

Interactive comment on “Oceanic CO₂ outgassing and biological production hotspots induced by pre-industrial river loads of nutrients and carbon in a global modelling approach” by Fabrice Lacroix et al.

Anonymous Referee #1

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This paper assesses the effect of riverine nutrient and C inputs on C cycling in global oceans, and notably on CO₂ outgassing in a pre-industrial state. The work includes quantification of riverine inputs of different elements under inorganic and organic forms, regional coastal shelves analysis, and global ocean biogeochemical modelling. Finally, the authors derive a global land-ocean-atmosphere pre-industrial budget, and discuss the effect of riverine inputs and gaps that need to be further addressed in the future (e.g., including volcanic emissions and the effect of shale organic oxidation).

I believe this is a substantial piece of work, improving the current understanding of

global C cycling, and totally fitting Biogeosciences' scope.

I however believe the author should improve the manuscript by addressing the 2 following points, for it to become clearer and meet the journal's standards.

1) In general, the paper is extremely dense and would benefit from being shortened and making it more to the point. Major conclusions should be better highlighted, and repetitions avoided. Also, consider deleting sections with discussions out of the main scope of the study and already addressed in previous work (e.g., on weathering). A few suggestions to improve this point are also listed hereafter, in the specific comments.

2) Further details should be provided on the construction of 2 ocean simulations, RIV and REF. The understanding of the differences between these 2 simulations is key to understand the paper's major conclusions.

—Specific comments—

Abstract

L7-8p1. “Thirdly, we quantify the terrestrial origins. . . in the framework, . . .” -> It is not clear from this sentence if this is purely a modelling exercise or if you are assessing global oceanic C budgets using the coupled land-atmosphere-ocean model.

L11p1. “leads to a global oceanic source of CO₂” -> “leads a net global CO₂ emission to the atmosphere of”. To be consistent with the following sentence, the “source” would be 183+128 TgC/yr (Fig. 11).

L14p1. It is not clear what a sink due to a model drift is without reading the whole paper in depth. . .

Introduction. The introduction provides a lot of information, some of it not totally relevant to the focus of the study. It should be shortened and re-organized to better highlight current state of knowledge, gaps and how they are addressed in this work.

L15p2. “these knowledge gaps” -> all knowledge gaps are presented in paragraph 7

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(L3-23 p4).

Paragraphs 2-6. These present in detail processing in watersheds, gaps in knowledge on tDOM and POM degradation and transfers, etc. These could be substantially shortened, since these are not points tackled in the paper, which waters down the main message/focus of the paper. Parts of it (e.g., uncertainties on degradation of different OM forms, desorption of P as it enters saline waters etc.) could however be used in the Discussion (e.g., subsection 7.2).

Methods

L2p5. “Pre-industrial” could be defined here (state in 1850).

L5p5. “spatially explicit quantification of global riverine loads” -> “spatially explicit quantification of riverine exports to global coasts”. It is not clear otherwise if the loads within watersheds are quantified as well.

L10p5. “We first briefly. . .” -> Description of the Method’s content is already described in the previous paragraph. These introductory sentences could be most of the time removed to make the paper shorter and more to the point.

L12p5. Define what you consider like alkalinity here.

L20-32p5. This is described in detail in the subsections; it could be deleted to avoid repetitions.

Fig.1. Precise that the C from weathering sources is DIC (OC assumed to originate only from the uptake of atmospheric CO₂).

L12-14p7. Doesn’t river damming affect POM loads between pre-industrial and the 1970s? This point could be considered later in the discussion on river loads.

L13p8. “Pre-industrial runoff. . .” -> this is already explained L1-5p7, not necessary. This repetition occurs several times throughout the paper.

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L24p8. Are the soil types listed here those with typically low erosion rates? Is the 0.1 factor also used for wetlands and areas with a high groundwater table? If yes, what definition did you use for “high water table”?

L4p9. What do the 1.6 TgP/yr used here correspond to? Fertilizer P in surface runoff reaching rivers? Hart et al. (2004) report an annual fertilizer consumption of 873 TgP in 1913.

L7-9p9. Why is it reasonable to assume P equilibrium in soils at pre-industrial state (besides that state-of-the-art models usually use this initialization)?

L9-11p9. The assumption that spatial distribution of P river loads is the same in pre-industrial and 1970 is quite strong. For example, agriculture was probably much more developed in North America and Western Europe than in Asia at the beginning of the 20th century. Implications of this assumption should be discussed later in the discussion on river loads. Why not use load distribution from models describing earlier states (e.g., Beusen et al., 2016)?

L23-24p9. Molar C:P ratios are already provided earlier. You can just say L19 that P is incorporated in organic matter accordingly to C:P molar ratios for tDOM and POM.

L6-12p10. Precise here that DIN export was calculated by subtracting the part contained in organic matter. Was DFe export equal to Fe inputs to catchments?

L11-16p11. This is not totally clear. Do you assume that only HCO₃⁻ affects alkalinity and that DIC is only HCO₃⁻ as well (Alk:DIC = 1:1)?

Sub-subsection 2.1.5 Silica. Why not use the same weathering model type as for P (Hartmann et al., 2011)?

Sub-subsection 2.2.1 Ocean biogeochemistry. A scheme with model state variables and processes would be helpful here (as well as a description of biogeochemical processing equations and parameters as Supplementary Information).

