Reply to Anonymous referee #1

We are grateful for the comments and edits of the anonymous reviewer who invested time for the review of our manuscript. The reviewer's text is reported in Italic and our responses in roman.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The authors did an amazing job in taking all the comments into account during the very thorough review. Thank you very much for working in all comments.

From my perspective, the paper is now almost ready for publications if the following comments would be implemented:

Larger comments:

I am still confused that the estimated effect of riverine influx and burial is an outgassing of 0.65 Pg C yr-1 (Regnier et al., 2022) but your model results suggest something different. This difference needs to be acknowledged and discussed somewhere. I understand that this is typical for most ocean biogeochemical models (riverine influx = burial) but it should not be the case if rivers and sedimentation are simulated as realistically as possible. Can you find a reason why adding the rivers does not lead to an outgassing (FIX-REF is 0.1 Pg C yr-1 but should be -0.65 Pg C yr-1).

1) Thank you very much for pointing it out. One of the reasons for the discrepancy is the different definition of the extent of "ocean" used in the calculation of the air-sea CO_2 fluxes in both studies. In the study by Regnier et al. (2022), the 0.65 Pg C yr^{-1} outgassing was calculated for open ocean (lateral flux from continental shelf water minus burial in open ocean sediment; 0.80-0.15=0.65). The carbon budget for estuaries, tidal wetlands and continental shelf water was calculated as input to the ocean. However, in our model, the air-sea CO_2 fluxes were calculated over the broader ocean areas, which implicitly include estuaries and continental shelves. Therefore, the carbon (C) uptake from estuaries and continental shelves in our study would partly balance the outgassing in the open ocean or elsewhere. If we take the numbers from their study and assume the burial is the same, our calculation of air-sea CO_2 fluxes would be 0.65-0.2-0.1=0.35 Pg C yr^{-1}, where 0.65 is outgassing from open ocean, 0.2 and 0.1 are C uptake from estuaries and continental shelf water. Therefore, the ocean outgassing would be 0.35 Pg C yr^{-1} let alone the ±0.30 Pg C yr^{-1} uncertainty.
2) If we understood correctly, by mentioning FIX-REF=0.1 Pg C yr^{-1}, the reviewer refers to the riverine impact on global C uptake during 2003–2012 (Table B2, 0.09±0.01 Pg C yr^{-1}). Our model results indeed suggest a weak C sink due to the riverine input at the level of 1970's. Note that the above mentioned 0.65±0.30 Pg C yr^{-1} ocean outgassing was calculated for pre-industrial conditions. Although the anthropogenic perturbation of riverine load of C is uncertain, the riverine nutrients load in contemporary era is increased significantly. Riverine loads of phosphorus (P) and nitrogen (N) increased from the pre-industry values of 2 Tg P yr^{-1} and 20 Tg N yr^{-1} to 1970's level of 7.6 Tg P yr^{-1} and 37 Tg N yr^{-1}, respectively (Beusen et al., 2016; Green et al., 2004; Seitzinger et al., 2010).

This can be the second reason why our results indicate an ocean C sink for contemporary time, due to the enhanced biological C uptake.

3) The third reason might be, as we mentioned in the manuscript, that our model overestimates the conversion of organic nutrients to dissolved inorganic nutrients, which probably leads to overestimate of biological C uptake.

When I look at figure 5, it seems that the GN scenario has no effect on NPP globally and in the Arctic Ocean. Maybe consider mentioning that the scenario has almost no effect? I think it would be easier to understand and read the paper if you show only one scenario in the main part and move the other ones to an appendix. In general, the Discussion reads not very well. I am not sure what the information is that I should take away from this. Maybe try to restructure the paper as follows: Traditional simulation without any riverine input (REF), changes in PP and air-sea CO2 flux from climate change with fixed rivers (FIX), and changes in PP and air-sea CO2 fluxes due to changes in rivers (one GN scenario). I think this would be a clearer message that would be easier to transfer to the reader.

