Thanks for these comments! We found the two reviewers shared the same insights in several points; in such cases, we will direct our responses to Reviewer #1 to avoid redundancy.

RC2: 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?

Response: We will correct the usage of the "downscaling" terminology, as suggested by both reviewers. Please refer to our detailed response to Reviewer #1 on selecting the GCMs as the boundary condition among available global climatology/ reanalysis products. The biogeochemical model used in this study is built upon the NEMURO (Kishi et al., 2007), and is of similar complexity. NEMURO model originally configured eleven state variables including nutrients (Si(OH)4, NO3, NH4), plankton groups (ZP: predator zooplankton, ZL: large zooplankton, ZS: small zooplankton, PL: large phytoplankton, PS: small phytoplankton), dissolved organic nitrogen (DON), particulate organic nitrogen (PON), and opal (OPL). We noticed a typo in the manuscript that misspelled eleven as seven. The added carbon cycle is linked with the original nitrogen cycle with a fixed ratio (the Redfield ratio of C: N = 6.625). Table B1 lists each added variable and their referenced studies.

Table B1 Newly Introduced Tracers to NEMURO by this study

Variables added to NEMURO model	Reference
Dissolved Inorganic Carbon (DIC)	Respiration and remineralization linked with the nitrogen cycle;(Moore et al., 2004)
Total Alkalinity (TA)	Alkalinity generation in sediment processes (Fennel et al., 2006; Hu & Cai, 2011); (Moore et al., 2004)
Dissolved Organic Carbon (DOC)	(Moore et al., 2004)
Calcium Carbonate (CalC)	Calcification rate (Moore et al., 2004); Dissolution (Mucci, 1983; Millero, 1982, 2007,

	1995);
Phosphate (PO4)	(Moore et al., 2004)
Oxygen (O2)	Linked with carbon/ nitrogen stoichiometry in aerobic respiration or photosynthesis Followed oxygen air-sea flux parameterization (Wanninkhof, 1992)

RC2*: 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.

Response: Thanks for the suggestions. Reviewer #1 also has the same advice on adding statistics to model-data comparison. Please refer to our response to this point suggested by Reviewer #1. We plan to add model *skill* (Eq. A1), the Taylor Skil Score (TSS) (Eq. A2), and commonly used statistical metrics such as correlation coefficient (R), Root Mean Square Error (RMSE), and the standard deviation (STD) to evaluate the model performance compared to the GCMs and global climatology products. For Fig. 5, we plan to use seasonal means and add subplots for the difference between the two products. Regarding the comparison of the improved quality of our downscaled model between the global model, please refer to Figure A4 in our response to Reviewer #1.

RC2: *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.*

Response: The structure issue of mixing results and discussion will be corrected. We will compare with other studies in the discussion section and present the results of the perturbed runs in the result section.

RC2: *Clarity on analysis: I spent a fair amount of time trying to understand the distinction between the different "types" of CO2 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 pCO2 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 pCO2 is because the temperature is either higher or lower than the mean". Here however the number presented is of same order as the actual pCO2. I would also expect that the thermal and the non-thermal part would add up to total pCO2, however adding equation 4 and 5 does not yield pCO2. So better explanation is needed in this part. I find the labeling "mixing" of the last pCO2-term presented in eq. 7 misleading, see my comment further down.*
Response: All equations concerning different components of CO_2 will be presented together, and pCO_2 thermal will be re-defined to allow a sum of all CO_2 components to add up to the apparent CO_2. The actual/apparent pCO_2 will be added in Table 3 for better comparison. The contribution from gross primary

production (GPP) is used because this is the process that directly affects the CO_2 uptake. Respiration in the ocean water column is much more complex to keep track of than that of photosynthesis. In the model, GPP can be conveniently calculated by tracking the photosynthesis activity of diatom and small phytoplankton (which is a function of solar radiation, temperature, nutrients, and phytoplankton concentrations). Respiration concerns both living biota (phytoplanktons, zooplanktons) and nonliving detritus (particulate organic matter, dissolved organic matter). More importantly, it would be problematic to only account for surface respiration, considering that most detritus sink and respire in deeper water. It seems to be more appropriate to leave the respiration in the end-member of the CO_2 components. In other words, net primary production (NPP) is not a readily calculatable quantity in the model. Because respiration can be allochthonous through advection or sinking (depth-dependent), it can be misleading if incorporated in surface spatial presentation. We used GPP as a component of CO_2, measuring the intensity of photosynthesis, which is a primary driver for surface ocean pCO_2 dynamic in the Gulf of Mexico. The label of the last pCO_2 term was "mixing", which represents various mixing processes (e.g. river water and oceanic water mixing, vertical mixing of upwelled waters, horizontal advection induced lateral transport of tracers with concentration gradients, and entrainment of waters with different chemical nature (i.e. temp/ salt/ DIC/ TA/ detritus concentration)). Remineralization and respiration are included in this term due to the result of the two processes altering water chemical nature (DIC, TA, detritus concentration), and the impact from water chemical nature on pCO_2 is constantly being modified by (and as a result of) the mixing process. We understand the reviewer's concerns since the typical effects from horizontal advection and vertical mixing are distinct enough to be treated separately, especially in the global models. Still, we cannot find a better representation to serve as the label. Also, similar classifications or labels are used in literature (Meléndez et al., 2022; Wanninkhof et al., 2019), and in these studies, horizontal advection is considered as included in the mixing term. As a result, we appeal to keep the "mixing" label.