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L22p13. How did you choose the 0.003 d⁻¹ value for tDOM degradation? 0.008 d⁻¹ for the oceanic DOM is also in the literature range for tDOM degradation.

2.2.3 Pre-industrial ocean biogeochemistry model simulations. Please provide more details here, since the distinction between the REF and RIV simulations is key to understanding the paper's results. In the REF simulation, are input fluxes (per surface area?) globally homogeneous? Or do they compensate sediment losses to reach equilibrium locally (at the cell scale)? In the RIV simulation, are there also open ocean surface inputs in addition to river inputs?

L1-6p14. What do you call quasi-equilibrium? How were the lengths of the different simulation chosen? Why did you perform a succession of 3 runs for the RIV simulation? Does "standard simulation" (L4) refer to REF? Are the 100-year means (output results) calculated on the last 100 simulated years for each simulation? What are the simulation timesteps? It is mentioned that water loads vary inter-annually based on runoff inputs from OMIP. Are the ocean physics' inter-annual variations modelled with the same patterns for every simulated year?

Subsection 3.1 Runoff, precipitation and temperature patterns. Hydrology model performance is not the scope of the paper; this section seems to water down once more the message/goal of the study. The scaling of the MPI-ESM runoff should be detailed earlier, in the Methods section. How is the factor 1.59 chosen? Why is the runoff not scaled to the OMIP one, to be fully consistent with the freshwater inputs?

L20-24p16. For which periods were these Si loads from the literature estimated?

L26-32p16. Comparison with Mackenzie et al. (1998) does not seem necessary here, especially since they assess only the TIC load.

Subsection 4.1 Global loads in the context of published estimates L2p19. Does the Fe-P load given in Table 3 only corresponds to the fraction that is desorbed when entering the estuary?

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Table 3p19. Precise for the POP comparison that the 5.9 value (for 1970) is for PP, and not only POP.

L20-23p20. Doesn't the potential increase in Si retention at the end of the 20th century mainly concern particulate forms?

L20-26p21. Please better organize text.

L4p22. Could the discrepancies also be due to the fact that weathering formalisms are less adapted to Arctic regions?

5.1 Ocean state – An increased biogeochemical coastal sink

L14-15p25. "There is a higher carbon load originating from organic matter, since tDOM C:P ratio is higher than the oceanic DOM C:P ratio" -> This explanation is not straightforward, since the estimation of DOC is not related to P in the model.

L15p25-L2p26. Comparison with other studies was already discussed earlier.

L17-19p26. Isn't the effect of large oxygen minimum zones on DIN concentrations also visible in WOA dataset?

What explains the major differences in DIP, DIN and DSi concentrations in the Southern Ocean?

Table 5.p27. Do N inputs include N₂ uptake from the atmosphere in the two simulations?

L12-14p28. Comparison of Arctic concentrations to the WOA database could be presented earlier in text, with the rest of the comparisons.

7.1 Rivers in an Earth System Model setting. Paragraph 2 (L12-17p36). These improvements could further avoid strong assumptions, such as globally constant N:P ratios for river inputs, and on the spatial distribution of non-weathering sources.

8. Summary and conclusions

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L11-14p37. Is the need for a runoff scaling factor a major conclusion of this work?

L17-21p37. “Even for present-day...” -> comparison with literature is already discussed earlier, this could be removed.

L26-27p37. “which have been identified to be more strongly controlled by extreme hydrological events than other C species” -> Was this considered in the study? What does it imply? It seems that this belongs to some discussion (that should be more developed if added).

L17p38. “despite previously being sinks” -> be more explicit by explaining that this is without accounting for river inputs, and not “previously” in a temporal way.

L27-29p38. Interhemispheric C transfers are already mentioned earlier in this section (L1-3p38). Please focus in this last paragraph only on the major points that this study shows have to be included in ESMs to better assess land-ocean-atmosphere C transfers.

Appendix C

L4p42. “Table 3” -> “Table 2”. How do you get to the 280 Tg/yr of CO₂ drawdown? Some calculations could be shorter explained (e.g., CO₂ emissions explained L16-20p42 and L9-12p43).

— Other minor comments —

L5p1. “in a regional shelf analysis” -> “in regional shelf analysis”; there are more than one.

L10-11p1. “total C” -> “global C”

L12p1. “which is largely a result of a source from” -> “which mainly results from”

L22p1-L2p2. The last sentence could be split in 2.

L13p2. “exported to the sediment” -> “stored in the sediment”

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L18p2. Define NPP

L25p5. “their its ratios” -> “their ratios”

L30p5. “a fraction P” -> “a fraction of P”

L4p11. “and that 2 HCO₃⁻” -> “and of 2 HCO₃⁻”

L11p12. “dynamical nitrogen fixation through cyanobacteria” -> “dynamic N fixation by cyanobacteria”

L14p13. “its consistence” -> “its composition”

L11p17. “much stronger gradients of variation” -> “much higher spatial variability”

L21p17. End of sentence is missing or “which” should be deleted.

L17p20. Start new sentence at “Dürr et al. . .”

L2p21. “and to a framework” -> “and could be fed/incorporated into a framework”

L2p30. “compensate” -> “counterbalance”

L15p30. “due higher” -> “due to higher”

L18 p36 “consistence” -> “composition”

L19-25p43. Check parentheses.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-152>, 2019.

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