Comment on bg-2021-293

Anonymous Referee #2

Referee comment on "Riverine impact on future projections of marine primary production and carbon uptake" by Shuang Gao et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-293-RC2, 2022

Using a global ocean biogeochemical model, the authors assess the future impacts of changing riverine inputs while performing simulations with several scenarios of riverine inputs. In the paper, the authors show a slight reduction in root mean square error of primary production with respect to observations on the continental shelf and in parts of the ocean highly affected by rivers. They show that, overall, riverine nutrients may alleviate part of projected primary production decline. The topic is very relevant and potentially important for fully constraining the ocean carbon cycle. However, while the modelling work is very sound in its closed framework, I believe the author's review of literature seems to be lacking, which leads to, at times, questionable assumptions and too high confidence in a model setting not necessarily adequate for investing impacts of riverine tracers and their fate in the ocean. I do think the study should be published, but the authors should discuss the following points and, in my opinion, clearly point out uncertainties related to their framework in the abstract.

General Comments

It is debatable that the model configuration used here is actually adequate for the research question addressed in this paper. The model is limited in terms of resolution (~1 degree), and this is a strong constraint for representing fine-scale circulation features that are thought to be of particular importance in the coastal ocean. This is a topic that many studies have discussed previously, and these should be considered. Secondly, the model doesn't consider specific biogeochemical processes relevant to the coastal ocean. For instance, organic matter decomposition rates are shown to be much higher in the coastal sediment than in the open ocean (Ardnt et al., 2013), bed shear stress also re-suspend biogeochemical from the shelf seafloor. I think omitting both of these physical and biogeochemical limitations lead to the important underestimation of primary production on river-dominated continental shelves shown in this study, which seems to indicate the model is underrepresenting. This also affect exports of riverine biogeochemical compounds to the open ocean, and thus this has very important consequences for the main outcomes presented in this study. The authors do mention these limitations briefly in the limitations

section, but this should be considered omni-present in attempts to interpretate the results, and at the least mentioned in the abstract.

A second, perhaps less central point, but still relevant to the study, is that the authors spin-up their model to present day fluxes, whereas these are actually more strongly perturbed over the historical time period (Beusen et al., 2016), than what is projected in terms of their future changes. Since time-scales of the ocean carbon cycle are notably long, this historical perturbation could have important legacy effects propagating into the future, potentially enhancing the primary production more than is estimated here. This should, in my opinion, also be discussed in the limitations section.

Specific Comments:

Abstract

L16 "With four riverine configurations: deactivated, fixed at a contemporary level, coupled to simulated freshwater runoff, and following four plausible future scenarios." Are only the nutrients (and if yes which ones) changing, or also carbon and alkalinity? This should be stated here.

L17 "The inclusion of riverine nutrients and carbon…" Those numbers are valid for contemporary I guess?

L20 "Riverine nutrient inputs alleviate nutrient limitation,…" Should be reformulated, since riverine nutrient inputs are unlikely alleviated nutrient limitation in general, but reduce (?) it in some regions (?).

Introduction

In general, there are very little citations in the introduction, and often the same ones are used repeatedly. There are some recent modeling studies of implications of riverine inputs in the ocean that would be very relevant for this study. These should, in my opinion, be considered in the introduction:

Lacroix, F., Ilyina, T., Mathis, M., Laruelle, G. G., & Regnier, P. (2021). Historical increases in land-derived nutrient inputs may alleviate effects of a changing physical climate on the oceanic carbon cycle. Global Change Biology, 27, 5491– 5513. https://doi.org/10.1111/gcb.15822

Liu, X., Stock, C. A., Dunne, J. P., Lee, M., Shevliakova, E., Malyshev, S., & Milly, P. C. D. (2021). Simulated global coastal ecosystem responses to a half-century increase in river nitrogen loads. Geophysical Research Letters, 48, e2021GL094367. https://doi.org/10.1029/2021GL094367

I would furthermore suggest citing some regional-scale studies that investigate implications of riverine inputs on biogeochemistry of specific shelves, literature is abundant here. In addition, I would read and refer to the last 2-3 Global Carbon Budget studies for potential importance of riverine carbon fluxes for the ocean.

L25 "The large range of the riverine input across our four riverine 26 configurations does not transfer to a large uncertainty of the projected global PP and ocean C uptake…" In terms of global PP, one could argue this could be due to the representation of continental shelf in the model, which leads to heavily underestimated continental shelf PP.

L35 "Although riverine carbon only plays a minor role in the global carbon cycle, …" Recent Global Carbon Budget publications disagree with this (Friedlingstein et al., 2021). If the higher estimates of outgassing of riverine carbon are true (up to 0.8 Pg C yr^{-1}), they could potentially play a large role in explaining discrepancies between CO_2 estimates arising observation-based products and model-based results.

L44 Maybe add the more recent Beusen et al. (2016) estimates to this for the historical time period?

Methods

L118 "The riverine influx includes carbon, nitrogen and phosphorus, each in dissolved inorganic, dissolved organic, and particulate forms, as well as alkalinity (ALK), dissolved silicon and iron (Fe)." Are there specific ocean variables for terrestrial dissolved and particulate organic matter? If not how does not model deal with organic P-N-C ratios that differ from those of the Redfield ratio? This is an important point to clarify because high C-to-nutrient ratios are thought to be largely responsible for ocean outgassing.

