Response to egusphere-2023-2525 RC2

Below is our response to RC2. Referee #2′s comments are in *gray italic*. Our responses are in black. Expected manuscript revisions are indicated in blue.

Summary

The manuscript provides an analysis of the changes in the ocean carbon cycle between the preindustrial years and the 2090s, under the RCP4.5 and RCP8.5 future scenarios. Using a biogeochemical model forced by decadal-mean ocean circulations derived from a climate model, in steady-state, the authors find that (i) the counteracting effects of the decline in nutrient supply into the euphotic zone and the warming-enhanced phytoplankton growth rate induce a slight decrease in biological production, and (ii) the reduction in biological production and export ratio leads to a decrease in organic matter export. In addition, they assess the changes in preformed and regenerated dissolved inorganic carbon (DIC) inventories and cycles and find that due to the weakening of the circulation the inventory of regenerated and preformed DIC increases while the cycle of preformed DIC cycle becomes faster.

The manuscript makes a novel contribution by developing a partitioning of preformed and regenerated DIC in accounting for the source and sink processes. The manuscript is well written and organized. However I would like to raise several points, mostly regarding the discussion of the results, that should be addressed before its publication.

Please see the detailed explanation of these major points and all my detailed comments below.

We thank Referee #2 for the positive feedback.

Before we provide detailed point-by-point responses, we would like to address Referee #2′s frequent requests for uncertainty estimation (e.g., with repect to circulation choice, mixed-layer depth, averaging period, neglect of seasonality). While we share the referee's appreciation for the importance of uncertainty in general, we think that for our idealized study it makes little sense to estimate uncertainties even if that were feasible within the scope of our study. We are not making specific predictions for the future of the real ocean for which uncertainties would be important. Instead, our focus is conceptual on the processes that drive future changes in the marine carbon cycle for idealized steady-state scenarios. Importantly, the uncertainties that the referee would like to see are very unlikely to affect our conclusions.
Uncertainties with respect to model specifics could in principle be quantified by comparing a wide suite of different circulation and biogeochemical models, but that would necessitate major new research and is beyond the scope of our study. In previous work, Pasquier et al. (2023) had quantified the effects from the ACCESS-M circulation biases by embedding PCO2 in OCIM2, the ocean circulation inverse model (DeVries & Holzer, 2019). Pasquier et al. (2023) had also quantified the effects from biogeochemical parameterization by altering its complexity or by changing biogeochemical parameter values. In the submitted manuscript, we focus on the conceptual and qualitative implications of our analysis without distracting the reader with uncertainties that are of little value in the context of our idealized study.
Below are our point-by-point responses.

Main comments

1. ***The circulation model****: The authors forced the biogeochemical model using the fields of the ACCESS-1.3 model. They referred to previous studies (Bi et al., 2020; Pasquier et al., 2023) stating a*

surestimation in the mixed layer depth in this model (in particular unrealistically deep mixed layer in the Weddell and Ross seas), that could impact the carbon pump. Several questions arise from the existence of these biases in the circulation model:

- *Could the author justify the choice of the ocean circulation ACCESS-1.3 simulations for this study?*

We used ACCESS1.3 because it is a state-of-the art climate model and because this work builds on previous publications based on the ACCESS1.3 circulations. While we could have chosen a different circulation model, all models come with biases, and none of the other models' transport matrices have been assessed in as much detail as those for ACCESS1.3 (Chamberlain et al., 2019; Holzer et al., 2020). Furthermore, Pasquier et al. (2023) optimized the PCO2 biogeochemistry model embedded in the ACCESS1.3 circulation ("ACCESS-M PCO2") to quantify in detail the effects of circulation biases on the biological pump. Thanks to its optimized biogeochemical parameters, the ACCESS-M PCO2 model fits observations of DIC, total alkalinity, dissolved O_2, and PO_4 better than most CMIP5 and CMIP6 models (Bao & Li, 2016; Fu et al., 2022; Planchat et al., 2023). These factors make the ACCESS-M PCO2 model a natural choice for our study. In response, we will add a brief justification to the manuscript along these lines.