pCO_2^{th} was originally defined as the effect of temperature changes on pCO_2, and the pCO_2^{nt} was defined to remove the temperature effect from the observed pCO_2 (Takahashi et al., 2002). Numerous literatures followed these definitions (Fay and McKinley, 2017; Landschützer et al., 2018; Lerner et al., 2021; Yao and Hu, 2017).

Although our previous definition of pCO_2^{th} and pCO_2^{nt} followed Takahashi et al.(2002), we understand the review's perspective. We would like to re-define the pCO_2^{th} to make the two parties sum up to the apparent pCO_2. Therefore we modified the definition of pCO_2^{th} as equation (B1). And we will modify Fig. 8 with the updated definition and unified color schemes. In equation (B1) and (B2), <SST> denotes the mean SST value over the studied period.

$$pCO_2{}^{th} = pCO_2 \cdot [1 - exp(\gamma_T \cdot (< SST > - SST))] \quad \text{(B1)}$$

$$pCO_2{}^{nt} = pCO_2 \cdot exp(\gamma_T \cdot (< SST > - SST)) \quad \text{(B2)}$$

RC2: *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.*

Response: We included vertical transect profiles of DIC, TA validation in our original submission and would like to show more below-surface results. However, there is a lack of measurements for subsurface and especially for water column that is deeper than 200 m. Here we show a vertical profile of calcite and aragonite saturation along the 200m isobath in Figs. B2 and B3. The coastal ocean (<200 m) remains over-saturated with calcite and aragonite throughout the modeling years and in the acidified bottom layer. The depth of the dissolution horizon for calcite and aragonite in the deeper ocean has not been validated.

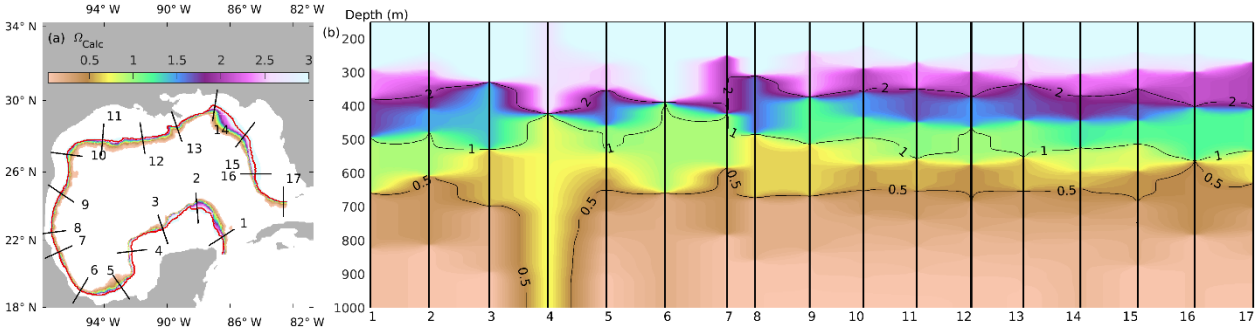

Figure B2. Multiyear averaged Calcite saturation state (2001-2019) along the 200 m isobath (red line in (a)) at the sea bottom in the 150-1000 m depth range (a) top view (b) stretched view. Intersections are chosen along the 200 m isobath line with direction normal to the 200-isobath tangential.

