Revised version of the manuscript bgd-2012-8

"Riverine Influence on the Tropical Atlantic Ocean Biogeochemistry

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Authors' reply to the comments – Referee #1

Please find here below a second reply to Referee #1's comments, now based on the latest revised version of the manuscript.

We have kept the Reviewer's comments in *italic* and have addressed them point by point. All changes to the manuscript text that are relevant to the comments are cited (page, line numbers) in our reply.

The most important change in the present version of the manuscript is that we have now used an updated version of the PISCES-T ocean biogeochemistry model called PlankTOM10, where the issues about nitrogen fixation in the tropical Atlantic Ocean could be addressed.

We'd also like to stress that the main objective of this sensitivity study was to assess the estimates of the upper and lower bounds of the potential impact of nutrient and carbon supply by rivers in the tropical Atlantic Ocean as a whole. It was not our objective to use a global ocean biogeochemistry model to assess river plume processes. We hope this new version of the manuscript is now suitable for publishing in Biogeosciences.

Reviewer #1:

However, for all the reasons listed below, I don't believe that this manuscript should be published. The authors seem to have done most of this work sometime around 2007 and don't seem to be up to date with findings and relevant papers published since (LeFevre, 2009; Subramaniam et al 2008; Mikaloff Fletcher et al 2007; Molleri 2010). Unfortunately for them, this would not be simply updating their reference list but fundamentally changing their approach and needing to rerun their model. Subramaniam et al 2008 showed that nitrogen fixation is stimulated by the Amazon plume and has profound consequences to the biogeochemistry and carbon cycling in the region. The fact that the authors don't seem to be familiar with the literature is reflected both in their approach and conclusions (e.g. Mayorga 2005 and 2010).

In the revised manuscript, we have both re-run the model simulations, now using an up-to-date version of the PISCES-T ocean biogeochemistry model called PlankTOM10 (Enright, Buitenhuis, & Le Quéré, 2012), and updated the bibliographic references, mainly concerning observations in the tropical Atlantic Ocean.

In PlankTOM10 nitrogen fixation, nitrification, denitrification and nitrogen-fixers (as a plankton functional type) are explicitly modeled. The details are given in page 4, section 2.1, in the revised manuscript.

Concerning the sea-to-air CO_2 fluxes in the tropical Atlantic, we were not quite sure the paper from (Mikaloff Fletcher et al., 2007) would be suitable for comparison with our model results: it deals with the ***natural*** CO_2 fluxes, i.e. the sea ↔ air CO_2 exchanges that existed before the pre-industrial era. Our model diagnoses the total sea ↔ air CO_2 fluxes, i.e. natural + anthropogenic CO_2 based in the difference in sea surface and atmosphere partial pressure of CO_2, wind speed, and a parametrization of the gas exchange coefficient.

In the table below are listed the PlankTOM10 sea ↔ air CO2 fluxes for the N and S tropical Atlantic Ocean, using the same longitude and latitude boundaries for the N and S tropical Atlantic Ocean as in (Gruber et al., 2009; Mikaloff Fletcher et al., 2007). Despite the increase in primary production provoked by the river nutrient inputs and the N2-fixation processes occurring in the tropical Atlantic Ocean, in this area as a whole there is CO2 outgassing.

Table 1 – Modeled (Gruber et al., 2009; Takahashi et al., 2009) sea-to-air CO_2 fluxes in Pg C a^{-1} in the north and south tropical Atlantic Ocean. Positive fluxes denote outgassing:

	Gruber et al (2009)	Takahashi et al (2009)	This manuscript
N tropical Atlantic	0.08	0.03	0.02
S tropical Atlantic	0.06	0.09	0.004

Concerning the N2 fixation in the western tropical Atlantic, we could now address it properly with PlankTOM10. Our model results suggest that river nutrients enhance N2-fixation offshore, thus alleviating nutrient limitation, and enhancing primary production (and carbon fixation) in the western tropical Atlantic. Our modeled N2-fixation rates are within the lower and upper limits of the in situ measured N2-fixation by (Subramaniam et al., 2008) → please refer to revised manuscript pages 7-8, lines 31-14, and page 9, lines 1-10.

