Biogeosciences Discuss., 11, C5679–C5681, 2014
www.biogeosciences-discuss.net/11/C5679/2014/

Biogeosciences Discussions
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Interactive comment on "Modeling pCO₂ variability in the Gulf of Mexico" *by* Z. Xue et al.

Anonymous Referee #1

Received and published: 4 October 2014

This manuscript by Xue et al. examines the pCO2 variability in the Gulf of Mexico by using a coupled physical-biological model. I have read this paper a few times, but I still don't think I can recommend it for future publication, as I found there are some major issues with this work including both the model settings and the modeling results. My detailed comments are listed below:

1. The initial and boundary conditions for DIC simulation are questionable. The model uses empirical relationships between carbon terms and temperature or salinity to estimate initial and boundary conditions for DIC, which could somehow confound the anthropogenic CO2 signal in the model. For the initial condition, this kind of estimation should come from the relationship derived in the initial model year, that is, if the model starts in 2004, then the DIC-Temp or DIC-Salt-Temp relationship needs to come from

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2004. Climatological DIC-Temp or DIC-Salt-Temp relationships are not appropriate for anthropogenic CO2 simulation, especially when the model tries to resolve interannual variations. If you use climatological relationships, you need to spin up the model much longer instead of one year. This is even more important for boundary conditions. Using a fixed relationship between DIC and temperature or salinity for boundary cannot accurately resolve the anthropogenic signal coming into the model domain, which can further introduce large errors for pCO2 and CO2 flux calculations. I believe the authors already realize the importance of initial conditions for DIC and alkalinity in the biological model simulation.

2. The model uses a fitting curve to represent atmospheric pCO2. Although the spatial variability of atmospheric pCO2 is much smaller the oceanic pCO2, has this relationship ever been verified in the GoM? I ask this question because the accuracy of the sea-air CO2 flux depends on the quality of atmospheric pCO2 used in the model.

3. The modeled-data comparison for pCO2 seems not quite well. In Fig.1a on Louisiana shelf, there is only one measurement point in summer showing high value but with a large std. From the rest of years' measurements, it is really hard to see a summer pCO2 peak, while the model produces a clear and strong summer peak. The Gulf-wide pCO2 looks a little better, but still the model indicates stronger seasonal variability than observations. I think reproducing the seasonal variability not just the mean level is quite important when evaluating a model, and is usually considered as the first priority while tuning a model. The authors already mentioned the model resolution and the structure of biological model could be the error sources, so I suggest the authors to continue refining the model.

4. The modeled-data comparison for CO2 flux seems not well either. Almost all of the CO2 flux differences between model results and observations are larger than a factor of 2, except one on Louisiana shelf. For example, in table 1, modeled annual values are 1.09 and 0.06 for Mexico shelf and Texas shelf, while observed values are 0.09 and 0.18. To me, the model can only produce the source and sink patterns for CO2 flux

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qualitatively, but not quantitatively.

Interactive comment on Biogeosciences Discuss., 11, 12673, 2014.