Review of Loginova et al for Biogeosciences (bg-2015-181)

Loginova *et al.* present a paper about the effects of nutrient additions (N and P) on chromophoric and fluorescent dissolved organic matter (CDOM and FDOM) in mesocosms filled with waters from the Eastern Tropical North Atlantic Ocean. I consider that the issue of this paper is interesting and suitable for Biogeosciences readers, however some changes (mostly reduction of the paper and a clearer focus) could improve significantly the readability of this manuscript.

The authors manipulated N and P to simulate a wide N:P range, including Redfield ratio, and tested how these changes affect the DOM optical properties (spectral slopes) and production of chromophores (measured at 325 nm) and 3 fluorophores (1 humic and two aminoacids like components) by phytoplankton and bacteria. Then, they compare their results with the previously reported relationships between a_{375} and the 320-500 nm spectral slope after Stedmon and Markager (2001) and between the a_{355}/DOC ratio and the 275-295 spectral slope after Fichot and Benner (2012). I think this manuscript needs a moderate revision and more focused goals.

General comments:
I think the authors should focus the goals of the manuscript better. Is the goal to test the nutrient influence on CDOM optical properties through stimulation of phytoplankton and/or bacterioplankton? Or by contrast is the goal to compare different models (relationships) with different optical parameters with the mesocosms data?

I think that the setup of the mesocosms etc was designed to test specifically the nutrient effects on DOM optical properties. Therefore, I think the comparisons with other models seems to be secondary and I have doubts about if their inclusion in this manuscript have any sense or just makes the paper wordy. For instance, I cannot see the relevance for the comparison with the relationship between a375 and the 320-500 nm spectral slope proposed by Stedmon and Markager (2001) obtained for the Greenland Sea. It is hard to see the usefulness of this comparison that makes the paper longer unnecessary. The comparison, any case, it should be in a natural nutrient gradient in the oceanic waters but not in a particular sea without any reference to mineral nutrients. That is, they can obtain more data from literature covering a wide gradient of nutrients or the authors should just reconsider to include this part of the manuscript. More or less the same comment for the comparison with the Fichot and Benner (2012) ´s model. This model was proposed to related terrigenous DOM with the spectral slope from 275 to 295nm for its use as terrestrial tracer, but not with mineral nutrients, then what is the point of that (see more comments below).

Specific comments:
Introduction
- Page 6 (line 138). Please introduce the meaning of OMZ the first time you use these acronyms

Materials and Methods
-Page 8 (lines 163-178). This paragraph includes too many details and I think could be shortened.
-Page 10 (line 229). The CDOM and FDOM samples were stored at 4ºC during 6 months. That is a lot of time storage!!!. Despite the low temperature of conservation and that the 0.45µm filtration will prevent some bacterial growth. It is well known that there are bacteria crossing this filter pore size and, of course, bacteria growth at 4ºC particularly under nutrient enrichments. I have my reservations about the time since the samples were collected and analyzed. I recommend including a note on that issue or any kind of control about potential errors.
-Page 11 (line 271-272). In the mesocosms, authors have calculated the absorption coefficients at 325 nm (line 267) because is the most common wavelength in the literature. Then, they also calculated coefficients at 355 nm and at 375 nm only for comparative reasons. The information provides by the spectral slopes encompasses the changes among wavelengths within a band. I think the coefficients at 355nm and 375 nm are redundant and I have many concerns about the relevance of the comparisons with the models of this paper (please see the previous comments) that is the

ultimate reason for these calculations. I suggest deleting the comparisons and these two absorption coefficients. The paper will be better focused.
-Page 11 (line 279-285). Again, It has no sense for me two calculate three spectral slopes; $S_{275\text{-}295}$; $S_{350\text{-}400}$; $S_{320\text{-}500}$ (S_{SEMO}). Helms et al. (2008) showed that the wavelength band more sensitive to changes is from 275 to 295. Therefore, the calculation of S_{SEMO} is redundant and less precise that $S_{275\text{-}295}$. I suggest to delete these calculations to simplify the paper without losing information.
-Page 12 (lines 308-309). Delete this last sentence of the paragraph.
-Page 13 (line 324). Delete "(see Table 1, Fig. 1,2)".
-Page 13 (line 329). Delete "(see Fig. 3,4,5)".

Results
-Page 14 (line 363). Change "abundance" for "concentration"
-Figure 3- I suggest to delete this figure and the associated results
- Figure 5- I suggest to delete the figure e. Even although the molar absorption coefficient at 355 nm (a355/DOC) could be considered as a surrogate of terrigenous DOM (dissolved lignin), the parameter determined in the Fichot and Benner (2012) in river-influenced oceanic waters, I can not see the connection between the influence of mineral nutrients (N and P) using waters from the Eastern Tropical North Atlantic with this molar absorption coefficient at 355 nm and the spectral slopes $S_{275\text{-}295}$ in the mesocosms. Sorry, but I cannot see the meaning of this figure.
-Table 2- Units of the spectral slopes are wrong just nm^{-1} not $d^{-1}nm^{-1}$
-Page 18 (line 489). Change "In order to access" for " to assess"

Discussion
-Page 21 (lines 534-548). This first paragraph seems an introduction. Please delete from line 546 to 548, these are the goals that should appear at the end of the introduction section.
In general, discussion section needs to be polished and I missed references to key papers on this topic. It needs more focus and structure.
For instance, some missing (not all) references.

Biers et al. 2007. The role of nitrogen in chromophoric and fluorescent dissolved organic matter formation. Mar. Chem. 103: 46–60.
Kramer & Herndl. 2004. Photo- and bioreactivity of chromophoric dissolved organic matter produced by marine bacterioplankton. Aquat. Microb. Ecol. 36: 239–246.
Ortega-Retuerta, E., et al. 2009. Biogeneration of chromophoric disolved organic matter by bacteria and krill in the Southern Ocean. Limnol. Oceanogr. 54:1941–1950.
Romera-Castillo et al. 2011. Net Production and Consumption of Fluorescent Colored Dissolved Organic Matter by Natural Bacterial Assemblages Growing on Marine Phytoplankton Exudates. AEM doi:10.1128/AEM.00200-11