- *The authors used optimized biogeochemical parameters, which partly corrects biases from circulation in the preindustrial conditions, if I correctly understand. Are these optimized parameters also used in the future scenario runs? If it is the case, how does this optimization influence carbon pump in the future states where these biases could have disappeared?*

Referee #2 is correct that the biogeochemical parameters were optimized for the preindustrial state and then also used for the future states. Optimizing the parameters for the future states (if future data were available) could fundamentally change the biogeochemistry, capturing, e.g., biological and ecosystem evolution, which is out of scope. Instead, the question here is how the carbon cycle changes due to changes in the physical state of the ocean at "fixed" biology (as modelled by PCO2), given that future biology is unknown. Importantly, we expect that our results are qualitatively robust to variations in parameter values because the mechanisms that drive these changes operate in the same way regardless of the precise parameter values. In response, we will make it clear in the manuscript that the same parameters were used for both the preindustrial and future states.

- *Could the authors expand the third point in the discussion section L. 406-420 by specifying how these MLD biases impact their results on carbon flow rates and inventories, and their main conclusions, given the "key importance of circulation changes"? Could they estimate the uncertainties of the results related to these biases? If these uncertainties are not negligible, at least, the authors should qualify, in the abstract and conclusion sections, the quantified changes found between the preindustrial and 2090s in both future scenarios.*

One could estimate this uncertainty by applying our analysis to a family of different circulation models, but this is out of scope. While such uncertainties would be important when making predictions, our goal is different here: We used an idealized framework of steady future ocean states to elucidate the driving mechanisms causing changes in the ocean's carbon pumps. What is important for that goal is that the circulation model qualitatively captures the expected future changes (e.g., weakening of ventilation, increased stratification, slowing down of meridional overturning, weakening of deep-water formation, and so on). In response, we will qualify the abstract and conclusions accordingly.

2. ***The preindustrial conditions**: For the preindustrial run, the authors forced the biogeochemical model using averages of circulation and thermodynamics over the 1990s instead of preindustrial years. They justified this choice based on the minor changes in hydrodynamics between these two periods. Why did they not directly use averages of circulation over preindustrial years? I suggest adding some explanations on this point in the Methods section.*

While we could have used a preindustrial mean, we chose the same period (1990s) as in previous work (Chamberlain et al., 2019; Holzer et al., 2020; Pasquier et al., 2023). The late-20th-century observations used to constrain the PCO2 parameters through optimization are more consistent with the 1990s circulation than with the preindustrial circulation. Regardless, we would expect preindustrial-to-1990s changes to be dwarfed by the 1990s-to-2090s changes, the latter being our focus here. We will add a few sentences on this to the Methods section.

3. ***Time integration**: The authors considered averages on 10-year time slices. I suggest adding a small discussion on this relatively short time integration for which decadal variability is not handled.*

We of course agree that we subsample decadal variability with a 10-yr average, but we expect decadal variability to be small compared to the centennial-scale changes considered here. More importantly, our goal here is not to make precise predictions of carbon-cycle changes for which uncertainty due to decadal variability is important. Instead, our goal is to use a reasonable future state to explore the consequences of expected circulation changes on the carbon cycle. We will add a sentence briefly discussing why decadal variability is of small importance to our study of idealized future steady states.

4. ***Seasonality**: In the discussion section (L. 408-410), the authors also mentioned uncertainties in the results associated with the absence of seasonality in their circulation model forcing. Could the authors be more specific and give an estimate of these uncertainties in the carbon pump and its plumbing, associated with this simplification, or refer to previous studies that could have estimated them?*

While we cannot quantify the effect of seasonality with a steady-state circulation model, we expect seasonality to mostly affect the upper few hundred meters of the water column. Huang et al. (2021) recently built a seasonally varying ocean circulation inverse model (CYCLOCIM) and found that the inclusion of seasonality only improved the model–observations mismatch above roughly 200 m depth in the global mean. We therefore do not expect the absence of explicit seasonality to affect the qualitative character of our results, as most of the circulation-driven changes analyzed occur at depth. In response, we will revise this passage with a brief discussion along these lines.

