Dear Editor, dear Reviewer #1,

I would like to thank you again for your comprehension with the delay. Please find in this document the reply to referee #1 comments (*in italic*). At the moment we are running the sensitivity tests with an updated version of the model used in the first version.

Referee #1:

Page 1 – "...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)."

The cited references will be included in the revised manuscript. The paper from Mikaloff-Fletcher (1) will be especially useful for comparing model results in the tropical Atlantic area since we are able to use the same "provinces" as in the cited study.

Page 2 – "... 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."

That is exactly our strategy for the revised manuscript: we are running a new version of the model where nitrogen fixation, nitrification, denitrification and nitrogen-fixers (like *Trichodesmium* sp. and unicellular prokariotes) are explicitly modelled. We believe that in the new simulations, our model results will be able to capture, together with the Amazon plume CO2-sink, a larger increase in regional new production, as shown in (2).

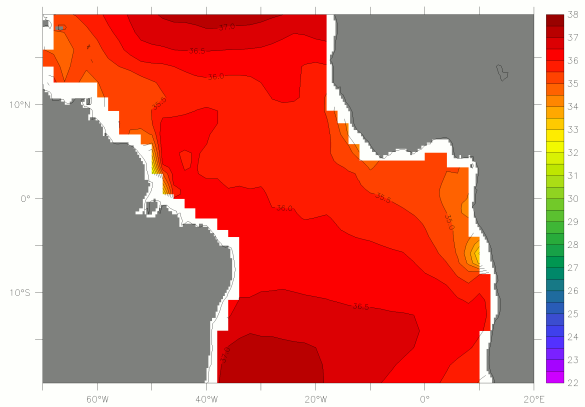
Page 2 – "The most fundamental problem I see with this work is that the model they are using may not be appropriate to study and for drawing conclusions on the influence of rivers on ocean biogeochemistry..."

Concerning the model ability to represent coastal area processes, we'd 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. From our previous global sensitivity study (3), 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. A quick look at the SOCAT1.5 data (4) show that surface seawater samples taken in Dec 1982 at the Amazon River mouth (closer to the shoreline than in (2)) have high fCO2 values.

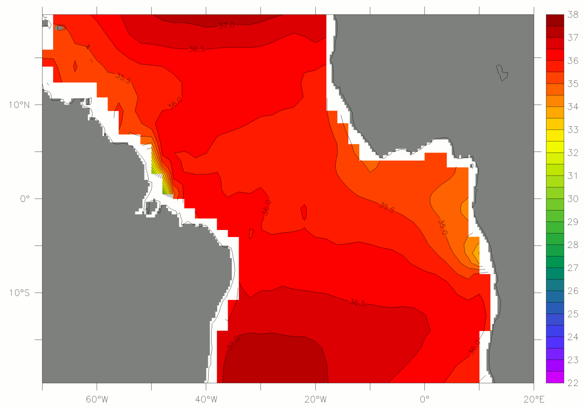
We fully understand your concern about the freshwater input, and the representation of the river plume (especially in the tropical W Atlantic). The ocean biogeochemistry model is coupled to an ocean general circulation model where continental/freshwater runoff (varying through the year) is included as a boundary condition (5, 6).

We would also like to stress here in this document that 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. The corresponding section will be re-written in the revised manuscript.

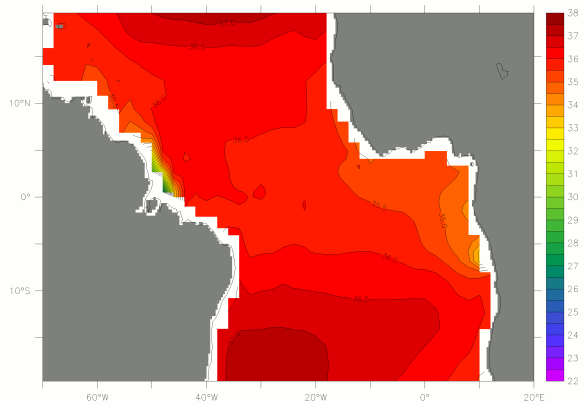
Please find here below the monthly plots (climatology, figure 3) of sea surface salinity in the tropical Atlantic, for the study area. Lowest salinity values in the Amazon river outflow region are found in May-June, according to (7). For the Congo river outflow region, our simulations show lower salinity in December and March-May, according to (8). In table 1 there are listed the modelled plume monthly extents in km² on the tropical western and eastern Atlantic Ocean. We have considered "plume waters" when the maximum sea surface salinity value is below 35. Please note that for the eastern tropical Atlantic, the low salinity areas adjacent to the Niger and Volta rivers were also computed.



