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Interactive comment on "Distribution, seasonality, optical characteristics, and fluxes of dissolved organic matter (DOM) in the Pearl River (Zhujiang) estuary, China" *by* Yang Li et al.

Anonymous Referee #2

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This paper deals with the seasonal variability, spatial distribution, transformation processes and fluxes of dissolved organic matter (DOM) in the Pearl River estuary (PRE) in China. DOM is investigated through dissolved organic carbon (DOC), chromophoric (CDOM) and fluorescent (FDOM) dissolved organic matter. Overall, this work provides relevant results and good quality data concerning the dynamics and fluxes of DOM in the PER. The manuscript is well structured, quite well written, and is obviously within the scope of Biogeosciences.

Therefore, I recommend the paper to be published in Biogeosciences after "moderate" revisions. Below my comments:

- Title. The part "optical characteristics" could be removed from the title.

- Although English is not bad, the manuscript could benefit from corrections of an English native speaker.

- The abstract has to be substantially improved. It does not reflect at all the relevance of the study. For instance, the following part: "The seasonality of average DOM abundance varied as follows: DOC: May (156 μmol L$-$1) > January (114 μmol L$-$1) $\sim$ August (112 μmol L$-$1) > November (86 μmol L$-$1); CDOM absorption at 330 nm: August (1.76 m$-$1) > November (1.39 m$-$1) $\sim$ January (1.30 m$-$1); FDOM expressed as the sum of the maximum fluorescence intensities of all FDOM components: November (1.77 R.U.) > August (1.54 R.U.) $\sim$ January (1.49 27 R.U.). Average DOM abundance in surface water was higher than in bottom water, their difference being marginal (0.1–10%) for DOC in all seasons and for CDOM and FDOM in November and January, and moderate (16–21%) for CDOM and FDOM in August" did not deserve to be included in the abstract.

- Introduction. Subtitles ("1.1 Overview of DOM", "1.2 The Pear River estuary", "1.3 Hypothesis and objectives") should be removed. Usually there is no subtitle in the introduction. The first part concerning DOM is OK but the second one (PRE) is too long and too detailed. Most of these details should go in the "2 Methods" part, in a "2.1 Study area" section, which currently does not exist by the way. Only information about PRE that is useful for highlighting the problematic and hypothesis is necessary in the Introduction.

- Introduction. The sentence: "The biogeochemical and optical significance of DOM depends on both its abundance and quality (i.e.chemical composition), with the latter strongly linked to its origin of formation" is not clear. Please re-phrase.

- Sample collection. I guess the number of samples collected at each season for DOM analyses is not mentioned. This should be mentioned here.

- The subtitle “2.2 Sample analysis” should be replaced by “2.2. DOM “analysis”

- DOM analyses. “The analytical uncertainty of aCDOM measurement was assessed by analyzing six pairs of duplicate samples collected from the August cruise. Average aCDOM at 330 nm (a330) was 2.19 m$-$1 (range: 1.19–4.37 m$-$1); the average difference in each pair was 0.07 $\pm$ 0.05 m$-$1, or 3.0% $\pm$ 1.4%.” This method for assessing the analytical uncertainty (precision?) is not clear to me. Why using six pairs of duplicates? I would have used six replicates (of the same sample). The values “0.07 $\pm$ 0.05 m$-$1, or 3.0% $\pm$ 1.4%” is not pertinent.

- DOM analyses. CDOM spectral slope in the range 300-500 nm (S300-500 in nm-1) is reported in the supplementary material (Table S1) but is not really discussed in the manuscript. Also, in addition to S300-500 I would recommend the determination and examination of S275-295, proposed by Helms et al. (2008) and largely used yet. It could bring significant information about CDOM molecular weight and transformation processes.

- DOM analyses. HIX, BIX and E2/E3 should be defined in this section and not in the results section.

- Results. The number of Tables is quite high. I recommend adding some in the supplementary material: Tables 1, 2, 4, 5.

- Results. Besides salinity, are ancillary parameters available for this sampling (i.e., dissolved oxygen, nutrients, chlorophyll,...) that could help the help the interpretation of the DOM dynamics?

- Results. I find there is a lack of use of statistical analyses. For example, ANOVA, t test, Mann Whithney test,... (depending on the normal distribution or not of samples) could be applied to determine statistical differences in the DOM concentrations between seasons, surface/bottom,.... - Moreover, instead of separate a priori the samples by seasons and looking at differences between these seasons (that do not necessarily

represent/reflect different hydrological or meteorological events which have occurred during the sampling period), it could be also interesting to apply multi-way statistical methods (principal component analysis, hierarchical ascendant classification,...) on all samples regardless of their sampling period. This could lead to different clustering of samples and underline particular processes affecting DOM dynamics, such as the impact of the mixing between marine and river waters, the impact of precipitation/runoff/river flow rate (ex: discrimination between samples collected in dry period and samples collected wet period), which could be obviously independent from seasons.

- Discussion. Lines 600-614: “[DOC] and [CDOM] in the PRE are the lowest among the major world rivers...” This is indeed intriguing. Why DOC and CDOM contents are so low in the PRE. In this part, the authors should also include the assumption of a DOM loss by bacterial degradation and photochemistry.

- Discussion. Line 604: “The lack of correspondence between [DOC]* and a330* and the freshwater discharge rate (Fig. S9) suggests that [DOM] in the PRE be controlled by both soil leaching and pollution input”. Here could be also added the hypothesis of in situ autochthonous DOM production from phytoplankton activities, which are generally not negligible in rivers.

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