Response to Review#2

General Comments

The manuscript addresses an important question about material transport across the land-ocean aquatic continuum, and is of particular interest given its global application. I appreciate the substantial amount of work presented here, which includes a synthesis of existing methods to derive a global data set of riverine sources of nutrients and carbonate species to the ocean, long-term simulations of a global ocean-biogeochemical model, and analyses of CO2 outgassing hotspots and the origins and fates of riverine carbon from land/atmosphere to the ocean/atmosphere. I am particularly impressed that the authors were able to run the global model simulations for several thousand years long (even though the model is relatively coarse). The scope of this study is certainly appropriate for publication in Biogeosciences, however the manuscript in this current format requires clarifications in some places while in others the text needs to be shortened and/or streamlined in order to avoid distraction and help readers better capture the key points of this study.

Response: We would like to thank the reviewer for their helpful comments and criticisms, and are also pleased with the acknowledgement of effort put into synthesizing the state of knowledge of land-ocean biogeochemical exports, as well as our computational expenses to simulate such long time-periods.

We agree the manuscript should be streamlined and shortened to a certain extent to improve the reader's understanding of the main points.

Major Comments

Authors note: The comments found in the next paragraph of the reviewer's comment will be addressed individually.

My major concern about the study is the comparison of carbon (and other) budgets between the standard simulation (RIV, what does it stand for?) and reference simulation (REF), which led to most key conclusions made in the manuscript.

Response: In short, the reference simulation REF represents a previous model version of the ocean biogeochemistry model, which is lacking in terms of its constraints and geographical locations of "riverine" inputs. In the simulation RIV, we consider the magnitudes of the pre-industrial riverine inputs as well as their geographic input locations to the best of our knowledge. The focus is to analyze how adding riverine fluxes plausibly in terms of magnitudes, as well as at their correct geographical location (RIV) affected the ocean's biogeochemical state. This is done in comparison to REF, where these inputs are added to the open ocean. As a note, the overarching goal of having biogeochemical inputs in REF is to compensate losses to the sediment; without these inputs, all biogeochemical variables would strive to an equilibrium state equal to zero. This however does not mean that these inputs must be added at the sediment interface (nor is this a more "realistic" solution to the problem), we will revisit this as a response to a later comment.

We see the need to improve the explanations of the differences between the simulations REF and RIV in section 2.2.3., as well as in the explanations in Section 5.1 from p25.l10-p26.l11 on how the differences in REF and RIV should affect global ocean biogeochemistry variables (->what is our analysis strategy), since these points were not fully clear for both reviewers. This should be done in the revised manuscript. Cutting unneeded information, as pointed out by the reviewer, will also contribute to a better understanding of these points.

The authors described REF as a configuration where no river inputs are added to the ocean but "the burial loss of biogeochemical tracers was compensated by a global homogenous flux to the surface ocean", such that REF is "fully constrained by the loss of the sediment layer" (P13L26-29). This REF configuration seems a bit odd or at least not so clear to me.

Response: We would like to note that the REF simulation is the state-of-the-art previous model version, which is lacking in terms of its representation of terrestrial nutrient and carbon inputs to the ocean, since it considers these inputs solely at magnitudes to compensate for sediment losses, and they are added to the open ocean. We improve the model in RIV in order to represent the riverine loads magnitudes and geographical locations. We did not design the REF model setup for this study in particular, but it is adequate in order to assess the impacts of riverine loads added at their geographical river mouths in comparison to biogeochemical inputs added to the open ocean.

Is this addition of homogenous flux occurs during the same or next time step?

Response: The loss of biogeochemical compounds to the sediment is not computed dynamically within the model, but we approximated the sediment loss fluxes for a 100 year mean around every 1000 years of the REF simulation. The resulting fluxes are added to the model at every timestep.

We will shortly mention this in the revised manuscript.

Why choosing to use a homogenous flux instead of a spatially varying flux that directly compensates for the bottom loss at the same location? Wouldn't this framework to some degree arbitrarily homogenize the resource distribution across the ocean?