1) In Figure 5a (see also Table B1) it shows that in GNS runs the projected decline in global PP is lessened by 0.66±0.02 Pg C yr^{-1} (29.5%) from -2.24±0.37 Pg C yr^{-1} (REF) to -1.57 ±0.38 Pg C yr^{-1} (mean GNS). We mentioned that also in the Abstract. In the Arctic, the effect of GNS have the same magnitude as the FIX and RUN (Figure 5b). We agree with the reviewer that there is almost no difference in the effect among the four GN scenarios when we consider the global integrated PP or C uptake, and the spatial difference among the four scenarios is also marginal. We consider that this warrants further investigation with higher resolution models, which can resolve the effects of riverine input at hotspots in different scenarios. Since we did not focus on discussing the difference among the four scenarios, we think it is not necessary to separate them.
2) We have restructured the discussion in the following way: as reviewer suggested, we firstly discuss projection of PP and C uptake under climate change only (REF), followed by discussion on projected change due to riverine input (in the order of FIX, RUN and GNS).

Please consider adding a discussion about what would be needed to improve modeling of the rivers following up on your study and other studies. Do you get insights why models do not find the same results as the one derived from observations (Regnier et al., 2022), i.e., a riverine carbon outgassing of 0.65 Pg C yr-1. When I look at Figure 4, the rivers seem to be a very minor problem compared to the rivers.

Thanks for the suggestion. We have added a paragraph at the end of Conclusion.

1) Better resolve shelf processes with higher model resolution, to have more realistic remineralization rate for riverine organic matter in the coastal water and shelf sediment, as well as lateral transport
2) Better constrain the riverine carbon to nutrient ratios
3) Explore future scenarios of riverine input to the ocean and their impact on ocean PP and C uptake, especially on regional scales

Can you explain the large overestimation of NPP in the Southern Ocean in NorESM and the large underestimation in the western Pacific and Indian Ocean? That might be very helpful to underestimate the general underestimation. Maybe nothing is exported north out of the Southern Ocean (Sarmiento et al., 2004)?

Biases in physical and biogeochemical processes in NorESM contribute to the regional PP model-data discrepancies, and it is challenging to attribute specific processes to these biases. Nevertheless, two factors have been recognized to contribute to the PP bias in the Southern Ocean. In NorESM1 the large PP in the Southern Ocean can be attributed to a too weak top-down control, leading to large spring blooms in phytoplankton at high latitudes. With a re-tuning of the ecosystem parameterization this bias has been reduced in newer model version (Schwinger et al., 2016). Moreover, the bias high PP in the Southern Ocean can also be due to the strong winter mixing, which upwells too much nutrients for the proceeding spring bloom, based on the improvements simulated in the latest version of NorESM (Tjiputra et al., 2020). Otherwise, PP is relatively low because the isopycnic model might have a too low vertical diffusivity providing too little new nutrient to the euphotic zone at lower latitudes. This low bias in the nitrate-limited western Pacific and Indian Ocean has been alleviated through improvements in the nitrogen fixation and productivity parameterizations. Other processes such as the equatorward nutrient advection from the Southern Ocean (Sarmiento et al., 2004) could also play a role.

Minor:

Line 19: simulated contemporary spatial distribution of what? A word seems to be missing.

We added “of annual mean PP and air-sea CO_2 fluxes”.

Line 24: The best estimate and the uncertainty do not have the same number of digits behind the comma.

We changed 0.7±0.02 to 0.66±0.02, as well as in Line 29, changed 0.1±0.03 to 0.11±0.03.

Line 27: I am not sure what the therefore wants to say here. I cannot see the link but it might be my fault.

We agree, “therefore” has been deleted.

Lines 37-42: Maybe consider discussing Regnier et al. (2022) here.

We have added “A recent study on global carbon cycle has emphasized the importance of the carbon transport through the land-to-ocean aquatic continuum (Regnier et al., 2022).”

Line 83: What kind of model? Some more details would be good here.

The name of the model (the Integrated Model to Assess the Global Environment-Global Nutrient Model (IMAGE-GNM)) has been added.

Line 148: Is 'state-of-the-art' still the right wording? It is surely one of the best ESMs available but given that a new version is available, I would reconsider this wording. But it doesn't really matter.

"State-of-the-art" has been deleted.

Line 175 and afterwards: Given that the model is not exactly HAMOCC5 as you explained in the responses, I would not call it HAMOCC5 here and later. A possible alternative would be ocean biogeochemical model component.

We have renamed it as $HAMOCC_{NorESM1}$ and edited it through the whole manuscript.

