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Interactive comment on "Impacts of a weather event on shelf circulation and CO_2 and O_2 dynamics on the Louisiana shelf during summer 2009" *by* W.-J. Huang et al.

Anonymous Referee #2

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The authors use observations of salinity, pCO2, and O2 to speculate on the relationship between CO2 and O2 in the context of shelf dynamics. The authors correctly identify 2009 as being an anomalous year, as compared to 2007, which was relatively normal. This study does not add anything new to the literature, in terms of identifying 2009 as an anomalous year. Also, the authors are incorrect or misleading in some of their representations of the circulation patters. For this reason, I recommend to reject this paper.

In terms of this distinction between the years, there is nothing novel about this study. Previous studies, cited by the authors, have identified the role of wind in shifting the position of the plume, and have identified this as a dominant factor in controlling hypoxia

on the Louisiana shelf. This study adds nothing new to that line of reasoning. Note that the discharge in 2009 was quite high, so that the riverine nitrogen load to the system was almost double that of 2007. The authors discus at length how the shifting of the plume will affect productivity on the shelf. It seems it would be best to do this analysis relative to salinity, as following particular water masses at a particular salinity will allow them to follow the high productivity regions of the plume as it is shifted westward by anomalous winds in 2009.

None of the analyses are particularly enlightening. Figure 6 plot of winds offshore of Galveston, for instance, seems far from the hypoxic region to be relevant. Also, previous studies have identified east-west winds as the dominant factor controlling hypoxia, and the authors comment on the persistence of the southerly winds. The difference in this analysis with previous studies was not explained. Figure 7 shows surface-to-bottom density differences. However, as there is an ambient vertical stratification, these differences will basically just be correlated with depth; the stratification occurs uniformly in deeper water.

The relationship between DO and DIC is interesting, but not explained or highlighted enough in the paper. Clearly, there will be some component of this relationship that will lie upon a mixing curve, biological processes will be responsible for the rest. But this relationship is not discussed in a way that highlights any of the processes that occur, where they occur, or how the physics and the biology are interrelated.

I think the authors have collected a very interesting data set, and I would like to see results from this study published. However, the present analysis does not add anything to our knowledge of the northern Gulf system that was not already previously published.

Interactive comment on Biogeosciences Discuss., 10, 19867, 2013.