

Interactive comment on “Characterizing the origin of excess dissolved organic carbon in coastal seawater using stable carbon isotope and light absorption characteristics” by Heejun Han et al.

Anonymous Referee #2

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Han et al provide a short summary of DOM properties in Sihwa Lake, a constructed coastal lake in a heavily industrialized coastal area, over 2 sampling trips taken in spring 2017 and in late summer 2018. Same sites were visited in each sampling. Using a combination of nutrients and optical and stable isotope tracers, they aim to distinguish multiple sources of DOM (though the sources are not clearly identified). The brevity of this manuscript makes it very difficult to follow. Many details are lacking and some deeper analysis is required to support the conclusions made in this study. Several conclusive statements are made without a clear logical argument to help the reader reach the same conclusion. These problems occur throughout this version of the manuscript, and, along with some substantial editing for grammar and usage, re-

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quire more than substantial revision. Specific comments L55: Finish the set up for this manuscript. What are the sources expected? It is curious why the authors didn't try to use endmember mixing analysis (EMMA) to disentangle the sources. The primary sources appear to be: terrestrial, marine, phytoplankton, and “anaerobic benthic processes” which I shorten to benthic. Methods L68: It appears the sluice gates are mostly closed; what does periodic openings entail? Were the gates opened prior to sampling? L70 What vessel was used for sampling? “a ship” is nebulous. L78 How many mL of 6 M HCl were used and what was the final pH? L88 Unlikely that the precision of the TOC analyzer for DSR measurement is 2.2 μM , round to 2 μM . How many analyses? L92: To my knowledge no consensus value of DSR is reported, though similar values have been reported as described here. Reword to indicate this (as was done in earlier work referenced here). If a consensus value is now published, please cite the publication. Also please report number of analyses (N) for these standards. Results Make the colorbar ranges for Figs 2 and 3 the same for each panel for ease of comparison. The PARAFAC results should be tested against the OpenFluor database. Here, spectral components are compared to Coble 2007 wherein peaks are visually identified. Surprisingly, the authors only describe 2 of the 4 peaks they find with the model. I recommend they discuss the dynamics of the protein-like component. Given the results presented, it would be informative to see how well this component correlates to other PARAFAC components and in cross section across the lake (ie, as in Figs 2 and 3). Also, correlation of this peak with $\delta^{13}\text{C}$ values. L140 Range of values does not capture the most negative value reported (-27.8‰ Discussion L155: How is “significant excess” being defined? It is unclear what the authors mean by this phrase and how they quantified it. L158: What does land-seawater interaction mean? Mixing? Proportional mixing would not add an excess of DOC; an excess implies production in spite of mixing... unless a 3rd source is implied. In this case, binary mixing analysis won't work. Perhaps the authors should suggest here the benthos as a potential source; but that source also should be parameterized (eg, what is its $\delta^{13}\text{C}$ -DOC value, FDOMH, FDOMM values, SR, etc.) L180: The groupings appear arbitrary; what crite-

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ria were used to separate them? I don't understand how the terrestrial source of DOM can be not fluorescent, given that the authors identify humic fluorescence as a specific marker. This section of the discussion is extremely hard to follow. L197: No evidence is provided for photochemical or bacterial degradation in this study. L201: As suggested earlier, the possibility to use EMMA or other multivariate means with these data are encouraging. I recommend the authors try to analyze their results with an aim of using exploratory methods (eg ordination such as PCA or non-parametric techniques) and perhaps 2-way analyses wherein the difference of season (or stream flow if available; not presented) is considered. A clearer way of quantifying the Groups (1 and 2) must be presented at the very least, so that readers can follow the study. L210: No analysis was presented to demonstrate the linkage of $\delta^{13}\text{C}$ values and NH_4^+ values

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