The most fundamental problem I see with this work is that the model they are using may not be appropriate to study and for drawing conclusions on the influence of rivers on ocean biogeochemistry. As the authors themselves point out, 1) this is a global model that is not able to resolve coastal processes, 2) given the high seasonality of the processes being considered, using annual means is not adequate to understand the impact of rivers, 3) considering the importance of Fe and Si to coastal productivity, simply using average concentrations is not adequate, 4) while I don't believe that increasing the complexity of the model is necessarily important, it does not seem that processes important to understanding influence of rivers including the physics of the plume, light penetration etc are well represented. Not understanding and representing the lability of carbon and nitrogen or even considering phosphorus all seem to be fatal flaws in a study that purports to investigate the influence of rivers on ocean biogeochemistry. It would be useful to know how well the model simulates the extent and seasonality of the plume before getting into the effects on biogeochemistry – do the spatial extent and thickness seem right, does the plume happen at the correct months etc. For all these reasons, while I don't know that the conclusions presented by the authors are necessarily wrong – I don't know if they are correct or not but since I don't believe the processes leading up to the conclusions are correct, it is difficult for me to believe the conclusions.

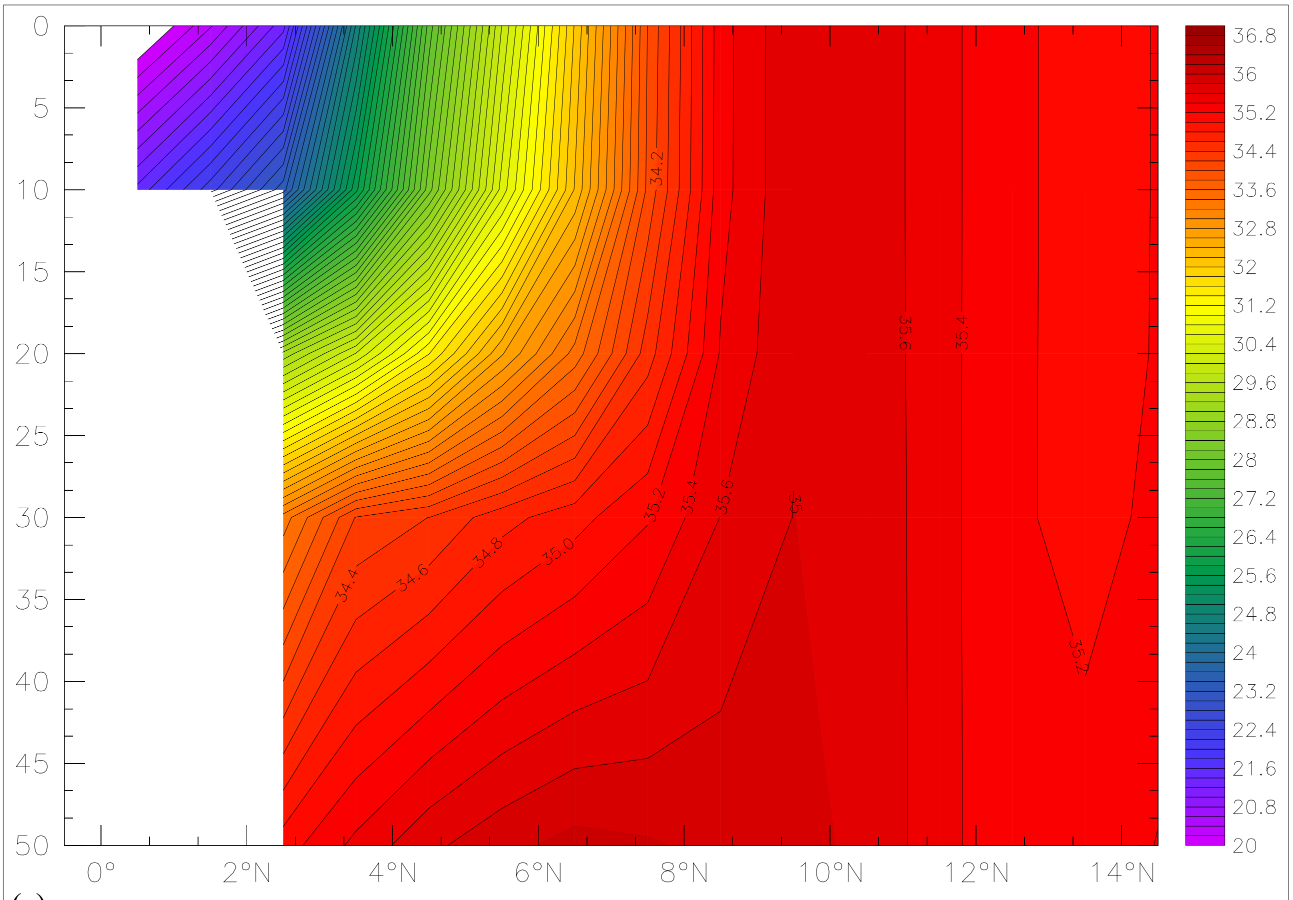
We would like to stress that the main objective of this sensitivity study was to assess the estimates of the upper and lower bounds of the potential impact of nutrient and carbon supply by rivers in the tropical Atlantic Ocean as a whole.

