Biogeosciences Discuss., 9, C7017–C7019, 2013
www.biogeosciences-discuss.net/9/C7017/2013/

Biogeosciences
Discussions

Interactive comment on "UV/PAR radiations and DOM properties in surface coastal waters of the Canadian shelf of the Beaufort Sea during summer 2009" *by* J. Para et al.

Anonymous Referee #2

Received and published: 3 January 2013

General comments: The manuscript presents the UV/PAR radiations and some DOM properties for the surface samples of a 27-stations cruise at the Beaufort Sea during summer 2009. As the authors state on the manuscript, this area has special interest as it is an important source of organic material to the oceanic circulation, and furthermore, it is mostly sure that this contribution will increase due to climate change. In general, the focus of the manuscript it is appealing, but as the reviewer #1 stated, data set is very limited, data analysis is very superficial and the conclusions are not clear and deficient in new contributions. Also, there are some inconsistencies in the methodology used that I am explaining below. The article should be rewritten, authors should avoid trivial information like some of the figures and tables, and they should do a better data

analysis, for example studying the correlations of the residuals for some of the variables with the salinity. In addition, some of the paragraphs of the manuscript are not easy to understand, specially the conclusions which also should not include references.

Specific comments: Material and methods: Page 15571, line 1: I don't understand the meaning of ". . . and radiation in the Easter sector contamination.". Do the authors use one Polycap AS75 cartridge per sample? They should also add how they cleaned it, even more if they reuse the filter for several samples. Why they use a different filtration/filters for DOC and CDOM/FDOM? The term SZA is not defined. Chlorophyll a concentration is set to a nominal value of 0.1 ug per L, nevertheless, elsewhere (Fig 3, for example) a value of about 7 ug per L is associated with stn 170. Authors should explain these differences. Authors should explain the calibration of the TOC analyser. Regarding the CDOM, I miss the data from 250-350nm, that is an important part of the absorbance spectra, which also would permit to calculate ratios and compare them with previous articles. FDOM should be measured within the first 24 hours after sampling. After that time the sample loses the entire protein-like signature (very labile material). The measurements made by the authors, 3 months later, can be used to study the humics acids dynamics, but not protein-like compounds, i.e. peak T or component C3 in this study. Authors have to state this fact on the manuscript. In addition, I am not sure that 54 EEMs are enough to do the PARAFAC, authors should prove if the results of this methodology are significant with this number of samples. Results and discussion: DOM characteristics: Authors observed how DOC and aCDOM correlates with S (also observed previous works), they should calculate the residuals of these correlations and compare them to study the processes involving DOC and CDOM independently of the salt gradient, this new correlation would conduct to new insight. Page 15579, line 13: It is not clear to me how you relate this bibliography to the previous result. This kind of inconsistencies are found through the results and discussion. Results of C3 should be revised, thinking that protein-like material would be degraded. Third plot of Figure 10 is not consistent with protein-like dynamics. In addition the fluorescence characteristics of this C3 component (Figure 8) are not similar to the ones of the protein-like substances.

I am not sure to understand the dynamics of C1 and C2 components as described in the manuscript. Looking at the results it seems that the allochthonous material (C2 component) is degraded very fast along the delta, this process generates a different kind of autochthonous compounds (C1 component) which is present in all oceanic waters of the study. The last long line of this section is not clear to me. It is also repeated in the conclusions, even with the references. Conclusions: They are not clear, they lack of new insights, and they contain references as they were part of the results and discussion. Page 15582, line 27: "in situ DOM production coupled with a limited DOM photodegradation process" this sentence is not discussed along the text, and it is stated in the conclusions. Figures: Figure 5, plot A. At least one DOC concentration (stn 118) has a different value from the one listed in Table 1. Authors should revise all the values. Figure 9. A, B, C and D labels are not corresponding with the figure caption.

Interactive comment on Biogeosciences Discuss., 9, 15567, 2012.