Response: In theory, this could be the case. We however show in the paper that the global open ocean distributions of for instance nutrients (DIN, DIP, DSi) and NPP are to a greater extent dictated by ocean circulation. Firstly, the distributions of these variables (for instance DIN oceanic concentrations) are not homogeneous in REF, which does not reflect the distributions of the inputs. Therefore the locations of the inputs clearly do not strongly dictate the distribution. Secondly, the difference between RIV-REF is relatively small compared to the "background" signal (For instance in the case of the NPP: Figure 8b). The mechanisms explaining the stronger importance of ocean circulation are that the fluxes added to the ocean to compensate for the sediment losses are small, especially per area, in comparison to the oceanic inventories of the compounds. This could be shortly shown quantitatively in the Supplementary Information of the revised manuscript. Secondly, the nutrients are transported away from their points of "addition" relatively rapidly, since we do not observe accumulation in the open ocean at the points of inputs (-> the distribution of the nutrients and NPP in the ocean is not at all homogeneous).

The reviewer is however to a certain degree correct, since by adding nutrient fluxes homogeneously to the open ocean, nutrient concentrations and NPP are slightly artificially increased due to the addition of nutrients to the open ocean. In reality there are no homogeneous surface fluxes to the open ocean. This is what we improve with the simulation RIV by eliminating biogeochemical compound fluxes to the open ocean and adding them to river mouths.

Why adding the flux at the surface instead of evenly distributing it throughout the water column?

Response: Our approach to add nutrient fluxes to the surface (REF) is slightly more realistic than adding them through the water column, since riverine fluxes also enter the ocean at the (near-) surface. Furthermore, adding biogeochemical compounds within all of the water column would also create an artificial signal: in areas where upwelling (rising of water masses from deeper layers to the surface) takes place, there would be an unwanted increase of compound supply. For instance, nutrients added to the deeper water column layers would artificially increase the NPP at the surface of upwelling areas, which is also not at all realistic. That being said, the RIV scenario is the most realistic of all mentioned scenarios, since it also considers the correct geographical location of river inputs.

How about distributing this flux only along the coastline (acting as a riverine source)?

Response: This is indeed a more realistic solution than was chosen in REF, but it would not allow us to investigate the effects caused by adding rivers to their correct geographical location in contrast to adding them in the open ocean, which is part of the focus in this manuscript. The difference between RIV and the suggested simulation would not be as large, since adding the flux to the coastline resolves to a certain degree the geographic distribution of the riverine fluxes. We are also not aware of any model approaches that tackle riverine inputs as a homogeneous coastline source, and therefore the found differences would not be of great use.

It seems that in this framework carbon (and other materials) is being relocated from the bottom to the surface and from some places to others without any explicit transport processes involved. And this would potentially make a HUGE impact on NPP and CO2 outgassing patterns regardless of riverine inputs.

Response: We show in Figure 8 that the impacts on the NPP between RIV-REF are relatively small, despite this large difference in distributions of the biogeochemical inputs. Therefore, the changes suggested by the editor are unlikely to be huge. We also would like to point out that this mentioned re-location is what happens in reality: riverine inputs at the surface are thought to approximately compensate sediment losses in the ocean sediment (the bottom). Thus, a "reflecting" sediment is not a more realistic scenario.

We already state in the manuscript the dominance of the physical circulation in dictating open ocean biogeochemical distributions and would like to avoid extending the manuscript with further discussions on other scenarios that we do not perform.

Also, what are other inputs to the REF beside this surface flux? Does REF also include N2 fixation? How is carbon synthesis associated with N2 fixation being handled in the model?

Response: Both model simulations include N$_2$ fixation, nitrogen deposition and dust deposition. These are all unchanged between in the simulations RIV and REF. At low DIN concentrations and favorable conditions, cyanobacteria fix N$_2$ in order to produce organic matter. The detailed description of cyanobacteria activity and their effects in the model can be found in Paulsen et al. (2017).

While we state that N$_2$ is fixed dynamically by cyanobacteria in Section 2.2.1, we will add revised manuscript the inputs of nitrogen deposition and dust deposition, and from which literature sources the values for the inputs were derived.

Depending on these details and whether the comparison between RIV and REF is justifiable, I recommend either a major (which would require a re-configuration run of the 5000-year REF experiment) or a moderate revision (which would be focused on streamlining and shortening the text plus clarification on some details as suggested below) of the paper before considering it for publication in Biogeosciences.

Response: Leaning on our previous responses, we do not see the need for an additional reference simulation. Using the current simulations enables us to assess the differences between biogeochemical riverine inputs as derived in the manuscript and adding the biogeochemical inputs to the open ocean, as was done previously in simulations in HAMOCC.

We agree that some additional clarification, as well as streamlining of the paper might help facilitate the understanding of the main points.

Minor Comments

P2L13: and also released to the atmosphere

Response: This is correct and will be corrected in the revised manuscript.