5. ***Export of particulate organic matter**: Pasquier et al. (2023) indicated that "particles are only submitted to gravitational sinking" in the coupled model. Is it also the case in the present study? I suggest clarifying how the export of organic matter is calculated in the Methods section. If the transport of POC is similar as done in Pasquier et al. (2023), could the authors specify or give an estimate of how including the advective-diffusive transport could quantitatively change preformed and regenerated DIC inventories and carbon flow rates, in particular in deep convection areas?*

We use the same PCO2 as in the work of Pasquier et al. (2023), and POC is not transported by the circulation. This approximation is desirable for computational efficiency and justified because the gravitational transport of particles typically dominates the advective–diffusive transport (water currents are typically orders of magnitude smaller than our POC sinking velocities). Our goal was not to build the most realistic biogeochemistry model, but a relatively simple one with reasonable fidelity to observations in the current state of the ocean. (Important processes such as, e.g., temperature- and oxygen-dependent remineralization are parameterized in PCO2.) Furthermore, from (unpublished) numerical experiments, we do not expect that including the advective–diffusive transport of particles would make a significant difference to the model solutions. In response, we will clearly state in the manuscript that the biogeochemical model used is identical to that of Pasquier et al. (2023) and thus only includes the gravitational settling of POC.

Detailed comments

L. 66-71: I suggest removing this text summarizing the main results of the study from the introduction section.
We think this is a matter of style, but we will remove the upshot paragraph here for brevity.

L. 113: Did the authors consider the deposition of particulate organic matter (POM) on the floor, remineralisation of POM in the sediment, and the flux of dissolved inorganic matter from the sediment to the water column?
In PCO2, all POM that reaches the bottom is eventually remineralized in the grid box adjacent to the seafloor (Pasquier et al., 2023). A more detailed parameterization of sediment fluxes could be implemented in future versions of PCO2, but is out of the scope here. No change expected.

L. 181: I suggest rephrasing "The future circulation of our states".
We will replace with "The circulation of our future states".

L. 185-187: I suggest showing the nutrient supply and its changes instead of (or in addition to) the euphotic nutrient inventory and its changes, which result from several processes.
The euphotic-zone nutrient inventory may be considered to be the nutrient *supply* as nutrient uptake rates depend on nutrient concentration and not nutrient flux. While one could consider the net nutrient flux into the euphotic zone, this flux also results from a number of processes. We think quantifying the standing stock of nutrients if preferable to make our points here. No change expected.

L 192: Maybe better "and despite" instead of "despite".
OK. We will change as suggested.

L. 192-200: Please see my first major comment. The authors presented that the main changes in mean euphotic nutrient concentration are localized in areas where ML is unrealistically deep in the preindustrial simulation (Fig. B1d-e, Fig. C2). Could the authors, in the discussion section, discuss or indicate an estimate of the uncertainties of the different contributions to DIC biological uptake changes and of the resulting uptake change, associated with the MLD biases, perhaps based on previous studies? Could the MLD biases impact the sign of the total production change and the first main conclusion?
As per our general response at the beginning, quantitative estimates of such uncertainty are out of scope here. However, our qualitative results are likely to be robust as the Southern Ocean mixed layer is predicted to shoal (de Lavergne et al., 2014; Kwiatkowski et al., 2020), driving a decline in nutrient supply, which in steady state must be balanced by a decline in export (and likely production).
Yes, a smaller change in MLD could impact the sign of the production change, whereby the increase in phytoplankton growth from warming could overtake the decline in nutrients. However, our key

point here is that the regenerated carbon inventories increase because of the increased residence times, even if production declines. Also note that comparisons with transient models are complicated, with most CMIP5 and CMIP6 models predicting an increase in Southern Ocean production by 2100, potentially caused by any combination of a warming-enhanced growth, reduced zooplankton grazing pressure, increases in micronutrient supply, reduced sea ice, or reduced light stress from increased stratification and a shallower MLD (Kwiatkowski et al., 2020). In response, we will add in the discussion that a smaller decline in MLDs could potentially have resulted in an increase in production.

L. 201: Maybe better: "Changes in export ratio"
Thank you. We will adopt this wording.

L. 202-203: I suggest defining export production at the beginning of "Changes in export ratios" section, instead of in L. 217-218.
Agreed. We will move the definition of export production.

