Revised version of the manuscript bgd-2012-8

“Riverine Influence on the Tropical Atlantic Ocean Biogeochemistry

L. Cotrim da Cunha and E. T. Buitenhuis”

Authors' reply to the comments – Referee #2

Please find here below a second reply to Referee #2'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. Text passages of the revised manuscript are represented in **bold**. 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 #2:

Cotrim da Cunha et al. published a global modeling sensitivity study of the 'Potential impact of changes in river nutrient supply on global ocean biogeochemistry' in 2007 in GBC. This new 2012 manuscript uses the same global biogeochemical model with similar methods, but is focused on the impact of riverine nutrients on the biogeochemistry of the tropical Atlantic Ocean. It also specifically examines and discusses the role of South American and African rivers in these biogeochemical changes. The model used in this analysis admittedly does not fully resolve all processes that affect biogeochemical changes in the tropical oceans; however this does not necessarily mean that the results discussed here are not useful and interesting. The manuscript could be dramatically improved, however, if the comments listed below are addressed.

General Comments: (1) The model was spun up with real river nutrient inputs from 1948 until 1993, and then the rivers were turned off from 1993 to 2005 in one run (NO_RIVERS) and left on in another (TODAY). The averaged per year difference between these two runs from 1998-2005 was calculated as 0.2 PgC. It seems possible that deep nutrients in the open ocean would be very different in a simulation where the rivers were turned off in 1948, as compared to a simulation in which the rivers were turned off in 1993. Basically, are the authors sure only 5 years are needed to spin up the nutrients in this scenario? Can this be demonstrated?

In the present version of the manuscript, we have done the following (page 4, lines 28-30):

“The model was forced by the daily wind and water fluxes from NCEP/NCAR reanalysis (Kalnay et al., 1996) from 1948 to

2005 as in (Buitenhuis et al., 2006) and (Jones, 2003).”

and page 4, lines 4-7:

“We did four simulations with the PlankTOM10 model considering different riverine nutrient load estimates (Table 1). We ran the simulations from 1990 to 2005. This is long enough for the surface ocean to approximate steady state, as shown by the stability of the results after 3-4 years of simulations. We present average output for years 1998-2005.”

Here below we present a table showing the modeled annual values sea-air CO2 fluxes (in Pg C a-1), primary production (PP, Pg C a-1), and export production (EP, Pg C a-1) for the tropical Atlantic Ocean (70°W:20°E, 20°N:20°S) from 1998 to 2005:

Table 1 – Annual modeled values of sea-air CO2 flux, export production, and primary production for the PlankTOM10 simulations used in the second manuscript version.

	Sea-air CO2 flux		PP		EP	
year	TODAY	NO_RIVER	TODAY	NO_RIVER	TODAY	NO_RIVER
1998	0.08	0.08	4.07	3.68	0.53	0.49
1999	0.02	0.03	4.05	3.66	0.53	0.49
2000	0.00	0.01	4.00	3.60	0.52	0.48
2001	0.04	0.04	4.02	3.62	0.53	0.49
2002	0.02	0.03	3.97	3.56	0.52	0.47
2003	0.05	0.06	3.84	3.42	0.50	0.46
2004	0.02	0.02	3.73	3.30	0.49	0.44
2005	0.03	0.03	3.64	3.21	0.48	0.43

(2) In Table 2, all of the Mean Absolute Errors (MAE) between PISCES-T simulations NO_RIVER and available data for the tropical Atlantic Ocean are smaller than those between model simulation TODAY and available data. Does this mean that the model simulation NO_RIVER fits the real observations better than the model simulation TODAY in the tropical Atlantic Ocean? Lines 11-25 on page 1949 and lines 1-13 on page 1950 describe how good the agreement is between data and the TODAY simulation, with no mention of Table 2 and MAE at all. It appears that in the global scale model simulation, TODAY fits better with real observations (Cotrim da Cunha et al., 2007), but this does not appear to be the case for the tropical Atlantic Ocean. More explanation is definitely needed here.

After running the 4 simulations with PlankTOM10 model, we have re-calculated the mean absolute errors for Chlorophyll (comparing with SeaWiFS data for the same 1998-2005 period), surface dissolved oxygen, nitrate, phosphate and silicate concentrations (comparing with WOA2005), dissolved Fe (comparing with [1] data), primary production (comparing with satellite-based estimates for the period 1998-2005), and export production (comparing with estimates from [2]). The results are listed in table 2 in the manuscript (here below there is a copy of the table).