P2L19-P2L6: introductory information in these a few paragraphs needs to be streamlined. I suggest shortening it to 5-8 lines and expand Fig.1 to include more details on the processes to be considered or discussed.

Response: We assume the reviewer is addressing the whole introduction. The introduction will be streamlined and shortened in the revised manuscript.

P3L9: without reading the referenced literature, it is not so clear to me why riverine DIC would cause CO2 outgassing. Is it due to solubility change? Would riverine DOC/POC be also, if not more, likely to cause CO2 outgassing as a result of microbial respiration?

Response: The exact mechanisms which cause pre-industrial carbon outgassing, are often unclearly explained in literature. In this manuscript, we explain this in detail in section C2. Riverine DIC inputs consist majorly of HCO$_3^-$ inputs (or DIC:Alk = 1:1). In the process of carbonate production, HCO$_3^-$ is consumed while increasing the pCO$_2$ during its lifetime in the ocean:

$$Ca^{2+} + 2HCO_3^- => CaCO_3 + CO_2 + H_2O =>$$

The remineralization of DOM and POM also cause an increase in pCO₂, the released nutrients through the remineralization process however enhance the biological productivity, which counterbalances this effect and decreases the pCO₂. Therefore, it is the C:nutrients ratio within the organic matter which determines the extent of the pCO₂ and of the outgassing. As an example, for tDOM remineralization (first reaction) and the subsequent uptake of the released nutrients (second reaction):

$$C_{2584}PN_{16}^* => 2584DIC + DIP + 16DIN +^* => C_{122}PN_{16}^* + 2462CO_2$$
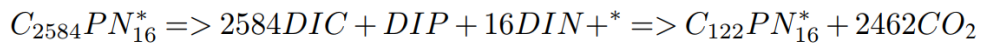
The extent of the long-term net outgassing is therefore strongly dependent on the carbon to nutrients ratio within POM and DOM.

P3L20: where does photodegradation most likely occur? In the rivers? Coastal margins? Is this process not reflected in the extremely high C:P ratio (2584:1) considered here?

Response: The degradation of the tDOM during its transit time within rivers is likely mostly biotic, thus explaining the low C:nutrients ratios found in tDOM at river mouths. Aarnos et al. (2018) derive from field measurements at 8 major river plumes globally that a substantial amount (around 30%) of tDOM is photodegraded within these plumes. In Fichot et al. (2014), which focuses on the Louisiana shelf, it is suggested that 40-50% of tDOM is mineralized on the shelf, with combined photodegradation and subsequent biotic breakdown being the major pathway for the degradation.

We believe this unfortunately exceeds the scope of our manuscript, since it is already very information-dense.

P3L24: why does POM control the availability of nutrients? By being remineralized?

Response: Indeed, POM contains nutrients that are released during mineralization of the POM. The strength of this control depends on various factors (other nutrient sources, POM composition, POM reactivity etc.).

This can be very shortly mentioned in the revised manuscript.

P4L13: which 10 years? Contemporary?
Response: The mentioned study performs a 10-year simulation only for the present day and for present-day riverine inputs. We can clarify this in the revised manuscript.

P5L8: Is there a particular reason to use 250m rather than the more commonly defined 200m?

Response: Using a depth threshold of 250m yields a better areal representation of the shelves in our relatively coarse resolution. This is especially the case for the narrower shelves.

P7L18: C:P=1000:1 here but 2584:1 in P9L23.

Response: C:P = 1000 is weight ratio and 2584:1 is the mole ratio. This is mentioned in both statements; we will however avoid switching from weight to mole ratio in the revised manuscript.

P10 section 2.1.3: why not deriving N:P ratios for different rivers from Global NEWS data set? Or at least to make a comparison with?

The Global NEWS data set prescribes very high N:P ratios, especially for dissolved inorganic species (approximately N:P = 30). This is plausible since this dataset represents inputs before their processing in estuaries. In estuaries, substantial denitrification and N outgassing takes place (i.e. Seitzinger et al., 2006), but these systems are not representable in the current state of global models due to resolution. We therefore do not have a choice but to simplify the ratios. The global P:N ratio of inputs to the ocean from estuaries should however approximately be 1:16, since the oceanic N source from N fixation and N deposition is thought to be approximately compensated by the sink from denitrification, leaving the elimination through organic matter formation and elimination, which takes place approximately at a P:N ratio of 1:16.

