Reply to Referee #2 comments on the BGD manuscript “Riverine influence on the tropical Atlantic Ocean biogeochemistry”

The authors are grateful to Referee#2 for the very constructive comments and suggestions to improve the manuscript quality. I (Leticia C. Da Cunha) would like to apologise for the delay in submitting the reply to the interactive comments. As I have also mentioned to Referee #1, I have moved from Germany to Brazil in May this year, to start a new position at Rio de Janeiro State University, and the settling up took much longer than I expected, both in my professional as in my private life.

In this document, the original referee's comments are shown *in italic*, followed by the authors' reply.

We would also like to inform that we are running the model sensitivity tests again, now using an updated version of PISCES-T called PlankTOM. The project web site is found at: (http://lgmacweb.env.uea.ac.uk/green_ocean/model/model.shtml?r1). We decided to proceed like this after reading the comments from Referee #1 about the role of nitrification in the Amazon river plume, and the comments from both reviewers about the mean absolute error (MAE) values for the tropical Atlantic being higher for the model version including riverine nutrients and carbon inputs.

I would kindly ask the reviewer to keep in mind that the absolute values shown in the first manuscript version are supposed to change with the new sensitivity runs. In this document the questions/suggestions were addressed regarding the PISCES-T simulations, and some preliminary MAE data using the new PlankTOM simulations are shown here in table 2.

(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?

I suppose the 0.2 Pg a^{-1} difference between NO_RIVER and TODAY refers to the sea-air CO2 flux. For the publication of the 2007 article (1) we have prepared a table with the annual modelled values of primary production, export production, and sea-air CO2 fluxes in order to check if the 5 years were enough to spin up the different nutrient scenarios (Table 1).

Table 1 – Annual modelled values of sea-air CO2 flux, export production, and primary production for the PISCES-T simulations used in the first manuscript version.

	Sea-air CO_2 flux	(Pg C a^{-1})	Export prod.	(Pg C a^{-1})	Prim. prod.	(Pg C a^{-1})
year	NO_RIVER	TODAY	NO_RIVER	TODAY	NO_RIVER	TODAY
1993	-2.48	-2.43	9.30	9.43	70.10	71.32
1994	-2.37	-2.30	9.29	9.51	69.74	71.96
1995	-2.31	-2.23	9.16	9.39	68.74	71.44
1996	-2.16	-2.07	9.14	9.41	68.73	71.77
1997	-2.38	-2.29	9.06	9.34	68.98	72.31
1998	-2.55	-2.46	8.93	9.25	68.35	71.91
1999	-2.04	-1.93	9.16	9.47	69.05	72.72
2000	-1.90	-1.80	9.11	9.44	68.45	72.29
2001	-1.85	-1.74	9.02	9.36	68.49	72.40
2002	-2.45	-2.34	8.96	9.31	68.61	72.66
2003	-2.32	-2.21	8.97	9.32	68.46	72.60

2004	-2.19	-2.07	9.09	9.45	69.01	73.19
2005	-2.32	-2.19	9.08	9.45	68.97	73.22

(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.

As mentioned in the beginning of the document, this was one of the reasons to re-run the sensitivity tests using an updated version of the model. The explanation for the higher MAE values for Chl (as an example) the TODAY scenario is that, on the one hand the model does not resolve the high surface Chl concentrations on the NE tropical Atlantic (from the WOA01 database) and, on the other hand simulates high surface Chl concentrations on the SE tropical Atlantic (panel "C" in figure 1 from the 1st manuscript version). Here below we present the preliminary MAE values for the tropical Atlantic using the new runs and data (Table 2).

Table 2 – Mean absolute error (MAE) between PlankTOM simulations and available data for the tropical Atlantic Ocean (70°W-20°E, 20°S-20°N).

	PARAMETRE				
MODEL	Chl (WOA01)	O2 (WOA05)	PO4 (WOA05)	Si (WOA05)	EXP (schlitzer JO2004)
JUNC	**0.02**	**1.15**	**0.08**	**1.66**	**3.29**
NRIV_JUNC	**0.08**	**1.55**	**0.09**	**1.85**	**6.39**

WOA01 and WOA05 = World Ocean atlas 2001 and 2005, respectively.
Schlitzer JO2004 = (2)

(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?