Line 192: Please consider a quantitative assessment. Good agreement is very subjective.

We have added "The simulated global annual mean PP is 40.1 Pg C yr^{-1} during 2003–2012, which is lower than the satellite-based model estimates, ranging from 55 to 61 Pg C yr^{-1}."

Lines 207-209: You might want to consider adding here also missing terrigenous input, which is at the centre of your study. Another important point would be lateral influx from the Atlantic and Pacific Ocean (Torres-Valdés et al. 2013), which is often underestimates in models with relatively coarse resolution (Terhaar et al., 2019) as ESMs.

1) We have added "Additionally, lack of adequate representation of riverine input in some ESMs can also lead to underestimate of PP, since around one third of current Arctic marine PP is sustained by terrigenous nutrient input (Terhaar el al., 2021)."
2) We have acknowledged the underestimate of lateral influx (from coast to open ocean) in the first paragraph of Section 4.4. However, in Torres-Valdés et al. (2013)'s paper, they showed that there are statistically robust net silicate and phosphate exports out of the Arctic, while the net nitrate flux is zero. There is no net influx of nutrients to the Arctic from the Atlantic and Pacific, therefore we cannot use it as an argument for the model underestimate of PP.

Lines 188-215: It would be interesting to also see an evaluation of nutrients and alkalinity in the model compared to observations.

The simulated alkalinity, phosphate, nitrate and silicic acid have been evaluated in previous work by Tjiputra et al. (2013) and they have been compared between NorESM1 and NorESM2 in a more recent work by Tjiputra et al. (2020). We have added this information in the manuscript.

Lines 342: Projections are always about the future, so I think future is not needed here.

We have deleted it.

Line 417-418: Consider citing Vancoppenelle et al. (2013) here.

Thanks. It has been added.

Lines 521-524: It seems a bit strange to compare model results to model results. There are many observational studies. You might want to consider discussion observational estimates of the lability of the riverine organic matter. Aumont et al (2001) showed the effect of lability in models and a comparison with Lacroix et al. (2021) suggests that the lability of organic matter might be on the high side. Adding observational studies here would probably help.

We have changed to observation based estimate as "Given that the proportion of the direct remineralized organic N (27.8%, see the calculation above) in our model is comparable to or lower than the reported values by field studies (~38.8% of DON decomposed during transition from Arctic rivers to coastal ocean), which indicates that the bias on enhanced PP is likely less than 33.3%."

Figure 3: Consider changing the colormap in a).

We have changed the colormap.

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Sarmiento, J., Gruber, N., Brzezinski, M. et al. High-latitude controls of thermocline nutrients and low latitude biological productivity. Nature 427, 56–60 (2004). https://doi.org/10.1038/nature02127

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Green, P. A., Vörösmarty, C. J., Meybeck, M., Galloway, J. N., Peterson, B. J., and Boyer, E. W.: Pre-industrial and contemporary fluxes of nitrogen through rivers: a global assessment based on typology, Biogeochemistry, 68, 71-105, 2004.

Schwinger, J., Goris, N., Tjiputra, J. F., Kriest, I., Bentsen, M., Bethke, I., Ilicak, M., Assmann, K. M., and Heinze, C.: Evaluation of NorESM-OC (versions 1 and 1.2), the ocean carbon-cycle stand-alone configuration of the Norwegian Earth System Model (NorESM1), Geosci. Model Dev., 9, 2589-2622, 10.5194/gmd-9-2589-2016, 2016.

Seitzinger, S. P., Mayorga, E., Bouwman, A. F., Kroeze, C., Beusen, A. H. W., Billen, G., Van Drecht, G., Dumont, E., Fekete, B. M., Garnier, J., and Harrison, J. A.: Global river nutrient export: A scenario analysis of past and future trends, Global Biogeochemical Cycles, 24, https://doi.org/10.1029/2009GB003587, 2010.

Terhaar, J., Lauerwald, R., Regnier, P., Gruber, N., and Bopp, L.: Around one third of current Arctic Ocean primary production sustained by rivers and coastal erosion, Nature Communications, 12, 169, 10.1038/s41467-020-20470-z, 2021.