Section 2.1 in the revised manuscript (page 4) also states that phytoplankton growth is co-limited by nitrogen, phosphorus, silica and iron. We have also considered dissolved inorganic phosphorus inputs from rivers.

From our previous global sensitivity study (Cotrim da Cunha, Buitenhuis, Le Quéré, Giraud, & Ludwig, 2007), the model results suggested that in the coastal ocean under influence of high riverine nutrient and carbon input, the increase in primary production by riverine inputs is counter-balanced by an increase in organic matter respiration owing to increased transport of terrestrial OC and increased organic matter from new production. This is in agreement with a model study for the Arctic, where terrestrial OM was added to an ocean biogeochemistry model, decreasing by ~10% the net uptake of CO2 by the ocean because of remineralization (Tank, Manizza, Holmes, McClelland, & Peterson, 2011). A quick look at the SOCAT1.5 data (Pfeil, Olsen, & Bakker, 2012) show that surface seawater samples taken in Dec 1982 at the Amazon River mouth (closer to the shoreline than in (Subramaniam et al., 2008)) have high fCO2 values. We have added this comments in the revised manuscript results discussion (page 7, lines 20-30, page 11, lines 17-30, and figure 9 in the manuscript).

We fully understand your concern about the freshwater input, and the representation of the river plume (especially in the tropical W Atlantic). The ocean biogeochemistry model is coupled to an ocean general circulation model (NEMO) where continental/freshwater runoff (varying through the year, as a climatology) is included as a boundary condition (Huang, 1993; Madec & NEMO-Team, 2008).

Figure 1 below shows the vertical modeled salinity profiles for a section on the W tropical Atlantic Ocean, from 0° to 15°N, at longitude 50°W, average 1998-2005, in May (higher discharge) and December (lower discharge). The lower surface salinity is provoked by the freshwater discharge from the Amazon River. These vertical salinity profiles are valid for all model scenarios assessed in this manuscript.



(a)