L. 208-215: In this paragraph, the authors listed mechanisms inducing changes in OC export ratio. Did the authors quantify the changes of DOC and POC export ratios, DOC and POC exports, and euphotic DOC and POC remineralizations? I suggest adding these changes to the appendices. Related to my first main comment, could the authors give an estimate of the uncertainties of the magnitude of export ratio changes due the MLD biases?
Yes, we quantified changes in export ratios for each export pathway (DOC, $\mathrm{POC_s}$, $\mathrm{POC_f}$, and PIC) but, for concision, we only showed maps of the total export ratio in the main text. However, Appendix Fig. D3 does show the corresponding zonal integrals partitioned according to export pathway, which we think provides ample detail. As per our overall response at the beginning, quantifying the uncertainty is not feasible within the scope of our study. In response, we will reference Figure D3 in this passage.

L. 217: "changes in carbon export production Jex itself".
OK, we will add "production" here.

L. 223: Do you mean organic-matter production or export production? Please clarify.
We meant export production. We will add "export".

L. 230: "suggest"
OK.

L. 259: "unrealistic deep ML"
We will change this for "unrealistically deep mixed layer".

L. 280-281: I suggest rephrasing this sentence.
While we are not exactly sure what Referee #2 is suggesting here, we will split the sentence and rephrase along the following lines:

> Figure 5 shows that for each export mechanism (DOC, $\mathrm{POC_s}$, $\mathrm{POC_f}$, and PIC), export production declines and sequestration time increases. The increases in sequestration time are consistent with an overall slowdown of the circulation and with longer re-exposure times for regenerated DIC to return to the euphotic zone (Fig. C3).

L. 302: “these pathways change”

We think it is clear that “these” refers to the DIC pathways in this short sentence. No change expected.

L. 303: delete [t]

Apologies. Deleted.

L. 338: Do you mean “regenerated nutrients at intermediate depths”?

We will rephrase as “nutrients regenerated at intermediate depths”.

L. 345-346: I suggest deleting the repetitions with L. 253-254, if any.

We are not sure where the repetition is here. L345–346 is about the preformed carbon inventories while L253–254 is about the total carbon inventories. No change expected.

L. 367-368: the flow rate decreases/increases or the flow rate slows/speeds up?

We will remove “rate” here and make sure to use “the flow slows/speeds up”.

L. 391-393: I suggest replacing “a significant advance over” by “an enrichment of”

We respectfully disagree here. The traditional view of preformed tracers as the solution to a concentration boundary-value problem is set in stone and cannot be “enriched” by going to flow rates (i.e., source/sink). No change expected.

L. 449-453: “the changes in the controls on carbon export and biological utilization identified by Boyd (2015)”: Could the authors list these changes and clarify the agreements with the study of Boyd et al. (2015)?

There are 12 carbon pump components identified by Boyd (2015) that are based on 25 publications (their Table 3). In our judgement, an exhaustive comparison would be too much detail for this discussion. For simplicity and clarity, we instead focused on the strongest disagreement, which is that PCO2 does not include shifts in community composition, which is the predominant control in the study by Boyd (2015). No change expected.

Appendices: I suggest (i) numbering the appendices and their figures in the order of reference in the main text, (ii) locating the figures after the title of the corresponding appendix (Fig. C1), (iii) changing the caption of Figure D2 (and Fig. D1), where the authors referred to Fig. 2 that is commented later in the main text.

We respectfully disagree with Referee #2 here. The appendices were organized for clarity and ordered by the first appearance of the first figure of each appendix in the main text: Appendix A is mentioned in L170, Fig. B1 in L187, Fig. C1–3 in L190, and Fig. D1–3 in L203. In response, figure placement with respect to titles has been fixed, although the precise placement of figures will occur in the final typesetting by the journal. We will also revise the captions of appendix figures so that they are self-contained.

L. 533-535 and L. 613-618 : These lines should be located after the appendices.

We used the Journal’s LaTeX template, which controls these placements. We defer to the editors for final typesetting details. No change expected.

Figure C2: I suggest adding the MLD changes for the two future scenarios as in Figure 2.

Agreed. We will add plots of the changes to Fig. C2.

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