We mention our inability to take into account denitrification in p10.l10-l11, and that this escapes the scope of our study. We will add a brief discussion of the limitations of using fixed N:P ratios in section 7.1 in the revised manuscript.

P12L7: "river freshwater model" -> is this the MPI-ESM model mentioned in P7L3?

The inputs of the Ocean-Model-Intercomparison-Project do indeed originate from the Hydrological Discharge (HD) which is a component of the global Max-Planck-Institute Earth System Model MPI-ESM.

P12L9-18: need some clarification on the biogeochemical model configuration and references for different versions of the model. The original model is described in Ilyina et al. 2013, and the version being used in this study is the same with Mauritsen et al., 2018? The major changes include cyanobacterial N2 fixation and incorporation of DOM? Was the DOM improvement also made in Mauritsen et al., 2018? What about Paulsen et al., 2017 and Six and Maier-Reimer, 1996?

Response: The core of the model, which is essentially still the same, is described in Ilyina et al. (2013). The model developments that were incorporated since the Ilyina et al. (2013) manuscript were published are described in Mauritsen et al., 2018. As stated in our manuscript, the major changes were "to incorporate dynamical nitrogen fixation through cyanobacteria (Paulsen et al., 2017), to follow recommendations from the OMIP protocol (Orr et al., 2017) and to correct errors in the model. " p12.l11-l13

The references discussed for these already in the manuscript, cited after the individual model development (for instance Paulsen et al. (2017) for the implementation of cyanobacteria, p12.l12 and p12.l14). The model was already extended from a "classic" NPZD model in the Six and Maier-Reimer (1996) manuscript to consider a DOM pool. This model version did not strongly change from the Six and Maier-Reimer (1996) manuscript to the Ilyina et al. (2013) manuscript.

We will attempt to improve the last part of the paragraph in order to improve clarity in the revised manuscript. We will also add a general scheme of the main processes represented in HAMOCC.

Are these studies relevant to the HAMOCC model development?

Response: These model developments are relevant to HAMOCC and affect results of the model (See the individual studies).

Also, is the model (e.g. photosynthesis) N or P or C based?

Response: We do not fully understand this question. The standard model is an extended NPZD type model (represents nutrients, phytoplankton, zooplankton, detritus), which was extended with DOM and cyanobacteria. The nutrients inorganic pools are DIP, DIN, DSi and DFe and can all be limiting. The phytoplankton and cyanobacteria produce organic matter at a fixed C:N:P ratio (122:16:1) taking up dissolved inorganic nutrients and DIC. The remineralization processes (grazing, or bacterial remineralization) then release dissolved inorganic nutrients and DIC back to the water column.

This should be clearer with the process scheme in the revised manuscript.

Are there different pools for D/POC, D/PON, D/POP, etc?

Response: For the composition of the organic matter (phytoplankton, cyanobacteria, zooplankton, detritus, DOM), the model solely uses the globally fixed C:N:P ratios. This is mostly due to the computational expenses associated with having to compute 3x5 additional tracers to represent D/POC, D/PON, D/POP.

We think that this should be clear with the addition of the HAMOCC process scheme in the revised manuscript, and we can further state all biogeochemical compounds in the model.

Beside river inputs, are there other inputs from e.g. atmosphere deposition? Is N2 fixation also a carbon input or just N? These important details, particularly on the sinks and sources of N,P,C, need to be provided here.

Response: This is a good point that was forgotten in the submitted manuscript; the model also represents atmospheric dust and nitrogen deposition. These are estimated datasets for the pre-industrial time frame. Regarding the second question, at low enough DIN concentrations and favorable growth conditions for cyanobacteria (see Paulsen et al. 2017 for details), cyanobacteria produce organic matter (thus take up DIC, DIP, DFe from the water column), while fixing (atmospheric) N2.

We will add the description of the dust and nitrogen inputs from atmospheric and their literature sources in the revised manuscript.

Abstract: focus on the riverine impact on CO2 outgassing and NPP hotspots in the ocean and leave the details of the river export (e.g numbers in P1L8) to the result section. P1L15-19 can be removed or shortened to one sentence.

Response: This is a good point, but our results are strongly dependent on our estimation of the inputs, which is a substantial part of the manuscript, and we therefore believe it is important to give the reader a grasp of the magnitudes of the exports.

References:

Seitzinger et al. : Denitrification across landscapes and waterscapes: a synthesis. *Ecological Applications*, 16(6), 2064-2090, 2006.