

Re: Modeling pCO₂ Variability in the Gulf of Mexico (bg-2014-391)

Dear Authors,

Thank you for your revised MS of "Modeling pCO₂ Variability in the Gulf of Mexico (bg-2014-391). I carefully went through your responses to the reviews as well as your revised MS, and I appreciated that you have well addressed most of the comments raised by the reviewers. I therefore see the potential that I can accept the paper for publication at BG after moderate revisions. Having said so, I urge you to consider the following concerns of mine along with some additional specific comments.

1) General comments:

While I am aware that this modeling effort is large and significant, and your validation shows overall good performance of the model, I would suggest to tune down the significance in quantifying the fluxes (throughout the MS but particularly in the introduction) and balance it with the community consensus that numerical model is a tool particularly compelling for process study. This is partially because the biogeochemical module is far from being realistic even though you have used realistic physical forcing.

2) Specific Comments:

2.1. abstract: Line 30-31: "On average, the GoM was found to be a CO₂ sink with a flux of, which, together with the enormous fluvial carbon input, was balanced by the carbon export through the Loop Current". This statement is not accurate. First of all, the form of carbon (organic vs inorganic) being referred here is unclear.Secondly, because of the involvement of the biological metabolism, CO₂ flux is actually balanced by the externally transported DIC plus the balance between DIC and nutrients metabolism (Dai et al., 2013, GRL).

3) About the model implantation:

3.1 P8, lines 173-175, "Because direct riverine DIC measurements were not available, we approximated riverine DIC ...", this has to be justified and the potential impact be evaluated as it is known pCO₂ is highly sensitive to DIC changes as indicated by the Revelle factor. Discussion about the uncertainties should be made if the approximate DIC has to be used.

3.2 P9, lines 181-183, "Experiment 2 (Exp2) was a "no-biology run", where all biological sources and sinks of DIC and alkalinity were disabled ...", disabling the biological sources and sinks of DIC and alkalinity can only reflect the biological effect qualitatively or quasi-quantitatively due to the non-linear relationship between them. Moreover, the biological effect interacts with other factors/processes such as temperature and air-sea flux, therefore, discussing the biological effect by disabling the biological process alone is not quite assuring.

3.3 P9, lines 183-186, "Experiment 3 ... the river inputs ... for the period of 1904-1910", except for the riverine input and air pCO₂, air temperature and seawater temperature also changed substantially during the last 100 years. Furthermore, the nutrient condition and carbonate system of the GoM may also have changed after 100 years' change in terrestrial input. Therefore, applying river input and air pCO₂ at that time and spin up for only 1 year is far from enough to simulate the coastal carbon dynamics a century ago.

Please consider to further revise your MS and resubmit your revisions within 3 weeks. This paper has to reach a final decision real quickly,

Sincerely,

Minhan Dai