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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.

Anonymous Referee #4

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Overall comments: The paper describes pCO2 and O2 data collected during a single 17 day study in the northern South China Sea in contrasting oceanographic regions and attempts to interpret them in relation to various factors forcing these changes, with a particular emphasis on the role of production/respiration in these regions. As such, the paper addresses an interesting and relevant scientific topic, within scope for Biogeosciences. The real value of the paper is in the method applied to interpret the various contributing factors to the pCO2 signal, which will be useful for others considering similar data and questions. The use of the Revelle Factor to relate pCO2 to DO, rather than the more direct DIC relationship, is a useful approach given that pCO2 observations are far more prevalent than DIC. As such, the study could be considered

suitable for publication following the revisions suggested below and on the manuscript.

The weakness of the paper is that it is based on results from a single study and not all the variables required to accurately constrain the calculations they apply were measured, so various assumptions are required. As such, the study should be seen as a preliminary assessment of the processes in the region rather than definitive. This point needs to be made in the discussion, title and abstract.

The paper is written reasonably well, but the language is loose in places. Various editorial changes are suggested on the manuscript. The reason for selection of some terms in some of the calculations is not clear, so makes evaluation of these difficult and would not allow for reproduction in comparative studies. This can be easily addressed with better description. The approximation term ($\sim$) is over used and should be avoided in most cases. The term 'metabolic processes' is used as shorthand for production and respiration. This is not strictly accurate and the text would be improved if more precise terms were used.

Specific comments:

Methods: The precision of the various temperature sensors should be reported as well as the combined accuracy. It is not clear whether the underway CTD (thermosalinograph) temperature is corrected back to SST (allowing for passage of water through the ship) using an inlet or hull sensor. It is also not clear what is meant by "the equilibrator was exposed to the outdoor open air on deck" p6253, l4. Is this referring to a vent? It would be useful to know equilibrator flow rate. The use of the 2.5% super-saturation in these waters compared to those in the references requires further justification. The method for measuring and calibrating chlorophyll observations needs describing.

Results: P6255, l 25. Have you given any consideration to the role of local rainfall over the sea, rather than just terrestrial origin low-salinity inputs? The term ExcessO2 is not eloquent and should either be split into two words (Excess O2) or defined symbolically for use in equations.

Discussion: The basis for the primary production equations (on p6260 from line 10) needs justification (references), including how respiration is allowed for.

Conclusions: P6262, l 17-18: The statement "Our data set has, for the first time, identified different influencing processes in contrasting systems based on field-measured data in a single study." The value of this being a single study seems somewhat overstated. Actually, I see this as a limitation. It ignores factors such as diel variability (as described previously by this group, Dai et al. 2009, L&O) over the time of the study and, as the authors admit, they cannot elucidate the effects of long-distance transportation and mixing on DIC from the data they collected. Generally, syntheses of several studies can provide more robust conclusions than a single study. They are right to highlight the point that it is a single study but it should be seen as qualifier on interpreting the results too widely instead of a claim for uniqueness.

Figures: Spatial plots of surface properties (T, S, pCO2, DO) would also help to show the geographic distribution of these surface properties in relation to the water types described. Figure 4 may be clearer if partitioned for inshore and offshore water masses rather than by transect. It would help show the offshore relationships better. I find the arrows superimposed in Figs 4 and 5 hard to follow. Is there a way to make the message from these clearer?

See also the comments on the manuscript.

Please also note the Supplement to this comment.

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