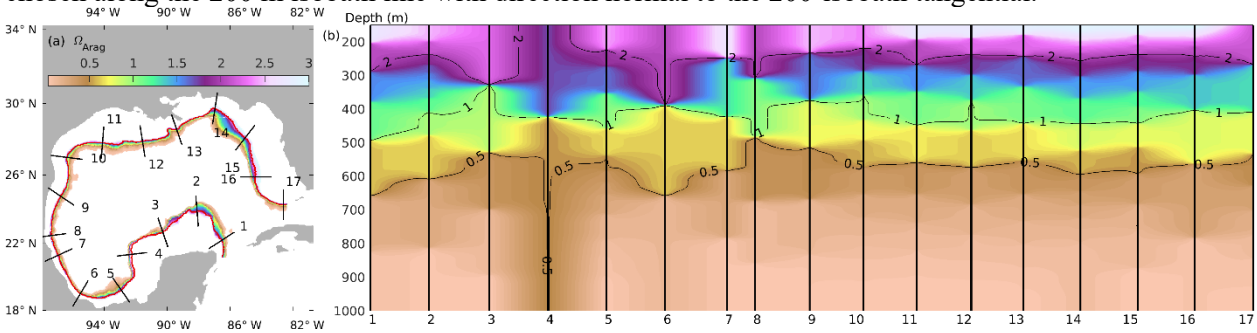

Figure B3. Multiyear averaged Aragonite saturation state (2001-2019) along the 200 m isobath (red line in (a)) at the sea bottom in the 150-1000 m depth range (a) top view (b) stretched view. Intersections are chosen along the 200 m isobath line with direction normal to the 200-isobath tangential.

RC2*: Title "...Model: Connecting the GoM to the Mississippi and the Global Ocean" would be more correct as this study does not address the influenc of GoM on rivers or the global ocean.*

Response: As mentioned above, "Downscaling CMIP6" is misleading as to the nature of the simulations presented. We intend to change the title into "A Re-assessment of the Gulf of Mexico (GoM) Carbon System: Connecting the Gulf of Mexico with the Mississippi River and the Global Ocean".

RC2*: 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.

Response: we will remove the assertion on "reduce uncertainties", and highlight the contribution to fill spatial and temporal gaps in the observation record. We will articulate what this model confirms – this model confirms with several previous models and ocean surface pCO_2 observations that the river-dominated northern GoM (NGoM) is a substantial carbon sink, and the open GoM is primarily controlled by thermal effect. We will be more specific on how the Mississippi inflow influences the carbon cycle in the revision in three facets, namely the coastal biogeochemical processes, river DIC/DOC/TA budget

delivered to the coastal ocean, and atmosphere-ocean-sediment carbon fluxes. Nutrient-fueled primary production removes DIC from surface water and extracts carbon from the atmosphere. Sinking organic matter undergoes decomposition and burial at the ocean bottom, contributing to bottom hypoxia and acidification. Sediment processes remineralize organic carbon, possibly dissolve particulate inorganic carbon, subsequently alter bottom water DIC/TA concentration. The difference between "His" and "Bry" experiments is more than just between multiyear means, which will surely be subtle. Clear differences in interannual carbon system variables due to dynamical boundaries at abnormal years can be observed before such signals are erased in averaging. Due to this reason, we still hold the opinion that interannually varying lateral boundary conditions are necessary for this 20-year model.

RC2*:Introduction Line 39 "works" should be "studies"*
Response: Thanks for the correction, we will change the "work" in Line 39 into "studies".

RC2*: 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?

Response: Variables originally in NEMURO: large phytoplankton, small phytoplankton, predator zooplankton, large zooplankton, small zooplankton, opal, DON, PON, Si(OH)4, NO3, NH4; Added variables: DIC, TA, DOC, CaCO3, PO4, O2. We added Table B1 elaborating where each process is connected with the relevant publication (see Part I). The temporal resolution of the boundary conditions is monthly for ecological variables (DIC, DOC, TA, NO3, Si(OH)4, PO4, NH4) taken from GCM. The temporal resolution of the boundary conditions is static from WOA for O2. The temporal resolution of the boundary conditions is daily for physical variables taken from HYCOM (temperature, salinity, zeta, u, v, ubar, vbar). The temporal resolution of atmospheric forcing is 6-hourly from CFSR, CFSv2 (shortwave radiation, longwave radiation, UWind, VWind, precipitation, air temperature, air pressure, humidity). (see Table A4 in response to Reviewer #1)

RC2*: 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.
Response: Thanks for the detailed suggestions. We will stretch the y-axis on the upper part (<200 m) of the water column and put a black dot in the middle of the observation circle in Fig. 4.

RC2*: Results*
Figure 7: Why are pCO2th and pCO2nt, 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?

Response: We will make changes to the pCO_2^{th} definition and use a unified color scheme for Fig. 7 (see Fig. B4).

