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Interactive comment on "Carbonate mineral saturation states in the East China Sea: present conditions and future scenario" *by* W.-C. Chou et al.

Anonymous Referee #2

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This study presents variability of the carbonate system in both spring and summer of 2009 over the East China Sea shelf aiming to assess the impact of rising atmospheric CO2 and eutrophication on the saturation state (Ω). The topic is timely and sensible in the context of increasingly recognized multiple stressors in the coastal ocean environment. At the same time, I have some major concerns about this study in its present stand. My major concerns are detailed in specific comments (4) and (6).

Specific comments:

(1) The present introduction is full of well known carbonate chemistry. I see saturation state is the central theme of the paper and thus authors should make efforts to define

the fundamental questions related to saturation states under the stressors of anthropogenic CO2 and nutrients ladening, and how this study would address the issues.

(2) P5562, L7-8: Here, [Ca2+] is calculated by salinity according to Riley and Tongudai (1967). I suspect that Ca2+ may not be that conservative in the present complex system in particular in the bottom water where dissolution of CaCO3 may well occur. This could significantly change your calculations of the saturation state.

(3) P5562, the results section: authors did not at all present the basic hydrology of the region during their sampling, which is clearly problematic if they are to examine the complicated hydrochemistry and the carbonate chemistry therein.

(4) P5566, L10-& Fig 5: here the mixing processes between different water masses are critically important (if not the foundation of this study) to further evaluate the changes in carbonate parameters caused by biological processes, e.g., ΔDIC & $\Delta\Omega$. Both the freshwater end-member for the Changjiang runoff and the seawater end-member are subject to large temporal variations, which should be very carefully justified and the implications of such variability to their conclusion should be quantitatively assessed. By the way, how does the TA-S curve look like? Did the authors assume TA is conservative here? If the authors cannot lay a concrete foundation of these mixing processes, the main conclusion of the study would be unfounded. Similar concerns also go to p5571 for the scenario analysis. I also have troubles with the future trend of DO declining rate being assumed to be 0.72 umol/yr in the bottom water. I did not check back into Ning et al. (2011) but such extrapolation could be very misleading because so far it is very difficult to even define the changes in areas of the hypoxic zones at inter-annual time scale.

(5) P5566, L5-7: please show Chl-a and DO data to support your conclusion.

(6) P5567, L20-24, the assumptions of the projections: (1) the assumption of air-sea equilibrium may be overall OK at longer time scales. But here the authors are projecting both summer and spring, or, at seasonal time scale without appropriate justification of

the assumption. It is well known that air-sea CO2 exchange takes long time depending on the mixed layer depth, wind stress and chemical buffer in the seawater. (2) constant TA may not be valid in the context of rising air CO2, in particular when the authors are looking at changes at seasonal time scale. (3) Based on what, the authors would assume a SST rise of 2 degree C at air CO2=723 ppmv. Note that 2 degree C is a very high rise for the ocean.

(7) P5572, L14: Considering the DO consumption will add additional DIC to $\triangle$DICac, you should also take account the change in TA given the high biological productivity in the surface and high respiration in the bottom water.

Interactive comment on Biogeosciences Discuss., 10, 5555, 2013.