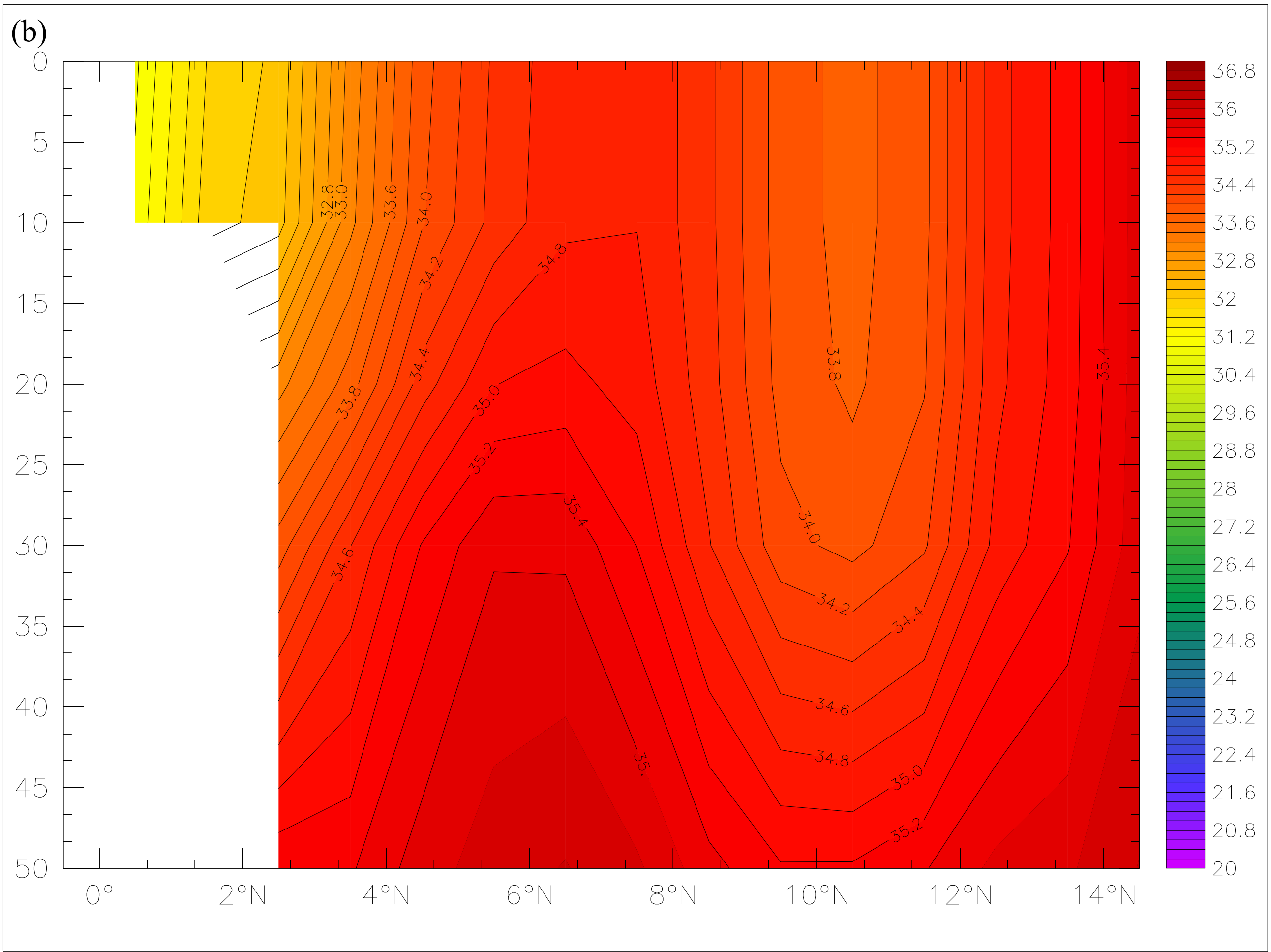


Figure 1 – Vertical section at 50ºW, showing the salinity average profile (color bar = salinity) at depth (0-50 m) in the area of the Amazon river outflow, (a) May (average 1998-2005), when the Amazon discharge is high, and (b) December (average 1998-2005), when the Amazon discharge is low. These salinity profiles are valid for all scenarios (NO_RIVER, TODAY, AFRICA, S_AMERICA).

There are several processes that are specific to riverine influence that would not be important in a global model but critical to understanding the influence of rivers on the biogeochemistry and productivity of waters in and adjacent to the plume – the supply of P, the specific penetration of light in the plume and to the waters below, the photomineralization of organic matter to produce labile nutrients (Morell and Corredor 2001), the adsorption/desorption processes associated with particulate material that would also be relevant to nutrient chemistry (Chase and Sayles 1980) etc. I believe that simply using modeled output of nutrients at the mouth of the river is not adequate as the model does not seem to take into account processes happening in the plume itself.

It was not our objective to use a global ocean biogeochemistry model to assess river plume processes, but to identify the main features at basin scale (tropical Atlantic Ocean) that are caused by riverine inputs of nutrients and carbon.

Specific Comments: Line 6, page 1947 – the area covered by the plume should be 2 million square km.

We are sorry for that, as stated in our first reply to the comments it was a typing mistake that escaped the reviewing for the discussion manuscript.