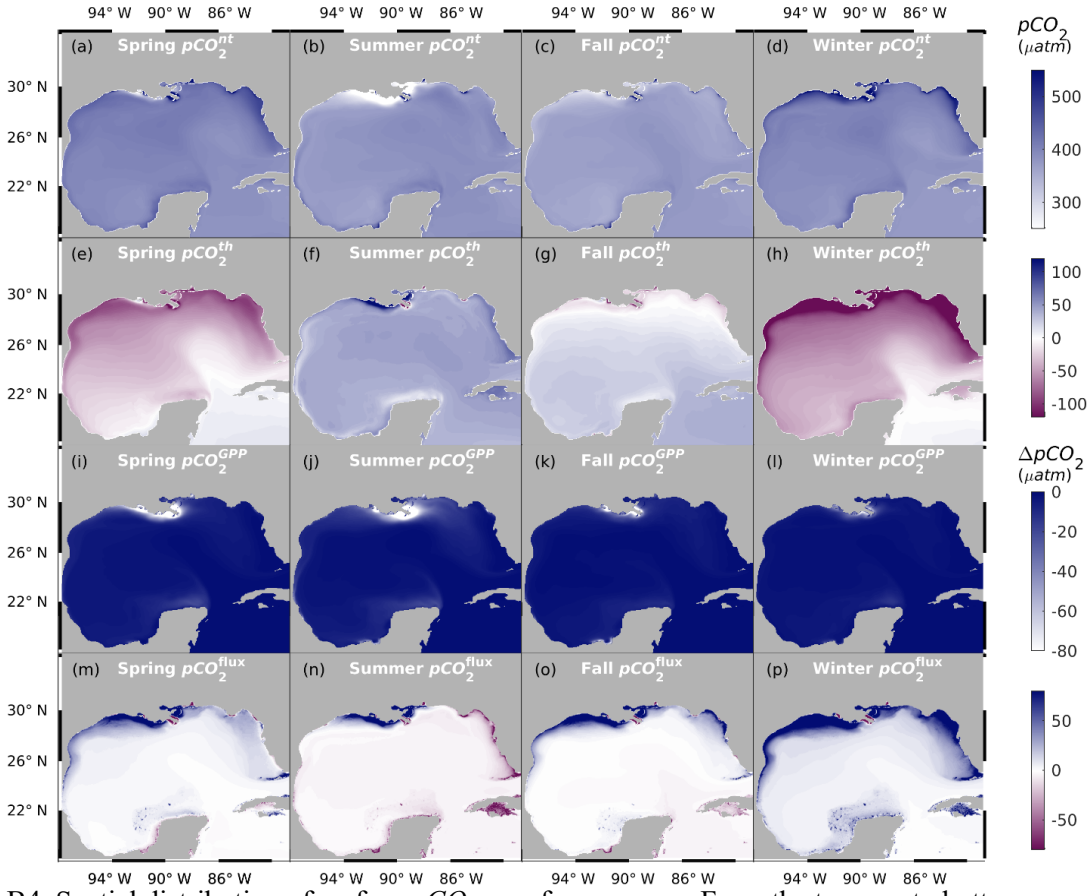

Figure B4. Spatial distribution of surface pCO_2 over four seasons. From the top row to bottom row: pCO_2^{nt}, pCO_2^{th}, pCO_2^{GPP}, pCO_2^{flux}.

We will use "combination" instead of "synthesis" in Line 338.

NEMURO is a process-based biogeochemical model and its timestep is determined at the point of implementation as a balance between computational cost and model stability. We will put the timestep detail under "Methods" in this revision.

One of the greatest difficulties in ocean carbon modeling is the lack of gold standard and ground truth data as the carbon system variables (pCO_2, pH) are volatile and change fast with regard to time and space. Therefore only continuous measurements at fixed locations provide the most reliable data to reveal the carbon dynamics. Only two moored buoys with surface pCO_2 measurement are available in the GoM to offer the ground truth. It is controversial to calculate the discrepancy of modeling studies as the ground truth for the whole GoM is largely unknown. Nevertheless, we plot out the model difference between Gomez et al. (2020) and this study in Fig. B5.

