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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.

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While we agree that this study needs to be improved in presentation and some science points need be further polished, we feel the referees' comments are not constructive and their final decisions are not supported by their comments. Our paper presents sea surface pCO2 and water column DIC and DO distributions and how a coastal current event altered the normal biogeochemical processes in July 2009 (as compared to August 2007), which eliminated the normally observed low pCO2 in sea surface and the low O2 and high DIC in bottom waters in the Mississippi River plume and the Louisiana Shelf. We used wind and current data to explain the unusual biogeochemical

properties. We emphasize that this is not a study of physical process (the coastal current), but rather, how it affects coastal ocean biogeochemistry. This study has its clear novelty in displaying and discussing more than previously published relationship between wind, surface pCO2, and water column DIC and O2 biogeochemistry. There has been no such a study in the Mississippi River plume or any other plume. The issue related to writing structure/style can be fixed (as suggested by the first reviewer). While we do not understand the review process, we truly feel this is unfortunate, especially when some comments are strange. As the conclusion has been made (rejection) and the comments are not constructive, we will not provide detailed rebuttal and this does not mean we agree with these comments.

To Referee 1,

We thank this reviewer's suggestion to restructure the presentation. This referee thought that we should use data from our previous publication from Guo et al (2012). Actually, previous paper of Guo et al. only presented surface data in a limited area (from previous years) close to the Louisiana Bight and its adjacent area. This is the first DIC and DO data over the Louisiana shelf.

For the specific comment 3: We already compared the nitrogen flux in May. They are in the introduction, Page 4, Line 1 to 4. "The associated inorganic nitrogen flux in May (a time best correlated with plume phytoplankton production during summer) was comparable between the two years compared in our study, 1.48$\times$1011 gN in 2007 and 1.55$\times$1011 gN in 2009 (US Geological Survey data)."

For comment 5. This information can be found easily at Page 10, Line 9-10.

For the specific comments 1, 2, 4, 6, and other comments for Figures, these can be added and fixed easily. Adding more statistic work could be more convincing but will not change the conclusion of this study, especially when these regional variations are apparent.

To Referee 2,

We accept that this paper is not perfect and needs to be improved. We should highlight our DIC and DO more than those physical processes. But this referee's comments (no novelty) do not have substance and do not support his/her final decision. This study has its novelty: first, we reported, for the first time, high resolution underway surface water pCO2 in summer and DIC and DO in the water column in this region. Second, we showed how such unusual weather event affected the air-sea CO2 flux on the shelf. (There has been no such a study in the Mississippi River plume or any other plume.) Third, we demonstrated the relationship between wind distribution, surface plume distribution, and DIC and DO in bottom water. The reviewer suggest that we report a physical process occurring in July 2009, which has been report recently by Feng et al. (2013) and nothing is new. This is clearly a misunderstanding of our paper. Our in-situ measurements and satellite information crossing the shelf showed totally different dataset from that model study (Feng, et al. 2013).

This referee stated that "Note that the discharge in 2009 was quite high, so that the riverine nitrogen load to the system was almost double that of 2007". This suggestion is strange. We have stated in the introduction, Page 4, Line 1 to 4. "The associated inorganic nitrogen flux in May (a time best correlated with plume phytoplankton production during summer) was comparable between the two years compared in our study, 1.48×1011 gN in 2007 and 1.55×1011 gN in 2009 (US Geological Survey data)."

Our use of physical and environment data (wind and current) can be improved in various ways, but that is not a good reason to kill a paper that uses physical data to study biogeochemistry. A constructive suggestion on how to use physics data better would be much appreciated.

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