Table 2 – Mean absolute error (MAE) between PISCES-T simulations NO_RIVER and TODAY (average 1998-2005) and available data for the tropical Atlantic Ocean (20°S – 20°N, 70°W – 20°E)

Scenario name	Mean absolute error							
	Chla^a mg Chla L^{-1}	NO3^b µM	Si^b µM	PO4^b µM	Diss. Fe^c nM	PP^d gC m^{-2} a^{-1}	EP^e gC m^{-2} a^{-1}	Diss. O$_2$^{f} (µM)
NO_RIVER	0.18	0.79	1.85	0.10	0.44	59.58	6.39	5.83
TODAY	0.13	0.79	1.90	0.10	0.77	32.90	3.29	5.85

Legend: Available data reference: a) surface Chla estimated from SeaWiFS satellite, average 1998-2005, b) surface NO3, Si, and PO4 from World Ocean Atlas 2005 (Garcia et al., 2006a), c) dissolved Fe data from (Tagliabue et al., 2012); d) primary production average 1998-2005, satellite-based estimation (Behrenfeld, 2005), e) export production (Schlitzer, 2004), f) surface dissolved O$_2$ from World Ocean Atlas 2005 (Garcia et al., 2006b).

Our results suggest that PlankTOM10 fits better the observations in the tropical Atlantic, compared to PISCES-T. The exception is dissolved Fe. Tagliabue et al. data for the tropical Atlantic ocean are mainly “oceanic” data, with no measurements within the coastal ocean (Figure 1). In scenario TODAY, we assume an input to the oceans of 5% (95% retention) of the gross riverine dissolved Fe loads, which may be in excess compared to the “real world”. At present there isn't an estimate at river-basin scale of the dissolved Fe inputs to validate/correct our modeled inputs.

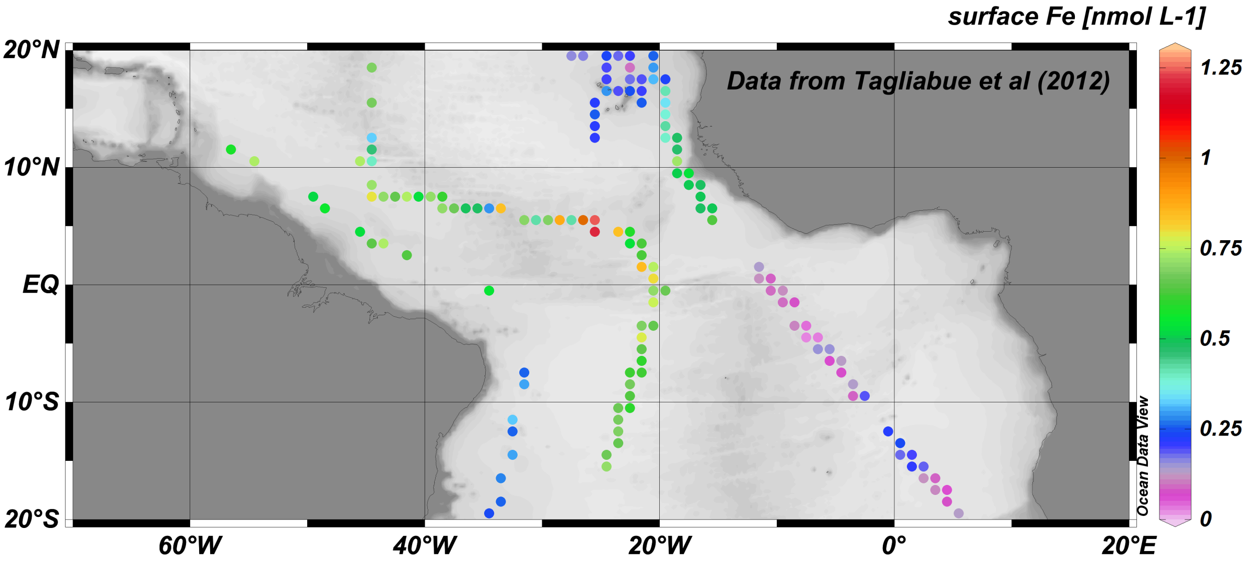

Figure 1 – Surface dissolved Fe (nM) distribution in the tropical Atlantic Ocean from the compilation of [1]

