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Discussions

Interactive comment on "Riverine influence on the tropical Atlantic Ocean biogeochemistry" *by* L. Cotrim da Cunha and E. T. Buitenhuis

Anonymous Referee #1

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Cotrim and Buitenhuis use the PISCES-T biogeochemistry model to investigate the influence of the three largest rivers on the biogeochemistry of the Atlantic Ocean. This is a very important question, one that we need to understand both in the context of effect of climate and anthropogenic activity on input of terrigenous material into the oceans. The influence of the Amazon, Congo, and Orinoco, being the three largest rivers are especially important and so a study of this kind is timely.

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 fun-

damentally 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).

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.

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 photomin-

eralization 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.

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

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.

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?

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

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).

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.

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

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?

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