L140 "Any remaining riverine organic matter is then added to its inorganic pool" This is not really clear. Is excess organic carbon is added to the DIC pool? If not I think this

might be the reason why river inputs cause a net sink in the model, and not source as is relatively well acknowledged (see e.g., Global Carbon Budget, 2021). Also keep in mind that organic carbon mineralization has a small effect on alkalinity (which I don't think would have a huge impact here).

Also, maybe more important here: are you assuming the large particulate fluxes (particulate P and N) from NEWS2 are organic? Because from my understanding, these can be inorganic (for P bedrock erosion, occluded etc..), and this would not at all be bio-available in the coastal ocean.

L156 "REF: Reference run. Riverine nutrient and carbon supply is deactivated." Are there other sources of nutrients and carbon in the model? If riverine nutrients and carbon were the only inputs to the ocean model and their sediment loss is non-zero, I would expect all related variables to thrive to zero, which does not make a very interesting reference run. In the case there are other inputs, they should be given in numbers and explained.

L179-L185 In my opinion the authors don't need to specifically defend themselves on this particular point, at least not to this extent. I would consider shortening or removing.

Results

L198 "Although the total PP in FIX is still considerably lower than the satellite-based estimates, the inclusion of riverine nutrients and carbon does slightly improve the distribution of PP especially on continental margins (Figure 3), according to our area-weighted root mean square error (RMSE) analysis. „

Figure 3 really shows that a large part of the underestimation of PP is originating from the continental shelf, in particular regions of riverine inputs. The improvement is minor compared to the actual bias. In my opinion, this actually shows that the model underestimates the impacts of rivers on PP, which does have a strong implication for the conclusions of this paper, and should be assessed somehow.

Figure 4: It's a bit concerning to me that considering riverine inputs lead to a sink of carbon in the ocean. It is relatively well acknowledged that river inputs are thought to cause a source of carbon (e.g., Regnier et al., 2013; Resplandy et al., 2018). The reason for this is that carbon to nutrient ratio of the (bio-available) terrestrial inputs is larger than the Redfield ratio. I guess the fact that most particulate P and N is thrown in as dissolved inorganic species might be the explanation for this. How is the alkalinity to DIC ratio of riverine inputs constrained? Either way, either explain the reason for this or I would consider not discussing the CO_2 flux for the "unperturbed" river simulation.

L305 "Our experiments show that riverine nutrient inputs have a dominant role over the organic matter inputs in FIX, enhancing CO2 uptake along continental margins via sustaining PP in both historical and future time periods." This is however purely a consequence of the ratio of (bio-available) nutrients to organic matter that is added to the ocean, which as mentioned, I don't think is completely correct from a process-based perspective. In fact, I think most river-dominated shelves show C outgassing, see regional CO_2 fluxes from Chen and Borges (2009) or regional-based studies.

L337 "…do not transfer to large uncertainties in future global marine biogeochemistry projections in NorESM." Yes, but if you would have taken uncertainties related to the coastal ocean into account, through e.g. sensitivity analysis of sediment degradation, I wonder if the conclusions would be different here, I would assume so.

Minor edits

L22 "and thus lessen the projected future decline in PP by up to 0.6 PgC yr-1 22 (27.3%) globally depending on the riverine configuration." -> , globally,

L55 "Taking the advantage of the latest improvement of global" Remove "the".

L171 "By comparing FIX versus REF…" Add comma here.

L332 ". Therefore, it is worth exploring the merits of using GNS in future projections of marine biogeochemistry." Not really sure what is meant here.

Arndt S., B.B. Jørgensen, D.E. LaRowe, J.J. Middelburg, R.D. Pancost, P. Regnier,Quantifying the degradation of organic matter in marine sediments: A review and synthesis,Earth-Science Reviews, Volume 123, 2013, Pages 53-86, ISSN 0012-8252, https://doi.org/10.1016/j.earscirev.2013.02.008.

Beusen, A. H. W., Bouwman, A. F., Van Beek, L. P. H., Mogollón, J. M., and Middelburg, J. J.: Global riverine N and P transport to ocean increased during the 20th century despite increased retention along the aquatic continuum, Biogeosciences, 13, 2441–2451, https://doi.org/10.5194/bg-13-2441-2016, 2016.

Chen C.-T. A., Borges A.V., Reconciling opposing views on carbon cycling in the coastal ocean: Continental shelves as sinks and near-shore ecosystems as sources of atmospheric CO2, Deep Sea Research Part II: Topical Studies in Oceanography, Volume 56, Issues 8–10, 2009, Pages 578-590, https://doi.org/10.1016/j.dsr2.2009.01.001.

Regnier, P., Friedlingstein, P., Ciais, P. *et al.* Anthropogenic perturbation of the carbon fluxes from land to ocean. *Nature Geosci* **6,** 597–607 (2013). https://doi.org/10.1038/ngeo1830

Resplandy, L., Keeling, R.F., Rödenbeck, C. et al. Revision of global carbon fluxes based on a reassessment of oceanic and riverine carbon transport. Nature Geosci 11, 504–509 (2018). https://doi.org/10.1038/s41561-018-0151-3