(3) Table 3 needs to be double checked in terms of the % calculations. For example, the % increase for CFLX and COASTAL CFLX for TODAY and S_AMERICA are not correct. The % increase numbers in the text need to be checked too. For example, line 22 on page 1950: isn’t the increase of 0.7 Pg C a-1 for open ocean PP equal to +9.2%? But the authors state +14%. Also, aren’t the first columns for PP, EP and CFLX numbers in Table 3 specifically for open tropical Atlantic Ocean, instead of for the whole tropical Atlantic Ocean?

In the revised manuscript these mistakes were corrected, and we produced a new table 3, where the changes for the whole tropical Atlantic and the changes for the coastal tropical Atlantic are listed.

We have also made sure that the values that appear in the manuscript text now correspond to the ones reported in the table. Here below there is a copy of Table 3 as it appears in the manuscript:

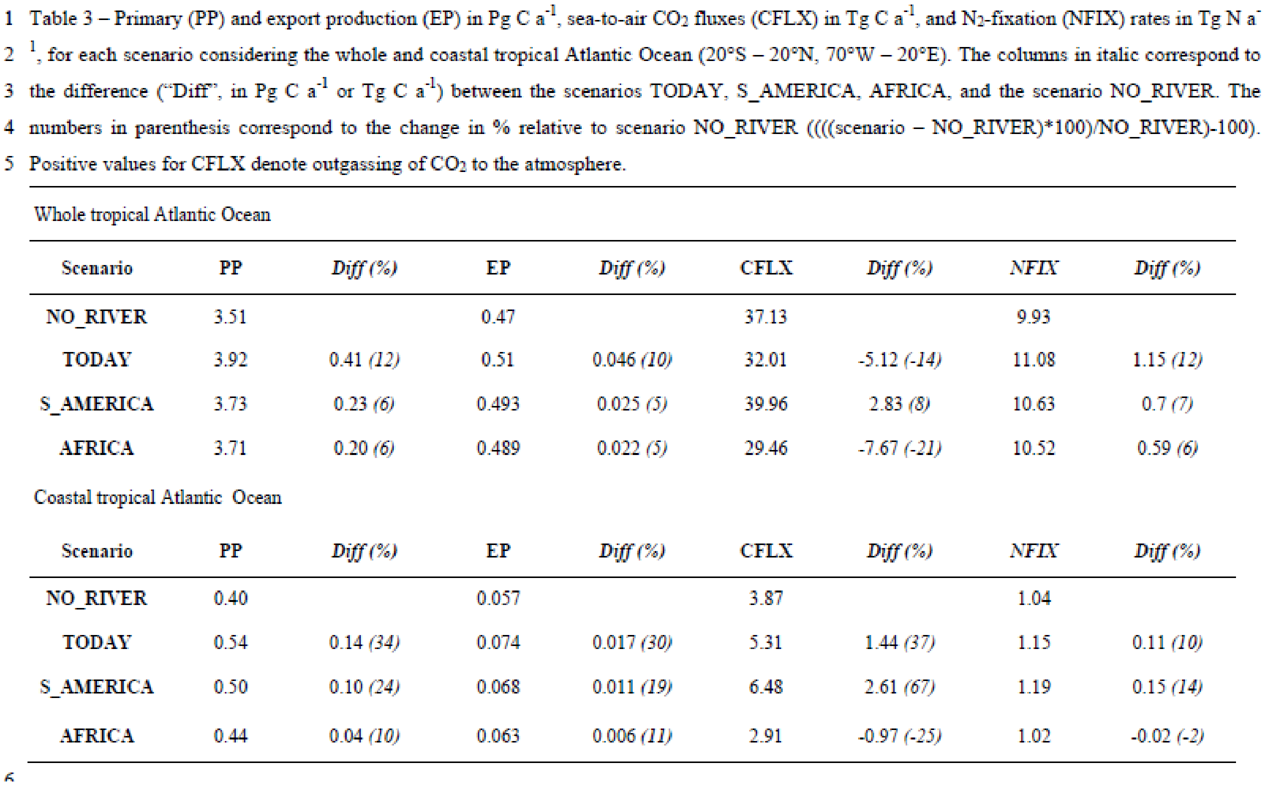

Table 3 – Primary (PP) and export production (EP) in Pg C a^{-1}, sea-to-air CO_2 fluxes (CFLX) in Tg C a^{-1}, and N_2-fixation (NFIX) rates in Tg N a^{-1}, for each scenario considering the whole and coastal tropical Atlantic Ocean (20°S – 20°N, 70°W – 20°E). The columns in italic correspond to the difference ("Diff", in Pg C a^{-1} or Tg C a^{-1}) between the scenarios TODAY, S_AMERICA, AFRICA, and the scenario NO_RIVER. The numbers in parenthesis correspond to the change in % relative to scenario NO_RIVER ((((scenario – NO_RIVER)*100)/NO_RIVER)-100). Positive values for CFLX denote outgassing of CO_2 to the atmosphere.

Whole tropical Atlantic Ocean

Scenario	PP	*Diff (%)*	EP	*Diff (%)*	CFLX	*Diff (%)*	NFIX	*Diff (%)*
NO_RIVER	3.51		0.47		37.13		9.93	
TODAY	3.92	0.41 *(12)*	0.51	0.046 *(10)*	32.01	-5.12 *(-14)*	11.08	1.15 *(12)*
S_AMERICA	3.73	0.23 *(6)*	0.493	0.025 *(5)*	39.96	2.83 *(8)*	10.63	0.7 *(7)*
AFRICA	3.71	0.20 *(6)*	0.489	0.022 *(5)*	29.46	-7.67 *(-21)*	10.52	0.59 *(6)*

Coastal tropical Atlantic Ocean

Scenario	PP	*Diff (%)*	EP	*Diff (%)*	CFLX	*Diff (%)*	NFIX	*Diff (%)*
NO_RIVER	0.40		0.057		3.87		1.04	
TODAY	0.54	0.14 *(34)*	0.074	0.017 *(30)*	5.31	1.44 *(37)*	1.15	0.11 *(10)*
S_AMERICA	0.50	0.10 *(24)*	0.068	0.011 *(19)*	6.48	2.61 *(67)*	1.19	0.15 *(14)*
