

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2021-339

Anonymous Referee #2

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Referee comment on "A Numerical reassessment of the Gulf of Mexico carbon system in connection with the Mississippi River and global ocean" by Le Zhang and Z. George Xue, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-339-RC2>, 2022

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Review of "Downscaling CMIP6 Global Solutions to Regional Ocean Carbon Model: Connecting the Mississippi, Gulf of Mexico, and Global Ocean"

This paper presents a 20-year simulation of the Gulf of Mexico with a coupled physical biogeochemical model including carbon chemistry. The paper provides a validation of the model against observations. It also presents two perturbed experiments, "Bry" and "NoR", "Bry" has fixed the DIC and TA boundary conditions for the year 2000 and "NoR" has no rivers. Furthermore, the carbon budget and how different processes such as temperature, primary production and mixing attribute to the total is presented, which was very interesting to see. The presentation is focused on the Northern Gulf of Mexico and the open ocean Gulf of Mexico, though some other regions are also discussed. This work is interesting and worthy of publication, but a major revision of how the results are presented is necessary before publication. Here are the major points:

- The title promises a downscaling of the Gulf of Mexico using a regional model. I was expecting an actual downscaling in a global model, including a historical simulation and forward projection. However, the presented model appears more like a hindcast forced by NCEP reanalysis with initial and boundary conditions from the climate model rather than a full downscaling. Several questions arise related to this:
  - Why was this climate model selected?
  - Why was a climate model selected for the boundary and initial conditions as opposed to either climatology or a global ocean reanalysis?
  - What is the biogeochemical model that was used in the climate model and is it of a similar complexity to NEMURO?
- Validation: It is very good that more than one type of observation is used, but the results are presented only graphically. I would like to see some statistical quantities such as bias and rmse. Additionally, the differences could be shown: for example in Figure 5, I would have preferred to see seasons rather than month, but then adding the plots for differences.
  - An important motivation for doing the study was the improved quality of this downscaling compared to the earth system model, to demonstrate that, also some

error estimated from the ESM should be included for comparison.

- Structure: In the results the main run is presented and then discussed against previous estimates. Then in the discussion, results from the two perturbed experiments are presented. In my view the discussion of the main run against other studies belongs in the discussion, while the presentation of the perturbed runs belongs in the results.
- Clarity on analysis: I spent a fair amount of time trying to understand the distinction between the different “types” of CO<sub>2</sub> in the analysis as described by equations 4, 5 and 7. There is no equation (6) it seems. First of all, these could all be presented together and some work is needed to make this more understandable. Additionally it would help if in table 3 also the actual pCO<sub>2</sub> was presented. I assume the triangular bracket is the temporal mean, but it should be stated. Why compute the contribution from GPP and not NPP? Furthermore the thermal contribution I would understand as “How much higher or lower the pCO<sub>2</sub> is because the temperature is either higher or lower than the mean”. Here however the number presented is of same order as the actual pCO<sub>2</sub>. I would also expect that the thermal and the non-thermal part would add up to total pCO<sub>2</sub>, however adding equation 4 and 5 does not yield pCO<sub>2</sub>. So better explanation is needed in this part. I find the labeling “mixing” of the last pCO<sub>2</sub>-term presented in eq. 7 misleading, see my comment further down.
- The results are very focused on the surface, it would have been interesting to see more of what goes on below the surface, for example it would be interesting to see the depth of the dissolution horizon for calcite and aragonite.

### **Specific comments:**

### **Title**

“...Model: Connecting the GoM to the Mississippi and the Global Ocean” would be more correct as this study does not address the influence of GoM on rivers or the global ocean.

As mentioned above, “Downscaling CMIP6” is misleading as to the nature of the simulations presented.

### **Abstract**

Line 8: “...reduce uncertainties in spatial...” I do not agree that models reduce uncertainties in estimates, they do however complement observations to fill spatial and temporal gaps in the observation record (with some uncertainty).

Line 20: “confirms”: Also write what it confirms, previous models, observations, both?

Line 23: Be more specific on how the Mississippi inflow influences the carbon cycle.

The last sentence seems obvious as those are the places with inflow to the GoM, but really, when comparing "His" and "Bry", the results are so close to each other I would say a more accurate conclusion from that perturbation experiment is that interannually varying lateral boundary conditions are not necessary on this timescale.

## **Introduction**

Line 39 "works" should be "studies"

## **Methods**

Line 120: Be more specific: which variables were originally in NEMURO, which have been added?

Line 134 and onwards: could be helpful with a table where each process added is connected to the relevant publication.

What is the temporal resolution of the boundary conditions?

## **Validation**

Figure 4: Stretching the y-axis on the upper part of the water-column and putting a black dot in the middle of the observation circle could help to better visualize the difference between the model and observations.

## **Results**

Figure 7: Why are  $p\text{CO}_2^{\text{th}}$  and  $p\text{CO}_2^{\text{nt}}$ , which have a very similar range, displayed with different colormaps? That makes them hard to compare.

Line 338: Suggest "combination" instead of "synthesis"

Line 405: Is this the timestep of the NEMURO, it would be more appropriate to mention this in "Methods".

Line 414: Please, spell out approximately how large the error is in Gomez (2020) compared to what is the typical discrepancy of this study.

Line 415-422. The region considered by the observational studies are not mentioned, is it gulf-wide? It is correct that this study gives a larger estimate on the shelf, but according to table 2, the open ocean and Gulf-wide estimate is larger in Xue 2016. So is it correct to conclude that this model is "more reliable" everywhere?

## **Discussion**

Line 440: What is the meaning of "heterogenous" here:

Line 441" It can also be considered as the pCO<sub>2</sub> level determined by the water with a multiyear mean temperature and without the influence from gross primary production or air-sea CO<sub>2</sub> flux." But other processes than mixing, for example horizontal advection, will contribute to this pCO<sub>2</sub>, therefore I find labeling this term "mixing" misleading.

The authors discuss "sources of uncertainty to the model", but they do not present actual model uncertainty, so I would change the heading 5.1 accordingly.

## **Conclusion**

Line 565: As mentioned below the abstract, I disagree with the conclusion that the boundary conditions had a very strong effect on the results, maybe on longer time-scales, as the trends changed slightly, but not for a 20-year simulation.

