Review of Zhai, Dai and Weijun Cai: Coupling of surface pCO_2 and dissolved oxygen in the northern South China Sea: Impacts of contrasting coastal processes

Below are some general comments on the manuscript. Additional comments, questions and edits are found in the attached edited version of the manuscript.

The manuscript is clear and well written. The subject deals with characterization of CO2/O2 relationships that are driven by biological and mixing processes. The authors are knowledgeable about the measurements and processes that affect the distributions of O2 and CO2. For science, they document a set of specific cases concerning these relationships over the time and space domains measured. This is valuable and demonstrates the tremendous heterogeneity in the in-water gas fields (and processes affecting them) over the area traversed during a 3-week cruise.

A few criticisms – none should be show-stoppers, but must be addressed:

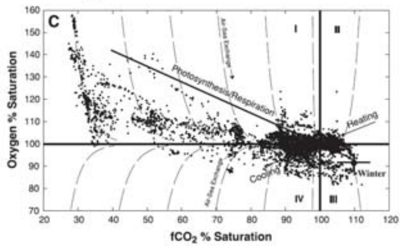
1. It should be emphasised that the observed relationships are not necessarily general, and indeed the slopes of these relationships will change dramatically as processes of growth, decay, mixing, air-sea exchange etc. proceed. When you sample during the course of a process will determine the C/O slopes. The time history of these water masses is quite important

e.g. 1 - Freshly upwelled water will have a much higher CO2/O2 than when blooming. The authors happened to catch a bloom at the upwelling site.

e.g. 2 – You can have high chl and high DIC uptake at the beginning of a bloom (low CO2/O2). However shortly after the max growth phase, chl will still be high, but the CO2/O2 will rise rapidly.

2. Why not use DIC instead of CO2? Please justify. The Revelle factors vary quite a lot in the data set, and presumably would vary even more over an annual cycle. To me it would make the manuscript easier to follow if you were tracking DIC and O2. Perhaps you have the data to decouple the biological DIC from that which is perturbed by mixing and air-sea exchange. These are the metrics the community should work toward developing.

3. Figure 4 is difficult to follow with the little arrows. Carillo et al (cited in the manuscript) had a very good way to demonstrate the same points the authors are making. I've attached the figure 3 from the Carrillo et al., 2004 paper.



4. Concerning the use of chlorophyll data:

What instrument did you use? Calibration? If it was continuous surface fluorescence, how does that relate to chlorophyll? Are you implying that a concentration of chlorophyll is related to rate of productivity? Need to be clearer about the use chlorophyll data to make implications about biological perturbations of DIC.