AFRICA	0.44	0.04 *(10)*	0.063	0.006 *(11)*	2.91	-0.97 *(-25)*	1.02	-0.02 *(-2)*

(4) What are the consequences for ecosystem structure in the tropical Atlantic Ocean? Are there any changes in different scenarios as were discussed in Cotrim da Cunha et al. (2007)? It would be very interesting to have this simulated model result discussed in the manuscript.

The ecosystem structure represented in PlankTOM10 model is more complex than in PISCES-T, and it includes a plankton functional group representing the N2-fixers. We have focused our discussion in the N2-fixers and the silicifiers, as we understand these PFTs have a larger influence in the biogeochemical cycles. In the revised manuscript we have described:

a) how the PFTs are represented in the model (page 4, lines 2-16) :

2.1 PlankTOM10 Ocean biogeochemistry model

We use the PlankTOM10 ocean biogeochemistry model (Buitenhuis et al., submitted, Enright et al., 2012), which is based on plankton functional types (PFTs) as described in (Le Quéré et al., 2005). PlankTOM10 was developed from the model PlankTOM5.2 (Suntharalingam et al., 2012; Vogt et al., 2010), and represents ten plankton functional groups: silicifiers (diatoms), calcifiers, pico-autotrophs, DMS-producers, N2-fixers and mixed phytoplankton, pico-heterotrophs, proto-zooplankton, mesozooplankton and macro-zooplankton (Enright et al., 2012). For the phytoplankton types, the model estimates their total biomass in carbon units, iron, chlorophyll, and silicium content for the silicifiers as prognostic variables. For the pico-heterotrophs and the three zooplankton size classes, only their biomass is modeled. The model assumes that all PFTs have a constant C:N:P ratio of 122:16:1, and their Fe:C, Chl:C, and Si:C (only for silicifiers) are variable and determined by the model. The model includes phytoplankton growth co-limitation by phosphate, nitrate, silicate and iron. The nitrate pool undergoes denitrification and nitrogen limited growth by N2-fixers can add to the ocean nitrogen inventory.

b) In section 3.1 (Tropical Atlantic) of the revised manuscript, we have added a discussion about the impacts of river nutrients in the ecosystem structure (page 9, lines 1-12, and figures 5 and 6):

