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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.

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The paper entitled "Distribution, seasonality, optical characteristics, and fluxes of dissolved organic matter (DOM) in the Pearl River (Zhujiang) estuary, China" investigated seasonal and spatial variations of CDOM and FDOM characterized by absorption and fluorescence spectroscopy. Since I am an organic geochemist focusing on the organic carbon and nitrogen cycling mechanism in estuarine coastal zones and the role of microbes during the organic matter cycling, I am very familiar with the topic of this manuscript. This manuscript identified the compositional characteristics and sources of DOM. The main conclusion is that (i) microbial inputs and anthropogenic inputs are

important sources of DOM in the freshwater end; (ii) small seasonal variations with respect to DOC and CDOM; and (iii) PR exports the lowest quantality of DOC among 30 large world rivers, although the size of PR watershed ranked the thirteenth largest in the world by area. Considering the anthropogenic activities can influence the quality and quantity of DOM in aquatic ecosystems and urbanization trends continue in response to human population growth, anthropogenic influences on DOM composition will likely become more widespread. Such human effects on DOM quality could have strong impacts on carbon cycles and need to be better understood. Therefore, this study provides a typical case study to approach the scientific questions mentioned above. However, some points need to be addressed as follows. Nevertheless, this work did provide interesting findings, and the data is reasonably strong to make the conclusions, and there I suggest a moderate revision needs to perform before the acceptance of this manuscript.

General comments: 1. In terms of English, I suggest the writing should be improved further. 2. The description of "overview of DOM" is great. However, I realize that it is too general. I hope the authors could provide introduction related with their discussion or the questions that need to be solved (or knowledge gap). In addition, the transition from 1.1 to 1.2 seems not that smooth to me. 3. The chapter "1.2 The Pearl River estuary (PRE)" is too lengthy to describe the important focus and question, and some of descriptions can be moved to "Site description", otherwise part of the information seems duplicated. For instance, the authors spent 9 paragraphs to describe the PRE, and some of the information is not closely related with the results/discussions. This needs to be shortened and be questions oriented. 4. The authors mentioned precipitation is an important factor affecting soil flushing, which may affect both DOM quality and quantity. It would be great if the author could incorporate some monthly or seasonal precipitation data to support their claims. In particular, the article indicated the terrigenous DOM is the main source of investigated areas, but it did not describe the influences of land runoff and rainfall on seasonal variations of DOM. 5. In this manuscript the author suggested that the low DOC concentrations in PRE (especially the low salinity region) was affected by biological degradation (due to input of labile DOM) and low inputs due to the low forest cover. This is a good point! I suggest the author expand this description a little bit. For instance, (i) the addition of labile DOM may "prime" the degradation of terrestrial (relatively more recalcitrant) DOM; (ii) the author could specify the land use percentages of the PR watershed and compare it with the other large river-estuarine systems (such as the Amazon River). Some of the land use% data has been organized in Wagner et al. (2015), and I believe the land use% data is not that difficult to find for PR watershed; (iii) since the authors claim that the PRE is an super eutrophic system, it would be interesting at least present some nutrient data (from literatures) to further support their main findings. 6. I really like the main findings in the manuscript, but these findings are not well reflected in the abstract. I suggest the author re-organize their abstracts and focusing on the main findings. Reporting numbers are great, but there seem to be too many. Keep the important ones would be good enough. Wagner, S., Riedel, T., Niggemann, J., VaÌĹhaÌĹtalo, A. V., Dittmar, T., & JaffeÌĄ, R. (2015). Linking the molecular signature of heteroatomic dissolved organic matter to watershed characteristics in world rivers. Environmental science & technology, 49(23), 13798-13806. 7. Considering the author spent a huge effort collecting all these samples, it would be very interesting to perform some statistical analysis such as the principal component analysis (PCA) to further confirm the major controls to the DOM variability across the whole dataset.

Specific comments: 1. There was no explanation about the inverse changes of BIX and HIX in Fig.7 2. I suggest the author make it clear what is "the saltier zone" because this is a ambiguous description. 3. Considering there are way too many tables. I suggest move some of the tables (e.g., Table 1) to the supplementary information. The DOC (μmol L-1) needs to be moved to the second column. 4. Would be wonderful if the author could point out the major metropolitan areas (or even land use patterns) in Figure 1 since it closely related with the major discussions in this manuscript. 5. When the authors describe each PARAFAC component, I suggest the author use DOM Openfluor database to compare the components in this study with literature data. Murphy, K.

R., Stedmon, C. A., Wenig, P., & Bro, R. (2014). OpenFluor–an online spectral library of auto-fluorescence by organic compounds in the environment. Analytical Methods, 6(3), 658-661. 7. R.U. should be defined in the abstract.

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