Yes, there is a mismatch between the text and the table, I am sorry for that. The first columns for PP, EP and CFLX refer to the open tropical Atlantic. We will revise the text and tables with the new modelled values.

(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.

In the first manuscript version the simulations suggest that increased Fe availability has a direct impact in diatom biomass, especially in the coastal ocean of eastern margins receiving high river inputs (like the Gulf of Guinea and the area off the Congo river mouth). We will add a section to discuss the impact in the plankton biomass and ecosystem structure in the revised manuscript, especially because PlankTOM has more functional groups.

(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.

After re-reading the manuscript and the comments it is clear that way the numbers (increase in PP etc.) were presented in the conclusion are not clear enough. The section will be re-written in the revised version using the new simulations results.

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.

Thanks for the comment – this will be re-written in the revised manuscript.

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

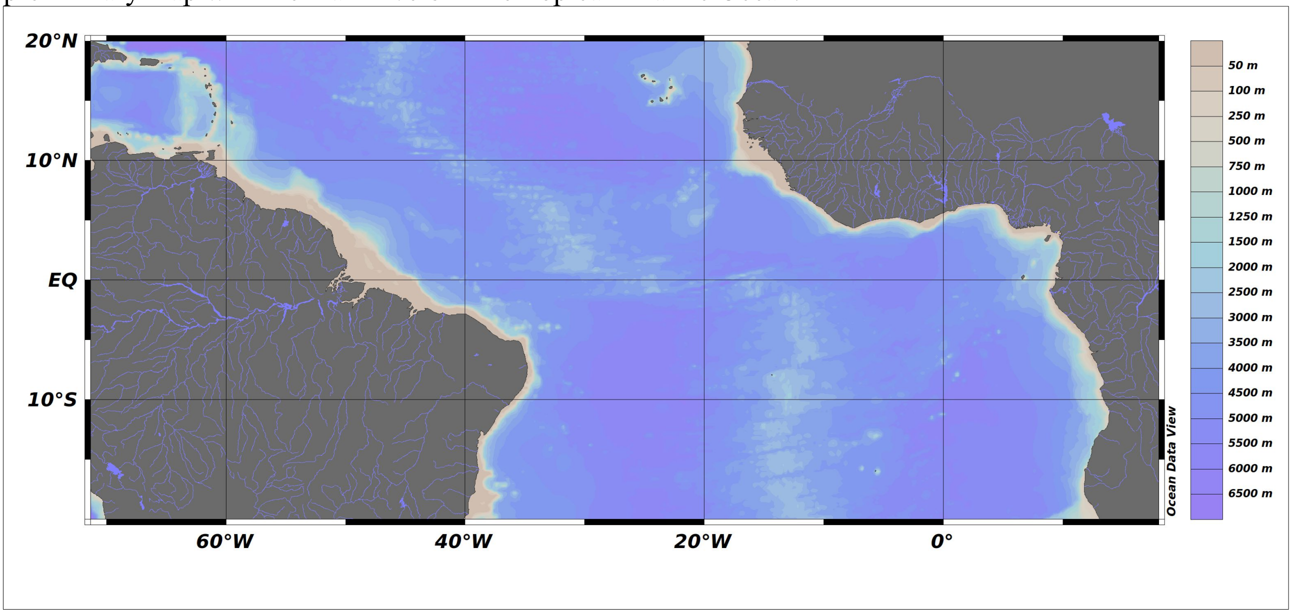
Yes, this was a typing mistake. The studied region is between 70°W-20°E, 20°S-20°N.

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. The definition will be added to the revised manuscript. I agree that the modelled primary production, EP, and CFLX values reported per square meter would be larger. The new calculated results will be added to the revised version.

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 still space for a figure similar to Figure 1 from (1) zooming into the tropical Atlantic. 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 we will also add that the river inputs are computed every 0.5° of latitude and longitude (3). Here below there is a preliminary map with the main rivers in the tropical Atlantic Ocean:



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

This will be corrected in the revised manuscript.

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.

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.

Yes, we'll improve the model description section in the revised manuscript. It may very confusing for a reader not familiar with the 2007 article.

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 Saba et al (5) is an updated version of PISCES-T called PlankTOM5. Our revised manuscript uses an updated version of PlankTOM5, mainly because of the criticisms of referee #1 about the importance of the nitrification processes in the tropical western Atlantic. Additionally, PISCES-T is no longer under development.