River nutrient inputs also impact phytoplankton composition in scenario TODAY because nutrient limitation (N, Fe) is

alleviated in the coastal ocean (Table 4, Figure 6, showing the thresholds of the nutrient half-saturation constant for diatoms). Surface chlorophyll associated to diatoms (silicifiers) has the largest relative increase in the whole tropical Atlantic when compared to scenario NO_RIVER, mainly in the coastal and near-coastal ocean area. The contribution of diatom-chlorophyll to total surface chlorophyll increases from 6% to 14% in the coastal tropical Atlantic. Diazotroph-associated chlorophyll increases in areas further off the coastal ocean, where Si and N become limiting to diatoms. Results shown in tables 4 and 5 suggest that most of the nitrogen fixation in the tropical Atlantic occur in the open ocean. An alleviation of N limitation in the northwestern tropical Atlantic Ocean off the coastal ocean suggest that enhanced N_2-fixation in scenario TODAY increases NO_3 availability in this area (Figures 5 and 6).

In the manuscript conclusions, we have also added a remark about the main impact of river nutrients to the tropical Atlantic ecosystem structure (page 15, lines 12-16):

In the case of the western tropical Atlantic, riverine nutrients would be able to maintain all the coastal export production, and in the eastern tropical Atlantic, riverine inputs partly alleviate the regional nutrient limitation. These results are confirmed by the relative increase in diatoms-associated chlorophyll (6% to 14% of the total surface Chla) in the coastal ocean, since diatoms have a higher nutrient requirement than the other phytoplankton groups. Our results suggest also that river nutrients may enhance N2 fixation in the western Atlantic Ocean, and thus alleviate open ocean nitrogen limitation.

(5) It is confusing to read in the Conclusions that the western rivers were responsible for up to 81% of the PP increase and that the eastern rivers were responsible for 48% of this increase, since 81+48 > 100%! After reading the paper in its entirety it is clear how these numbers were derived, but a solid conclusions section should be understandable even to a reader who only reads the Abstract and Conclusions. Can the authors instead simply say that the South American rivers were responsible for the majority of the increase in open ocean PP difference between the case with no nutrient inputs from the rivers, and the case based on real river inputs? Similarly, rather than saying that the African river nutrients are responsible for 71% of the open ocean EP difference and the South American river nutrients were responsible for 69%, the authors could simply state that the African and South American river nutrients contributed equally to the EP changes. In fact the authors seem to claim that 71% is significantly greater than 69%, but this is not substantiated. The conclusion here should be that both eastern and western rivers contribute nearly equally.

The manuscript has been re-written, and so the discussion about the influence of S. American and African to the tropical Atlantic. Our conclusion now is that, considering to total increase in primary production, S. American rivers and African rivers contribute equally. However, if we take into account the smaller amount (2-3 times less nutrients) of delivered riverine nutrients by African rivers, one may conclude that the African rivers have a higher impact on the tropical Atlantic. This is stated in the abstract (page 1, lines 25-29) and in the conclusion (page 14-15, lines 28-7) of the revised manuscript.

Specific Comments:
Abstract, line 2: Actually the authors compare three sensitivity cases (no rivers, east only and west only) to a reference run (both rivers). In the abstract they say they just perform two sensitivity tests, which is a little misleading.

This was corrected in the revised manuscript:
Abstract → page 1, line 15

Abstract, line 6: "70W-20" should be "70W-20E" (although it looks more like 12E?)

This was corrected in the revised manuscript:

abstract → page 1, line 18

Abstract, lines 8 and 9: How are 'open ocean' and 'coastal' quantitatively defined here? Are they separated by a specific isobath? Does the 'open ocean' value represent everything seaward of the 500m isobath, within the 20S-20N and 70W-20E region? If these were reported per square meter, presumably the effect on the coastal region would be much larger?