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Tjiputra, J. F., Schwinger, J., Bentsen, M., Morée, A. L., Gao, S., Bethke, I., Heinze, C., Goris, N., Gupta, A., He, Y. C., Olivié, D., Seland, Ø., and Schulz, M.: Ocean biogeochemistry in the Norwegian Earth System Model version 2 (NorESM2), Geosci. Model Dev., 13, 2393-2431, 10.5194/gmd-13-2393-2020, 2020.

Reply to Referee #2 Fabrice Lacroix

We are grateful for the comments and edits of Fabrice Lacroix who invested time for the review of our manuscript. The reviewer's text is reported in Italic and our responses in roman.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The authors have gone great lengths to improve their manuscript. While the chosen setup is still limited, the review of literature, as well as the analysis and discussion of the results is well done. I therefore suggest accepting the manuscript with only few technical corrections.

Major Comments:

In their manuscript, Gao et al. investigate the role of contemporary river fluxes for alleviating bias of the ocean biogeochemistry model, as well as the impacts of potential future changes in riverine fluxes for ocean biogeochemistry. The authors find improved model performance on the global continental shelf after introducing riverine fluxes, as well as counteracting effects of individual impacts of increased carbon and nutrients inputs to the ocean. As mentioned in the previous reviews, the material is new and the analysis was well performed, although the chosen setup is still clearly very limited. As one of the authors of one of the two rather critical reviews, I believe that the manuscript is now immensely improved in other aspects over the previous submitted version:

- *The introduction is now a very nice review of state-of-art knowledge and implementations of riverine exports in the ocean (for which remain many uncertainties)*
- *The methods and results are better structured and clear to follow*
- *The interpretation and discussion of results are now more complete and clearer. In particular, a discussion on model limitations due to simplified relationships of riverine stoichiometries, degradation and shelf circulation are all now included. The authors included an well-thought back-of-the envelope calculation on the impacts of too low shelf mineralization rates on the change in primary productivity.*

Overall, it was a very enjoyable read. I thus approve accepting the manuscript for publication and have only minor specific comments and edits.

Specific Comments:

L28 *"while in the more plausible riverine configurations the river inputs cause a net C source of ~0.1 ± 0.03 Pg C yr-1 „. In my opinion, this is quite an interesting result being that the effect of increased riverine carbon is stronger than increased nutrient inputs for the future projections. For the historical perturbations, it is usually assumed the nutrients are the dominant component (e.g. as the authors mention in the Lacroix et al., 2021) because of the large relative change in the past. But this indeed might not hold for the future, and there is very little work on the impacts of changing riverine C fluxes. I however leave it to the authors whether they would like to perhaps underline this more strongly.*

Thank you very much for pointing it out. We have added one sentence in the Abstract: “It implies that the effect of increased riverine C may be larger than the effect of nutrient inputs in the future on the projections of ocean C uptake, while in historical period increased nutrient inputs are considered as the largest driver.”

Edits

L14 “So far, this contribution is represented in the state-of-the-art Earth system models with limited effort.“ Sounds a bit awkward.

We have changed this sentence to “So far, this process has not been fully represented and evaluated in the state-of-the-art Earth system models.”

L243 maybe formulate like this: “...are aggregated within catchment basins defined by the NEWS 2 study for every river.”?

Changed accordingly.

L245 “..up to...”

Changed accordingly.

L251-253 “First, we calculate the riverine organic P-N-C ratios for both dissolved and particulate forms, then add the least abundant species (scaled by the Redfield ratio) to the DOM and DET pools, respectively. „ I understand what is meant, I wonder however if this could be formulated more clearly however.

We have added the following equations to make it more understandable.

DOM_{riv}=min (DOP, DON/16, DOC/122)

DET_{riv}=min (POP, PON/16, POC/122)

L384 if -> when

Changed accordingly.

L447 and no riverine C input -> no varying riverine C input

This part of the sentence has been deleted due to change in paragraph structure.

L487 “Given that the riverine nutrient and carbon inputs account for only a small proportion of the total amount of nutrients and carbon in the euphotic zone of the ocean“ I assume this refers to “yearly inputs”, but the sentence also feels out of place and doesn’t relate to what comes next.

Thanks for pointing it out. We have deleted this part of the sentence and modified it as "We acknowledge several limitations of our study, particularly related to the resolution and complexity of our model."