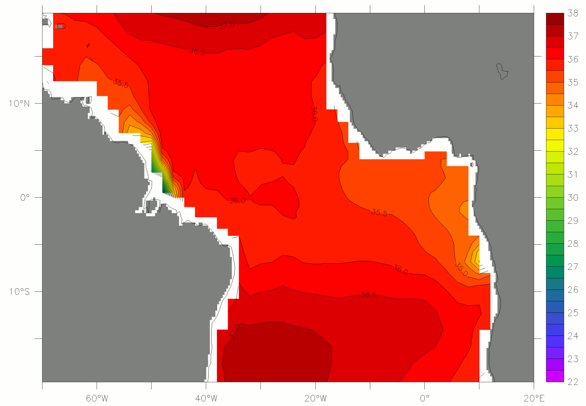
Sea surface salinity - JANUARY



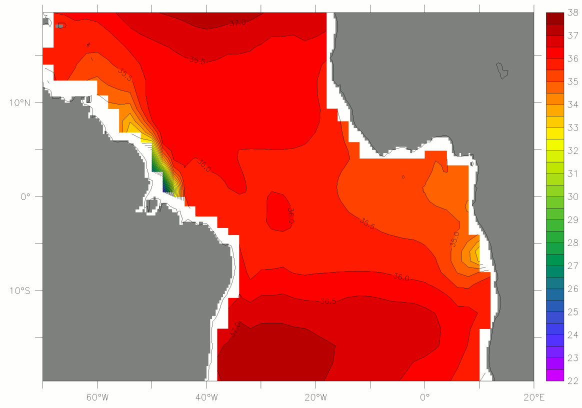
Sea surface salinity - FEBRUARY



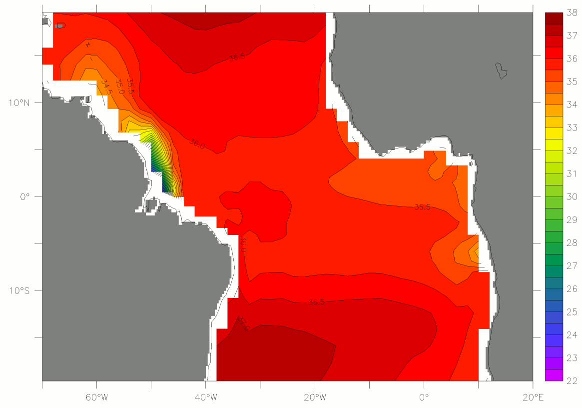
Sea surface salinity - MARCH



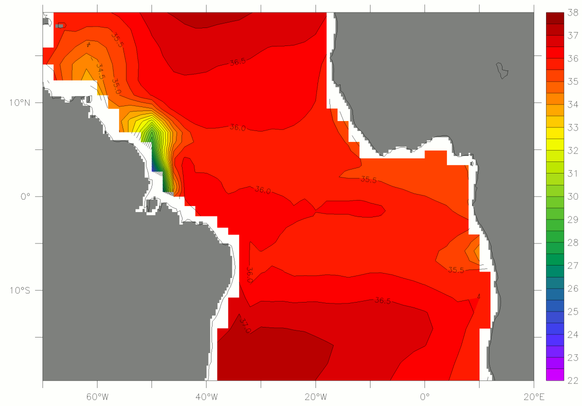
Sea surface salinity - APRIL



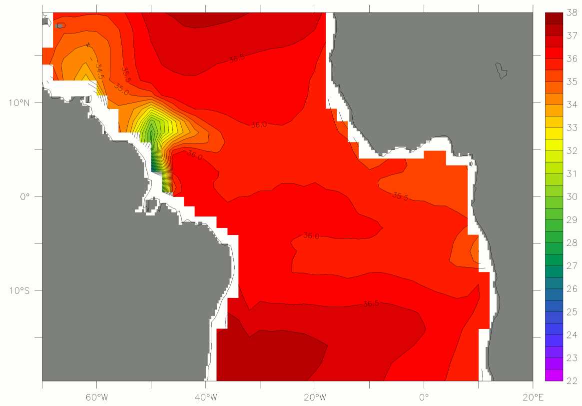
Sea surface salinity - MAY



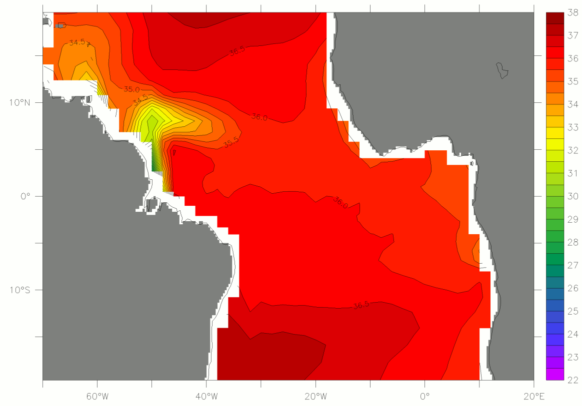
Sea surface salinity - JUNE



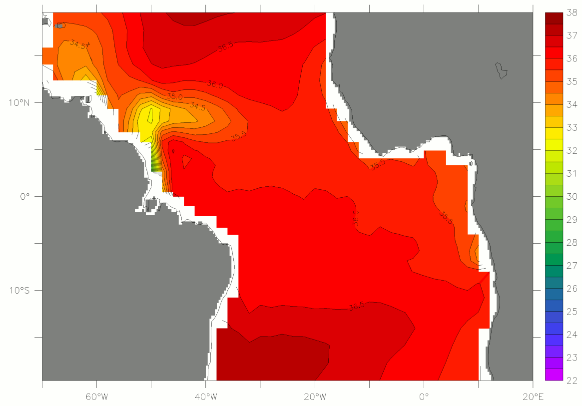
Sea surface salinity - JULY



Sea surface salinity - AUGUST



Sea surface salinity - SEPTEMBER



Sea surface salinity - OCTOBER

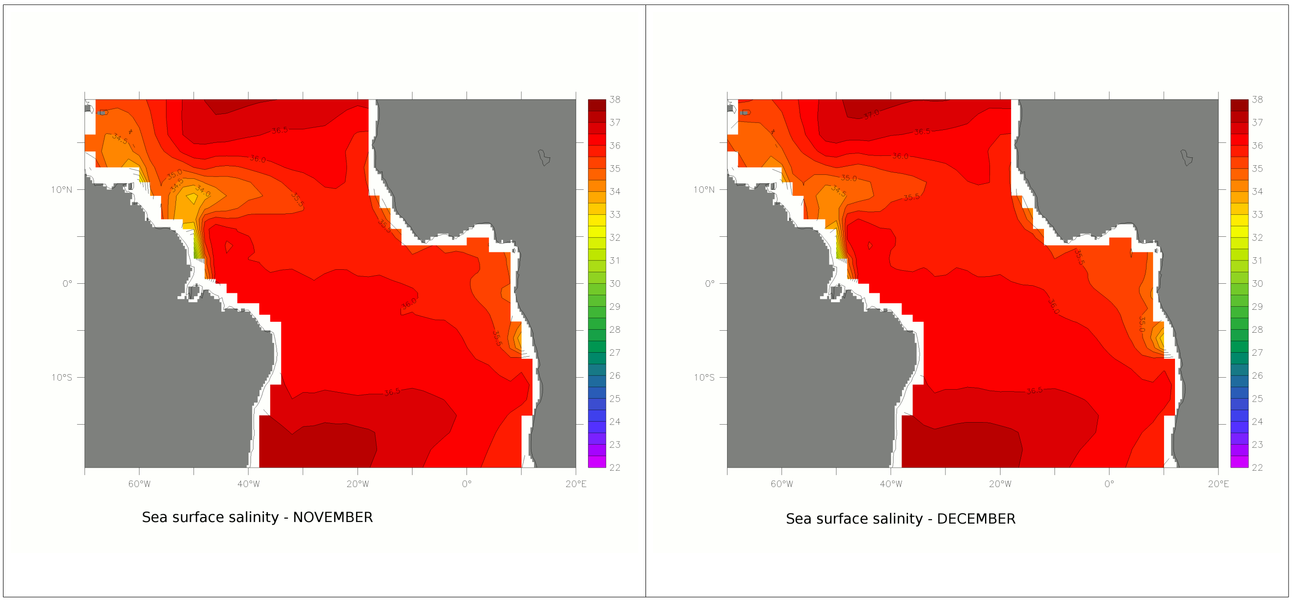


Figure 3 – Sea surface salinity climatology for all PISCES-T model runs used in the first version of the BGD manuscript.

Table 1 – Modelled plume extent in km² for the western and eastern tropical Atlantic Ocean. We computed "plume waters" the modelled areas where the sea surface salinity was below 35.

	W tropical Atlantic Ocean (lat: -20º:20º, long: -70º:-40º)	E tropical Atlantic Ocean (lat: -20º:20º, long: -20º:20º)
January	5.3 10^5	8.9 10^5
February	2.5 10^5	9.5 10^5
March	3.4 10^5	8.0 10^5
April	5.1 10^5	1.0 10^6
May	7.9 10^5	9.1 10^5
June	1.1 10^6	5.7 10^5
July	1.5 10^6	2.1 10^5
August	1.8 10^6	1.4 10^5
September	2.3 10^6	1.7 10^5
October	2.1 10^6	2.8 10^5
November	2.1 10^6	4.5 10^5
December	1.5 10^6	5.9 10^5

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

We are sorry for that, 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.*

Yes, we concede the modelled downward irradiance corresponds that of ocean waters (we used two extinction coefficients).

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?