The open ocean value represents everything seaward of the 200m isobath within the studied region. We have added this information to the revised manuscript (page 7, lines 10-15), and compared the modeled primary production in the western tropical Atlantic (per m²) with other references in the literature:

In this study we have considered as "coastal area" the ocean area up to the 200m isobath. In the North Brazilian Shelf (NBS) area, modeled average primary production (18 mol C m^{-2} a^{-1}) was within the range of the measured PP values for the offshore area of the NBS (4-75 mol C m^{-2} a^{-1}, average 25 mol C m^{-2} a^{-1}, (Smith and Demaster, 1996)), and more recently 2-49 mol C m^{-2} a^{-1}, average 20 mol C m^{-2} a^{-1}, as fixed carbon (Subramaniam et al., 2008).

p. 1946, line 21: In addition to the three largest rivers, how many other rivers have direct discharge to the tropical Atlantic Ocean in this model? It would be helpful to have a map showing the location of the major and minor rivers.

There is a multitude of smaller rivers in the Gulf of Guinea coast and between the Amazon and Orinoco outflows in S. America, plus the relatively large São Francisco River on the Brazilian E coast. In the model description added that the river inputs are computed every 0.5° of latitude and longitude [3] (page 4, 17-22):

The model was forced with riverine nutrient (nitrate, phosphate, silicate, and iron) and carbon (DIC, alkalinity, DOC, POC) inputs as described in (Cotrim da Cunha et al., 2007) following a global river drainage direction map (DDM30) at 0.5° increments of latitude and longitude (Döll and Lehner, 2002). As before, cells corresponding to basin outlets to the ocean are used as input points for our PlankTOM10 simulations (Figure 2).

We have also added 2 figures to the revised version: figure 1 is a map of the Tropical Atlantic Ocean showing the location of the main rivers, and figure 2 shows the annual river freshwater input in km³ a-1 (logarithm scale) to the Tropical Atlantic Ocean – every data point in this map represents a source of nutrients to the model.

p. 1947, line 20: Sometimes the authors refer to DOM and POM and sometimes DOC and POC. Please be consistent.

We have revised the text, and in the 2nd version of the manuscript we have adopted "DOM" and "POM".

p. 1947, line 22: For this region, what percent of the freshwater input comes from the eastern vs. western rivers? How about for nutrients - what percent comes from the east and what percent comes from the west?

In Table 1 of the manuscript there are listed the amounts of nutrients from eastern and western rivers. We haven't added the amount of freshwater input because the latter was not stopped in any of the simulations, only the nutrients and carbon inputs. The nutrient inputs from S. American rivers are larger (2-3 times) than those from African rivers – but despite that, they equally contribute to the tropical Atlantic primary and export production. Please also refer to the answer to comment (5) in this manuscript.

p.1948, line 8: Because river transport is a main focus of this paper, it would be best to include at least a few sentences describing how Cotrim da Cunha et al. (2007) computed annual riverine inputs of nutrients.

This was done, and now a more detailed description of how we computed riverine nutrient inputs appears in section 2.3 – Model scenarios (page 5, lines 8-22):

- TODAY: This scenario considers riverine inputs of DIC, DOM, and POM estimated according to the models by (Ludwig and Probst, 1998; Ludwig et al., 1996a, 1996b). River carbon fluxes are mainly controlled by: drainage intensity and lithology (DIC), basin slope, drainage intensity and soil OM content (DOM), and sediment flux (POM). We used a C:N:Fe ratio of 122:16:6.1 10-4, thus riverine DOM and POM, when they are remineralized, are also C, N, P and Fe sources to the ocean. Dissolved inorganic nitrogen (DIN) and dissolved inorganic phosphorus (DIP) inputs were estimated based on a regression model by (Smith et al., 2003). Dissolved Si inputs were calculated using the runoff data from the DDM30 map, and applying an average concentration of dissolved Si in river waters of 4.2 mg Si L-1 (Tréguer et al., 1995). Additionally, it has riverine Fe input, considering 95% loss of dissolved Fe in the estuaries at low salinity, and an average concentration of dissolved iron in river waters of 40 µg L-1 (Martin and Whitfield, 1983). Literature suggests that around 80% up to more than 95% of the riverine dissolved iron loads are removed from the dissolved phase inside the estuary at low salinities (Boyle et al., 1977; Chester and Jickells, 2012; Lohan and Bruland, 2006; Sholkovitz, 1978).

We have also decided to consider 95% loss of dissolved Fe in the estuaries for the revised manuscript in order to be more consistent with the values reported in the literature.

p.1948, line 10: This is a little confusing, because the Carr comparisons were for 1998. Is this model run (1948-2005) the same as that used in Friedrichs et al. (2009, JMS 76, 113-133.) It also might be worth noting that this model did extremely well, and in fact nearly the best of all the biogeochemical ocean circulation models tested, in Saba et al. (2010, GBC, 24, GB3020; doi:10.1029/2009GB003655.)