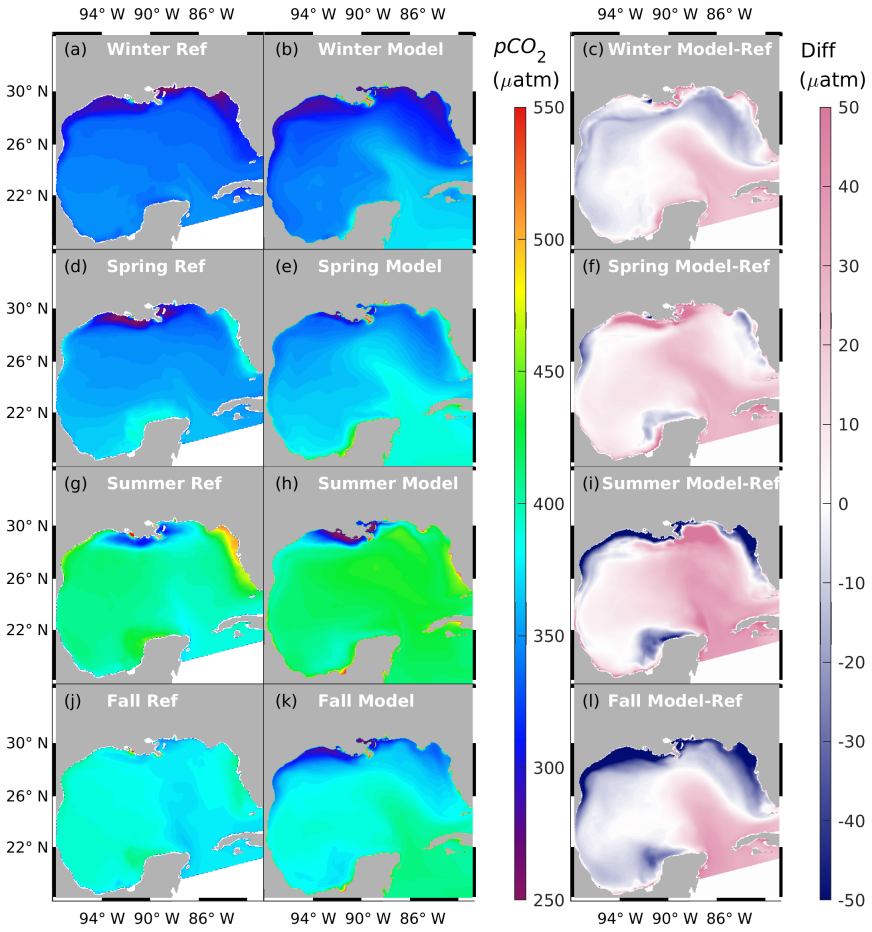

Figure B5. Comparison of seasonal surface pCO_2 (2001-2014 multiyear average) between Ref [Gomez et al. 2020, (a) winter (d) spring (g) summer (j) fall] and this study [(b) winter (e) spring (h) summer (k) fall] as well as their difference [(c) winter difference (f) spring difference (i) summer difference (l) fall difference]. Surface DIC, TA, temperature, and salinity from Gomez et al. (2020) were downloaded from NCEI Accession 0242495(Gomez et al., 2021) (https://doi.org/10.25921/c34h-gb83) to derive the surface pCO_2 patterns according to the method described in their paper. Our model's performance at the two coastal buoys compared with that from the global model and other studies can be found in Fig. A4 in our response to comments of Review #1.

Line 415-422. We will add the region associated with the observational studies, namely the NGoM shelf for both observational studies (Huang et al., 2015; Lohrenz et al., 2018). Xue et al. (2016) gave larger flux estimations in the open ocean and Gulf-wide, but its seasonal and spatial pattern of surface pCO_2 lead to a conclusion that the coastal ocean is a carbon source during summer, which contradicts observations. Compared with the results from Gomez et al., 2020, our model simulated a lower pCO_2 in the coastal ocean during summer and fall, yet the time series comparison in Fig. A4 (in response to RC1) indicated that our results agree more with the monthly climatology based on the two buoy records. Also from the time-series comparison and the Taylor diagram, we conclude that our model yields a better surface pCO_2 estimation.

RC2: *Discussion*
Line 440: What is the meaning of "heterogenous" here:
Line 441" It can also be considered as the pCO2 level determined by the water with a multiyear mean temperature and without the influence from gross primary production or air-sea CO2 flux." But other

processes than mixing, for example horizontal advection, will contribute to this pCO2, 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.

Response: Line 440: "heterogenous" means water with different concentrations of carbon species such as DIC, DOC, PON, etc.; we will remove this word to avoid confusion in the revison. Please see previous answers for using "mixing" as the label. Thanks for the suggestion, we will change the heading for 5.1.

RC2*: 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.*

Response: We will add additional figures in the supporting materials to illustrate the difference driven by boundary condition variation. We acknowledge that from the multiyear mean view, the difference is not that salient, as the interannual variations will be largely averaged out. Most importantly, we feel the boundary impact is more overt in an abnormal year, such as in 2010 under the impact of El Niño. We will add a new paragraph explaining the relevant impact from the boundary in abnormal years in the revision.

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