient of change in their physical and biogeochemical conditions.

We fully agree with the judgment of the reviewer both about the value and about the weakness of the paper. And we have modified our statements accordingly in both the abstract and conclusion.

As for the methodology:

1. We affirm that we used an inlet temperature sensor to measure SST (before passage of water through the ship). The inlet sensor was also a standard product of Seabird. The precision has been added in the modified MS.

2. About the air-water equilibrator, we have deleted misleading phrases because the details have been introduced in our previous publication (Zhai et al., 2005a; 2005b). Basically, surface water $p\text{CO}_2$ was determined using an underway system with a continuous flow and cylinder-type equilibrator (9 cm in diameter and 20 cm long) that is filled with plastic balls and enclosed with 100 mL of the headspace (Zhai et al., 2005b). Water flow rate was set to about 4 L min^{-1} .

3. For the chlorophyll observations, we have added more information in the modified MS. Basically chl-a was determined by fluorescence analysis of discrete filtered samples, and the standard material was taken by HPLC.

4. With regard to the usage of a fixed bubble effect of 2.5% supersaturated DO. We do appreciate that the reviewer pointed out this important issue, and we fully agree that the bubble effect on surface DO may vary depending on for example 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 $0.2 \mu\text{g L}^{-1}$, 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

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(1982) and Stigebrandt (1991) should be reasonable to be applied to the study area. However, again, this supersaturation might be subject to variations given the regional heterogeneity in terms of surface wave field. As such 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 ($200 \mu\text{mol kg}^{-1}$), 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 $5 \mu\text{mol kg}^{-1}$. Unfortunately, by admitting that there is no way that we could make the correction for individual data point, 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 (in the original MS), where results of excess O_2 were presented. We must also point out that such uncertainties ($<5 \mu\text{mol kg}^{-1}$) should be minor, given the fact that the ranges of DO spatial variations were $80 \mu\text{mol kg}^{-1}$ in nearshore areas (Fig. 5a in the original MS) and $50 \mu\text{mol kg}^{-1}$ 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 $p\text{CO}_2$ and DO measurements. And the general conclusion of this study will not change.

On specific comments and questions:

1. We thank the reviewer's suggestion to check on the probability of local rainfall over the sea. If the low-salinity zone in Transect B was originated from local rainfall, the net rainfall (precipitation minus evaporation) should be as heavy as $>300 \text{ mm}$, based on the assumption that the rainfall reduced surface ($>5\text{-m}$) salinity from 33.5 to 31.5. This did not happen during the present cruise or during 10 days prior to our cruise (24 Jun - 6 Jul 2004) according to Chen and Chen (2006). In addition, based on our surface salinity profile along Transect B, the offshore low salinity area is clearly concentrated on a single point (Fig. 2b), which is difficult for a rainfall to lead to.

2. Just prior to our cruise, both primary and new production were measured in the offshore region under study. On this basis we evaluated the possible biological impacts

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on surface $p\text{CO}_2$ and DO in the offshore areas under study. Primary production (PP) was $31 \pm 12 \text{ mmol C m}^{-2} \text{ d}^{-1}$ in the basin and $72 \pm 22 \text{ mmol C m}^{-2} \text{ d}^{-1}$ on the shelf (Chen and Chen, 2006). Both are among the lowest of the world's oceans. If we use the above average photosynthetic rate and assume an euphotic layer of 100 m, the overall impact of biological activity on the surface CO_2 system may be no more than $0.72 \mu\text{mol C L}^{-1}$ ($72/100 = 0.72 \text{ mmol C m}^{-3} \text{ d}^{-1}$), which would be equivalent to $<1.5 \mu\text{atm}$ of $p\text{CO}_2$, converted from Revelle factor of 9 as discussed above or $\text{DO} < 1.0 \mu\text{mol L}^{-1}$ based on the classic Redfield ratio. If we take into consideration the respiration, the net effect of the biological metabolism might be close to zero. Indeed, the new production was determined to be only 7% (shelf) to 30% (basin) of the PP, indicating that the PP was mostly recycled on a daily time scale (Chen and Chen, 2006). Therefore, the net effects of biological activity on both $p\text{CO}_2$ and DO were minor in the outer shelf/slope and basin waters.

3. The reviewer criticized an overstated sentence in our conclusion. We agree with the comment and have deleted the sentence.

4. According to the reviewer's suggestion, we have added data maps of surface T, S, $p\text{CO}_2$ and DO in the revised MS. We also deleted the puzzling arrows in our original Figs. 4 and 5. Since the original Fig. 4 serves as a result in this study, we keep the original pattern that partitioned by transect. The offshore relationship as suggested by the reviewer had been shown in Fig. 5b in the original MS.

5. The reviewer criticized that the term ExcessO_2 is not eloquent and should either be split into two words (Excess O_2) or defined symbolically for use in equations. We now defined it as EO_2 in equations.

Last but not the least, we thank the reviewer's great efforts to provide editorial suggestions.

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