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

The longer spin-up time is needed for the forcing (e.g. winds, temperature → that have a large control in the sea-air CO2 fluxes) of the physical model (OPA) where PISCES-T is embedded (6).

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.

The "river damming" will be removed in the new version.

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."

Ok, the sentence will be changed for both AFRICA and South America runs.

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% (7, 8). The PlankTOM model version uses 80% river Fe loss, and this will be discussed in the model description section.

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 (9, 10). This approach was first used by (11), in a former PISCES version. The Amazon and Congo DIC values from Probst et al. (12) were measured, and then the fluxes were modelled. It is possible to add a short discussion on this topic, giving details on how DIC and alkalinity are computed in the model, and why they are different from the available data from Probst et al.

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?

This observation is valid, but it would be very difficult to use different river nutrient/carbon fluxes for each freshwater input point in the model, and also use the Ludwig 1996 models (9, 10) where data is not available. Additionally, in the cited literature, the methods used to estimate riverine inputs slightly differ from each other. This is why we considered it as "our best estimate" - but not meaning this is the best existing estimate.

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

We'll add it in the new manuscript version – in this case it will probably be Figure 2 when the river input figure is added to the manuscript.

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?

It is possible to add a panel with Seawifs observations. In a first moment we thought it would be more interesting to compare the model with observational data when available. The location of the main rivers outflow can also be added to the figures. We used a similar scale to compare the surface chlorophyll concentrations in our previous 2007 article.

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

This will be added in the revised version: we consider "open ocean" the model domain seaward of the 200m isobath.

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

The expression will be corrected in the revised version.

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".

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.

Yes, thank you for the comment, and it is complementary to the comment about the EP and CO2 flux in p. 1951 and p. 1953. Despite being small changes in absolute values, it should be discussed why. In the manuscript we have used the EBP parameter and the ratios between river input:EP to evaluate why the increases are small. The model results suggest that the coastal tropical Atlantic remains N-limited (Table 3), and that a decrease in EBP in scenario TODAY suggests that the primary production is "recycled" in the upper ocean layers, and that little material is exported below 200m.

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.)

Did the reviewer mean page 1951, lines 9-12? Yes, the comparison could be changed to the discussion of the TODAY simulation. In a first moment the model-data comparison was used in both passages (S_AMERICA and AFRICA) to show that, despite being sensitivity tests, the simulations still reproduce observations.

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.

Yes, the reviewer observation is right, but in the text we didn't want to emphasize the % increase because the absolute flux is very small. We will re-write the discussion to avoid this confusion on how significant the increase in EP and CO2 fluxes is.

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?

Sorry, the phrasing is not clear here: we meant that the river organic inputs are mineralised, leading to an increse in surface CO2 fluxes. The sentence will be re-written in the revised version.

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

The corresponding values are around 10-20 mgC m^{-2} a^{-1}. Besides, the river nutrient inputs for the Caribbean and N. America was not stopped. If this sentence is misleading, the above explanation can be added to the text.

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?

Sorry, there were lost fragments in the sentence. One should read: "... 69% of the EP increase."

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

The verb will be corrected in the 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.)

The expression will be corrected in the text.

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

The preposition will be changed in the 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.

More information can be added to the panels, together with the location of the main rivers outflow, as previously suggest.

References:

1. Cotrim da Cunha L, Buitenhuis ET, Le Quéré C, Giraud X, Ludwig W (2007) Potential impact of changes in river nutrient supply on global ocean biogeochemistry. *Global Biogeochemical Cycles* 21:GB4007 ST – Potential impact of changes in river. Available at: http://dx.doi.org/10.1029/2006GB002718 [Accessed November 2, 2011].

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3. Doell P, Lehner B, Doll P (2002) Validation of a new global 30-min drainage direction map. *Journal of Hydrology* 258:214–231. Available at: <Go to ISI>://WOS:000173810800015.

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11. Aumont O et al. (2001) Riverine-driven interhemispheric transport of carbon. *Global Biogeochemical Cycles* 15:393–405.

12. Probst JL, Mortatti J, Tardy Y (1994) Carbon river fluxes and weathering CO2 consumption in the Congo and Amazon river basins. *Applied Geochemistry* 9:1–13. Available at: <Go to ISI>://WOS:A1994NM02500001 [Accessed August 30, 2012].