The first manuscript version used PISCES-T, the same used in [4], but the version used in [5] is an updated version of PISCES-T called PlankTOM5. As PISCES-T is no longer under development, our revised manuscript uses an updated version of PlankTOM5 – PlankTOM10. In the revised manuscript we could assess the impact of riverine inputs also to the nitrogen fixation in the tropical Atlantic, as pointed out by referee#1.

p.1948, line 11: If the model reaches steady state after 3-4 years, why was a 44-year spin up needed?

The physical model (NEMOv2.3) needs a longer spin up for the temperature and wind forcing (those have a large control in the sea ↔ air CO2 fluxes). Planktom10 is embedded in NEMOv2.3.

p.1948, line 21: This is a little misleading, because damming would stop water flow, which is not stopped in this experiment. "due to river damming" should be removed. Also on line 28 "the South American river inputs were stopped" should be changed to something like: "Nutrient input through the South American rivers was stopped."

We have removed “river damming” and used **“... riverine nutrient fluxes would stop completely”** instead (pages 5-6, lines 23-2).

p. 1948, line 24: Can a reference be provided for the 99% Fe loss?

The 99% Fe loss was considered as a minimum value for a net river Fe input. The values found in the literature vary between 80% and 95% [6,7]. The PlankTOM model version uses 95% river Fe loss. Please also refer to the comment concerning page 1948 (top of this page).

p. 1949, line 19: The text says what was used in TODAY for OC, but not for DIC. Are the modeled or measured values used in "TODAY"? Why are the modeled values so far off? What impact will this have on the results of the sensitivity studies discussed here?

River DIC and alkalinity inputs are computed from a model developed by [8,9]. This approach was first used by [10], in a former PISCES version. The Amazon and Congo DIC concentrations and the river discharge used in [11] were measured, and then the fluxes were modeled, so that may explain the discrepancy. We have added a comment in page 6, lines 22-24:

We attribute this to the difference in the methods applied for estimating DIC flux (discharge-weighed for the measurement values in both cases, Congo and Orinoco).

p. 1949, line 23: If the model produces values 75% lower than observed, is this really our "best estimate"? Why not force the model with the observed river nutrient fluxes? Wouldn't that be a better estimate?

Given the large number of rivers that discharge in the tropical Atlantic Ocean and the paucity of data concerning nutrient fluxes in this region, to force the model with observed river fluxes is unfortunately not possible yet. It would also be difficult to combine the model forcing with observations and modeled fluxes where data is not available. But we do agree that using observational data would certainly produce a better estimate.

p. 1950: Reference to Figure 1 is needed here.

The revised manuscript is very different from its first version, and now Figure 1 in the manuscript is now the map showing the main rivers in the tropical Atlantic.

Figure 1: Why not show (also or instead) satellite surface chlorophyll for the particular years being analyzed here? In Figure 1 it looks like climatological (in situ?) chlorophyll is being compared to a model run for 1998-2005. Why not compare satellite chlorophyll from 1998-2005 with model output for the same years? Also, can the 3 large rivers be added on here, and labeled? Is there any reason a log scale wasn't used here?

In the revised manuscript we have used SeaWiFS data for the same period (1998-2005) in figure 3 and also to estimate the mean absolute error. We haven't added the location of the 3 main rivers because we have added the map in figure 1. In the first manuscript version we used the same scale as in [12]. Here we chose a different scale, which we believe show the areas under influence of river inputs.

p. 1950, line 9: How do you quantitatively define open vs. coastal ocean?

We consider as “open ocean” the model domain seaward of the 200m isobath.

p. 1950, line 20: “rivers outflow” should be “river outflows”

Thank you for the correction. Please be aware that some passages of the revised manuscript may have completely changed, and some comments/corrections may not apply anymore.

p. 1950, line 25: “open ocean eastwards the Congo” Grammatically this doesn’t make sense, and is ‘eastwards’ supposed to be ‘west of’?

Thank you for the correction – this mistake was not seen when revising the text. The meaning was supposed to be “west of”. Please also refer to the reply to the comment above.

p. 1950, line 26: But the percent increases seem to be nearly exactly the same size (in terms of EP) and much greater in terms of air-sea CO2 flux. This needs to be discussed.