Lines 5-8, page 1948 – the formulation for light penetration in the ocean is not appropriate to study river plumes or waters affected by them. This problem would affect both the model's calculation of primary productivity as well as potentially the physics of the plume in terms of radiation absorbed and its impact on heating and buoyancy of the plume.

We are using a global ocean biogeochemistry to gain insight in the role of river nutrient and carbon discharge in the tropical Atlantic Ocean. We fully agree that the current model parametrization for light penetration may not be ideal to study river plume processes, but that was not our objective. We also fully support the idea of using a more adapted parametrization of light penetration in models assessing research questions specific to estuaries, river plumes and the coastal area. However, the Amazon plume and its seasonal variation in extent is satisfactorily represented for our purposes in the present model configuration "PlankTOM10 coupled to NEMOv2.3" (please refer to figure 1 in this document and to table 1 in the first reply to the reviewers).

Lines 5-9, page 1949 – I don't understand the use of mean error to represent the results. Why not present the absolute numbers as well. But taking a step back, it seems odd to compare whole basins – for example, how does one interpret the fact that the mean error for "Today" is larger than "No river"? To me, this seems to be an indication that the model is not doing a good job or the values being compared have problems. In addition, I don't see much value in comparing one model value against another or with highly averaged satellite data. At least, why not use time series at points where data is available?

Yes, indeed the mean absolute error for the tropical Atlantic Ocean in the PISCES-T simulations were lower for the NO_RIVER scenario. In the revised manuscript, we have calculated MAE again (please refer to Table 2 in the manuscript), and the values are lower for the TODAY simulation or similar.

Lines 1-3 page 1950 – what about comparison to the plumes themselves? How good is the model at reproducing the plume?

Figure 1 in this document shows that the the model is able to reproduce the low salinity plume in the western tropical Atlantic Ocean.

Lines 18-20 page 1951 – How is export production calculated? If, as it seems, it is based on NO3, the authors seem to miss the effect of nitrogen fixation and photolabilization of DON (Subramaniam et al 2008, Morell and Corredor, 2000).

EP corresponds to the amount of particulate organic matter exported below the euphotic zone. In this version we consider it at 100 m. We have added this information in page 7, lines 16-18.

Lines 15, page 1952 – section on Impact of African Rivers: The authors would well advised to read LeFever 2009 and Bakker et al 2001 where the influence of the Congo River on pCO2 is discussed.

We have added the suggested referenced to the revised manuscript (pages 12 and 13, influence of rivers in the eastern Atlantic). Indeed PlankTOM10 results are in agreement with the in situ observations (Lefèvre, 2009).

Lines 12-14 Page 1954 – why is there a salinity minimum in "No river"?

In all model simulations, including NO_RIVER, there was no reduction in the freshwater supply to the ocean. Such reduction in the freshwater input to the coastal zone would not only reduce river nutrient inputs but also diminish the river plume buoyancy effect on the shelves (i.e., estuarine water over seawater), which in turn reduces cross-shelf upwelling and the consequent upward nutrient input from subsurface waters and deep sea. We have re-written the methods section (pages 5-6) emphasizing that we haven't changed the model freshwater river input to the ocean.

Lines 18-23 Page 1954 – If this is the case, why is there an undersaturation in mesurements? Also how is organic C modeled as a nutrient in the model?

The model simulates an undersaturation of CO2 in low salinity surface waters as a physical effect, and the model results match in situ observations (Körtzinger, 2003).

Here is a quote from (Cotrim da Cunha et al., 2007) describing how riverine organic carbon (DOC and POC) act as a source of nutrients in the model: "we estimate a gross discharge of 148 Tg C a-1 and 189 Tg C a-1 for POC and DOC, respectively. We assume that DOC has a conservative behavior in estuaries. These values are in agreement with recent modeled values of 170 Tg C a-1 as DOC [Harrison et al., 2005], and 197 Tg C a-1 as POC [Beusen et al., 2005; Seitzinger et al., 2005]. We used a C:N:Fe ratio of 122:16:6.1e10-4, thus riverine DOC and POC, when they are remineralized, are also N and Fe sources to the ocean."

The information above can also be found in page 5, lines 11-13 in the revised manuscript.

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