

Interactive comment on “Sources, fluxes, and behaviors of fluorescent dissolved organic matter (FDOM) in an estuarine mixing zone: Results from the Nakdong-River Estuary, Korea” by Shin-Ah Lee and Guebuem Kim

Anonymous Referee #1

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Major comments

In this manuscript, the authors determined variations of dissolved organic carbon (DOC), stable carbon isotope of DOC ($\delta^{13}\text{C}$ -DOC), and fluorescent DOM (FDOM) with tide from monthly observation at a 24 hours monitoring station in the Nakdong-River Estuary, Korea. The authors found significant relationships between salinity and DOC as well as FDOM (both of humic-like and protein-like) throughout a year, implying that riverine DOM were conservatively distributed with estuarine mixing but contribution of autochthonous DOM were minor in the estuarine mixing zone. The conservative be-

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havior of terrestrial DOM was also supported by the spatial variation of $\delta^{13}\text{C}$ -DOC at the estuary. The authors also found that the relationships (slopes and intercepts of linear regressions) between salinity and DOC as well as FDOM differed among months, due to different levels of their riverine end-members. From the findings, the authors pointed out that the estimation of annual riverine flux of DOC (and also FDOM) requires careful considerations of seasonal changes.

I think the methods in the manuscript are technically sound, and manuscript is logically well written. Even that said, I have two major comments on the manuscript.

(1) I thought that data analyses and discussion were insufficient. In the Introduction section, the authors described that the behavior of DOM in the estuarine mixing zone and fluxes of DOM from the river can be determined in the manuscript. However, the authors basically focused differences in fluxes of DOM from the river among months but did not discuss about different behavior of DOM in the estuarine mixing zone. It seemed that correlation coefficients (or coefficients of determination) between salinity and DOM parameters were largely different among months. Such different correlation coefficients imply that the behavior of DOM in the estuarine mixing zone, e.g., photo-degradation, sedimental inputs, autochthonous production, were different among months. I think it's better to add some discussions about different behavior of DOC and FDOM in the estuarine mixing zone among months based on different correlation coefficients.

(2) The authors evaluated monthly “variable” concentrations (i.e., 174–284 μM) of riverine end-member of DOC from relationships between DOC concentration and salinity. On the other hand, the authors also obtained one “fixed” riverine end-member with 270 μM from the conservative mixing curve of $\delta^{13}\text{C}$ -DOC with the two end-members. These interpretations derived from different analyses seemed to be contradicted. I think the authors should also analyze conservative mixing curve of $\delta^{13}\text{C}$ -DOC for each month.

Specific comments

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Line 32, line 153 and lines 185-186: I think correlation coefficients of relationships between salinity and DOM parameters should be minus value. For example, in line 32, “ $r=0.55-0.99$ ” should be minus 0.55 to minus 0.99? Otherwise, these values were r^2 ?

Line 44-45: Please add reference(s) to a description “As FDOM accounts for 20 - 70

Line 80: Is 23,380 km² area of watershed? Please clarify it.

Lines 89-91: The system of auto-sampler is not clear from the description. Please add some information for the auto-sampler (structure, model number, manufacture etc) or cite previous work(s).

Lines 94-96: How did the authors preserve DOC and $\delta^{13}\text{C}$ -DOC samples? Freezing? Please make it clearer.

Lines 101-129: The authors determined DOC concentrations using two different instruments, i.e., TOC-VCPH and TOC-IR-MS with NDIR. Which DOC concentration was used for the manuscript? Please make it clearer. In addition, for some readers who are interested in use of TOC-IR-MS, it would be great if the authors can add some information regarding with comparison of DOC concentrations determined by two different instruments.

Lines 131-147: Since DOC concentrations were relatively high, in particular in the low salinity waters, inner filter correction may be necessary to obtain precise EEMs. Did the authors apply an inner filter correction to EEMs? For the inner filter correction, please see Miller et al. (2010) *Aquat Sci* 72, 269-275 and references therein.

Lines 137-140: Please add validation method(s) of PARAFAC. I think that the authors should show spectra or describe peak positions of two PARAFAC components. In addition, previous EEMs-PARAFAC studies with DOMFluoro toolbox generally identified more than 2 fluorescence components. Did the authors have any idea why only two-component model was validated with dataset of this manuscript?

Lines 145-147: How did the authors measure water temperature? Please add the

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method in the Materials and Methods section.

Lines 158-160: I could not understand how did the authors exclude high salinity periods. In other words, what was the definition of the high salinity periods? Please explain it in detail.

Lines 202-205: The authors only discussed the case of October and November here, even though FDOMH slopes were higher for July, August, October, and November compared with other months. How about July and August? Please discuss it.

Line 204: The term of “organic weathering” is usually used for the breakdown of rocks by plant or animal action, e.g., extension of roots. Thus, I think “degradation products of soil organic matter” (for example) is much better than “organic weathering products”.

Lines 212-214: Mayer et al. (1999) and Zhang et al. (2009) did not determine the spring and fall phytoplankton blooms in the Nakdong-River. Please cite more proper studies here.

Line 257: “positive” should be “negative”? Please check it!

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