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Comment

## ***Interactive comment on “Fluorescence and absorption properties of chromophoric dissolved organic matter (CDOM) in coastal surface waters of the Northwestern Mediterranean Sea (Bay of Marseilles, France)” by J. Para et al.***

**Anonymous Referee #1**

Received and published: 1 September 2010

### General comments

The authors investigate the temporal dynamics of chromophoric dissolved organic matter (CDOM) in Bay of Marseilles using absorbance spectra and excitation emission matrix fluorescence (EEM) over a year period with monthly sampling. Temporal variations of absorbance spectra and EEMs were discussed with potential drivers, i.e., intrusion of river plume, autochthonous production, photodegradation, and input from deep waters. I think this manuscript will make a contribution to the literature on environmental dynamics of DOM in coastal environments. Even that said, I have concerns regarding

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Interactive Discussion

Discussion Paper



the presentation of the data. The authors analyzed the absorbance spectra and EEMs for Rhone River water samples. However, the authors did not show any riverine data in figures. I think that some discussions are weak due to lack of riverine data as the end-member. The addition of riverine data into Figs. 4, 5, 6, and 7, and related discussion into the manuscript will strengthen the authors' messages. Secondly, it should be pointed out that the number of data set in this paper is too small to say something. For example, only one point data (23 September 2010) showed the clear effect of the photodegradation in Fig. 3, and using these limited data the authors discussed too much. I think addition of some data (e.g., CDOM characteristics in deep waters) and/or some experiments (e.g., photo-irradiation experiments) need to obtain the conclusive messages. In addition, I have some comments regarding the analytical issue listed below. I believe this paper will significantly benefit from a thorough revision of the data presentation and related discussion.

#### Specific comments

Page 4, lines 16-17: Throughout the introduction section, the authors mainly introduce the environmental dynamics of DOM in coastal environments. Thus, the deep ocean circulation is not suitable in this context. I recommend introducing upwelling and/or vertical mixing instead of deep ocean circulation.

Page 5, line 12: It would be of help in the readers' understanding, if the authors can provide the value of "the averaged reported for the World Ocean"

Page 7, line 24: Please clarify the actual pathlength used this study.

Page 9, lines 9-14: Tyrosine-like fluorophore (Ex/Em =270/300) shows the peak at the same region with Raman scatter peak. If the authors normalized the fluorescence intensities of samples using Raman scatter peak of samples, normalized fluorescence intensity should be underestimated. Fluorescence intensities of samples are usually normalized by Raman scatter peak of Milli-Q water which is determined at the same day with sample analysis (e.g., Coble, 1996). In this case, I think Raman scatter peak

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do not correspond to internal standard.

Page 10, lines 11-13: DOC concentration of LCW have been reported to be  $1 \mu\text{MC}$  (<http://www.rsmas.miami.edu/groups/biogeochem/CRM.html>). How did the authors correct high DOC concentration ( $10 \mu\text{MC}$ ) of LCW, namely, system blank?

Page 11, lines 19-24 and Fig. 2: The satellite imagery at non-intrusion of low salinity water is necessary in Fig. 2 for the comparison with those at 7 May 2008 and 23 June 2008 (at the time of low salinity water intrusion).

Page 11, line 27-page 12, line 3: I could not follow how did the authors estimate the spreading time of Rhone River plume to reach Bay of Marseilles (2~3 days minimum). Please explain it.

Page 13, lines 14-16: Please add the literatures for this statement.

Page 13, line 24-Page 14, line 7: The salinity data showed the intrusion of low salinity water on 23 June 2008 (Fig. 2), and relatively high levels of CDOM were corresponding to low salinity (Table 2). These results suggest that Rhone River plume contributed the relatively high levels of CDOM. However, highest S values were also observed on 23 June 2008 (the authors introduced that terrestrial CDOM are characterized as low S value). The highest S values observed on 23 June 2008 seem to be inconsistent with low salinity and high levels of CDOM.

Page 15, lines 21-25: I have concerns regarding a strong fluorescence signal in short Ex wavelength found on 23 September 2008. I could see such signal only in EEMs on 23 September 2008 in Figs. 4 and 5. The sudden disappearance of these fluorophores, i.e., nearly 0 of fluorescence intensity, is curious for me. Xenon lamps usually show very low outputs in short Ex wavelength. Thus, I guess part of the huge difference in fluorescence in short Ex wavelength was due to artifact.

Page 18, lines 14-19: I was confused by these sentences. The authors pointed out that biologically freshly produced CDOM showed high S value, but also mentioned that low

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Interactive Discussion

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S value suggest the presence of humic CDOM from deep waters. Do this mean that terrestrial humic CDOM existed in deep waters and was exported to surface waters by vertical mixing? Or, did the authors want to describe that biologically freshly produced CDOM showed high S value, but marine humic CDOM showed low S value? Please clarify it. In addition, I think the authors should discuss this issue using additional data (for example, addition of in vitro experiments) or citing appropriate literatures.

Page 19 line 10- page 20 line 2: The authors should show the analytical errors of HIX, BIX, and M/C ratio for discussion described here. Fluorescence spectra shown in Figs. 6 and 7 seem to have relatively large noise compared to fluorescence signals. Such noise may significantly affect the values of HIX, BIX, and M/C ratio.

Technical comments

Page 5, line 10: TOC should be total organic carbon (TOC) here.

Page 13, line 16: 5 June 2008 should be 6 May 2008?

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Interactive comment on Biogeosciences Discuss., 7, 5675, 2010.

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