Review of Xue et al, "Modeling pCO$_2$ Variability in the Gulf of Mexico"

This revised manuscript addresses an important issue, namely, the status of the Gulf of Mexico as a regional source or sink of atmospheric carbon dioxide. The authors use a modeling approach to derive sea surface pCO$_2$ from total alkalinity and dissolved inorganic carbon, which is a reasonable approach given the lack of direct pCO$_2$ observations in many areas of the Gulf. The authors have done a good job to address the points raised by reviewers of the original manuscript, particularly regarding the atmospheric pCO$_2$ data source. As discussed below, I think more discussion of the relationships used to derive total alkalinity and dissolved inorganic carbon, especially the strengths and weaknesses of these relationships in the Gulf of Mexico, is warranted.

Major Comments:

-I was surprised by the data presented in Figure 4, specifically the Mississippi/Atchafalaya River DIC input for the 1904-1910 and modern time periods. The river DIC flux appears largely similar, on average, between the two time periods. It was my understanding from the paper by Raymond et al. (Nature 2008) that DIC flux from the Mississippi increased substantially over the preceding century- an increase that the DLEM model used in this work does not seem to capture. This contrast is worth noting and discussing, although the discussion of results from the 1904-1910 simulation was very limited.

-The validation data for the WGoM and MX regions, and to a lesser extent the WF regions, are extremely sparse. Also, there is a very large region of the south/western Open Ocean region with little to no validation data. I acknowledge that the utility of a model lies in the opportunity to estimate conditions in unsampled times/regions, but all those empty spaces leave a lot of uncertainty. The WGoM, MX and WF regions are not specifically addressed at all in the Discussion section. I imagine that the overall Gulf of Mexico results/fluxes would not change substantially if these three regions were excluded from the totals, and just the Open Ocean and NGoM results were used. This comparison (Open Ocean+NGoM vs. the entire GoM) would be useful to present.

-The model presented in this manuscript relies on the empirical relationships of Lee et al. (2006) to determine total alkalinity and Lee et al. (2000) to determine total inorganic carbon boundary conditions. However, these relationships specifically exclude all of the Gulf of Mexico for total alkalinity, and about half of the Gulf of Mexico for inorganic carbon. The authors should explicitly describe which relationship/region from each Lee et al. paper was used in this work, and justify the choice to use these relationships. The authors should also explicitly describe that total alkalinity was parameterized from salinity and surface temperature (Lee et al. 2006), while inorganic carbon was presumably parameterized from just surface temperature (and not temperature and nitrate, Lee et al. 2000). This helps explain the larger apparent variability in DIC when compared to total alkalinity in Figure 9.

-Section 4.1 discusses Mississippi River discharge and pCO_2 results, yet pCO_2 is not shown in Fig. 4 where it is referenced. Instead NO3 is shown. This needs to be fixed.

Minor Comments

-The manuscript could use another proof-read. Some instances: Abstract L32 change to "Two model sensitivity"; P3L44 "In the face"; sP6L124 change were to was; P7L133 change to 'regardless of which'; P7L138 change to calculations; P7L149 change to relatively; P8L173 change composed to compiled; P10L204 change to 'model results in shallow'; P12L259 change to "uniform"; P12L268 change was to were; P13L271 change to 'rest of the seasons'; P13L291 change to pCO_2; P14L293 change to pCO_2; P15L325 change to times; P18L390 change were to was; P19L425 change to relatively

-I echo the sentiment of the Anonymous Reviewer #2 that details of the biogeochemical component of the model are needed.

-P3L63: the authors may want to elaborate on how the loading of carbon causes eutrophication. I generally associate the term eutrophication with nutrient loading; the contributions of carbon (presumable organic) are more indirect that nutrient additions.

-P4L65-70 and throughout: I believe the authors are generally using the term 'carbon' in this paper to refer to inorganic carbon. If so, this should be made explicit, and care should be taken when describing total or organic carbon, as I presume this section does. Another example is P13L284: presumably the authors are talking about a CO2 sink, instead of an inorganic or total carbon sink.

-P4L71-76: Please show the Loop Current in Figure 1, for those like myself who are less familiar with Gulf of Mexico circulation.

-P5L95: This is a nitpick, but I believe one does not quantify pCO_2 fluxes, as pCO_2 refers to the partial pressure. Instead it is the CO2 flux which was quantified.

-P5L107-108: Please explain exactly what the terms "realistic atmospheric forcing" and "open boundary conditions" refer to.

-P7L154: Please list the major rivers whose observations were taken from USGS data.

-P8L167: Should be Exp3.

-P9L187: Specifically name the model simulation as the control (Exp1).

-P9L187-197: Why not show the model pCO_2 results from all sub-regions in a table, as the fluxes are shown in Table 1?

-P10L222-223 and throughout: Please add uncertainties to the average fluxes listed here, as variability is quite important in understanding these results.

-P15L329-336: This is a very nice section of the discussion, and would benefit even more from additional description of the biogeochemical component of the model, as mentioned earlier in my review.

-Figure 2: Panel A is labeled TIC (total inorganic carbon), while the manuscript discussed DIC.

-Figure 3: Is there a way to show the 1904-1910 atmospheric pCO_2 used in the model? Perhaps as an inset?

-Figure 6: The shading showing the model uncertainty is very light, and may not print well. Can it be darkened?

-Figure 10: I'm not quite sure what this figure is showing. What does each data point represent: one grid cell in each sub-region? The caption describes seasonal means from 2005-2010, but that would produce 24 data points for each sub-region, and there are many more data points shown here. Are these data points averaged over some sort of longitude range?

-P41L700: Isn't salinity technically unit-less?

-P42L713: I believe Alk should be in units of micro-equivalents per liter, or milli-equivalents per cubic meter)