The MAE allows us to assess the difference between the modelled and available data, i.e. model skills in representing available data. Another reason that made us decide re-running the simulations using an updated version (PlankTOM) of the ocean biogeochemistry model was the problem with the MAE values, also pointed out by referee #2. In (9), the version 5.2 of PlankTOM was used in a comparison exercise using HOTS and BATS data, and stayed among the best skilled biogeochemical ocean general circulation models. Unfortunately there aren't many time-series in the tropical Atlantic Ocean. If already available, we will try and use nutrient and dissolved oxygen data from the Cape Verde Ocean Observatory (CVOO) in the revised manuscript. However, one should keep in mind that the CVOO is, on purpose, located in an area without any riverine continental influence. Please find below in table 2 the preliminary calculated MAE for the new model runs.

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 = (10)

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

Figure 3 and table 1 previously shown in this document show that the the model is able to reproduce the low salinity plume. Again we would like to stress that it was not the objective of this manuscript to assess river plume biogeochemical processes.

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).

The model computes export production as the amount of particulate organic matter that is exported below the euphotic zone (150 m). For the full description of how organic matter sinking is parametrised, please refer to the PlankTOM guide (11), available at http://lgmacweb.env.uea.ac.uk/green_ocean/model/PlankTOM10_equations_Feb2012.pdf

In the manuscript we have used the biological pump efficiency, an approach suggested by (12), that uses the concentration of residual nutrients (we chose nitrate) in surface waters and the concentration of nutrients in deeper waters. I am looking forward to finish analysing the new run results with the explicit representation of N2-fixers in order to see how the "biological pump efficiency" will behave.

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

Thank you for the suggestions, and we will include them in the manuscript discussion. We have cited (13) in the first manuscript version, which uses already data from a moored buoy in the eastern tropical Atlantic.

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

As stated before in this document and in the first manuscript version, the freshwater input to the ocean was kept – the model simulations have only stopped the river nutrient and carbon inputs. Stopping the freshwater input would have unpredictable consequences to the results and would affect the salinity (and the plumes) and heat budgets.

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?

Sorry, but the question was not very clear: do you mean the case of having a salinity minimum in the western Atlantic? The model simulates an undersaturation of CO2 in low salinity water as a physical effect, and the model results match the observations from (14).

Here is a quote from (3) 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 behaviour in estuaries. These values are in agreement with recent modelled 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.

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