The new model results are quite different than those of the first manuscript. In the revised version, our results suggest that the modeled decrease in sea-to-air CO2 fluxes in scenario TODAY correspond to the increase in PP and EP (page 9, lines 13-16):

Sea-to-air CO_2 fluxes for the whole tropical Atlantic ocean decrease by 5.12 10^{-3} Pg C a^{-1} (-14%), corresponding to the relative increase in PP and EP for the whole tropical Atlantic. However, in the coastal area, model results suggest an increase in sea-to-air CO_2 fluxes relative to NO_RIVER by +37% (1.44 10^{-3} Pg C a^{-1}).

The relative increases/decreases in PP, EP, N2-fixation and CO2 fluxes are listed in table 3 in the revised manuscript.
We have also added 2 sections: 3.2.1 (River nutrients and the sea-to-air CO2 flux in the western tropical Atlantic) and 3.3.1 (River nutrients and the sea-to-air CO2 fluxes in the eastern tropical Atlantic), where this is also discussed, and a remark in the section Conclusions. The 2 new sections in the manuscript were added because the new results suggest that CO2 fluxes are very different in the coastal ocean for both western and tropical Atlantic.

p. 1950, lines 9-12: This model-data comparison doesn’t seem appropriate for this section which is discussing an idealized sensitivity experiment. (Similarly, the model data comparison on p. 1952, lines 21-24 would also be more appropriate when discussing the TODAY simulation.)

We have moved the model-data comparison text to sections 3 (Results) and 3.1 (Impact of rivers in the tropical Atlantic Ocean biogeochemistry).

p. 1951, line 16 & page 1953, line 21: The description of increase on EP and sea-to-air CO2 flux needs to be more accurate. As shown in Table 2, both increases on EP and sea-to-air CO2 are much smaller than those on PP on absolute flux. The % increase on sea-to-air CO2 flux, however, is very significant.

The description of the changes in PP, EP, sea-to-air CO2 fluxes and now also nitrogen fixation has been completely changed in the revised manuscript. Table 3 is now updated, and the impact of river inputs to the tropical Atlantic sea-to-air CO2 fluxes (and especially the larger impact in the coastal ocean) is discussed separately in sections 3.2.1 and 3.3.1 (added to the revised manuscript).

p. 1951, line 17: How could the doubling of CFLX (from .03 to .06 in Table 3) be due to riverine outgassing, when the rivers don’t appear to be inside the model domain?

The phrasing was not clear: we meant there was outgassing at the river outflow due to the large input of riverine DIC.

p. 1952, line 16: Actually it looks like there is some change between 10-20N in the West. Why is that?

At first, we thought there could be some influence from the river inputs in the Caribbean sea, but we have found an error in the river nutrients forcing files for both scenarios S_AMERICA and AFRICA used in the former manuscript version. This has now been corrected and the new primary production figure (figure 3) does not show this “change” neither for the western nor the eastern Atlantic.

p. 1956, line 26: "of the increase in EP if the same export production increase". Not sure what is meant by this? Perhaps some words are missing here?

This was corrected in the revised text – there were words missing in this sentence. One should read: “... 69% of the EP increase.”. But the text has changed in the revised manuscript, so this no longer applies.

p. 1956, line 27: "depend" should be "depends".

This was corrected in the revised text.

p. 1957, line 2: "eastern margin" should be "eastern ocean margin" to make it clear that you mean the eastern ocean (which is along the western continental margins.)

This was corrected in the revised text.

p. 1957, line 11: "On" should be "In"

This was corrected in the revised text.

Figures 2-4: These figures would be much clearer if they were labeled as to what each panel represents. This information is included in the caption, but it would be best to also include this information on each panel.

All figures now have a label corresponding to the information in the caption text, and units were also added to the color scale.

References:

1. Tagliabue, A. *et al.* A global compilation of dissolved iron measurements: focus on distributions and processes in the Southern Ocean. *Biogeosciences* **9**, 2333–2349 (2012).
2. Schlitzer, R. Export Production in the Equatorial and North Pacific Derived from Dissolved Oxygen, Nutrient and Carbon Data. *Journal of Oceanography* **60**, 53–62 (2004).
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