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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A systematic examination of the relationship between CDOM and DOC for various inland waters across China" *by* Kaishan Song et al.

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Reviewer #2

GENERAL COMMENTS This manuscript investigates the relationship between CDOM and DOC in a variety of inland water systems in China. The authors found, as has been shown before, that the predictive power of CDOM vs DOC concentration regressions vary; and that variation is likely associated to other biogeochemical factors. The data set in this study is extensive and representative of numerous types of water bodies, and has great potential to help inform organic carbon transport and dynamics for continental China; however, there is a lot of room for improvement. In general, the figures and tables seem appropriate. The introduction is vague and the results and discussion

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section is poor and many important results are overlooked or addressed superficially. The comparison of regression slopes made among regions for the different types or water bodies is based on data that are not shown in any figures or tables, the regions used throughout the paper are not described in the site description. A deeper discussion of the potential mechanisms that drive CDOM chemistry and thus the DOC/CDOM correlations is strongly recommended for all the different water types investigated. The authors do a better job at proposing mechanisms for the results obtained for ice covered systems, however, that section is not entirely clear either and needs refining too. Another issue is the use of SUVA and E to separate the data, it would be really positive for the manuscript if the authors explained better the reasoning behind this kind of data sorting and compared their results to other people doing the same (as far as I could tell, this type of sorting has not been previously published). I believe there is a major flaw in the use of SUVA (see specific comments about section 3.3.1). Finally, the authors claim how important the investigation of correlations between CDOM and DOC are for feeding remote sensing models, however, they chose to quantify CDOM using the 275 nm wavelength while most empirical remote sensing methods used for inland waters are based on reflectance at wavelengths > 500 nm. A major concern is the poor grammar and lack of clarity in the text in numerous occasions throughout the manuscript, it is recommended that the authors revise this heavily to ensure no grammatical errors are present and ideas are clearly stated. Responses: the authors really thank for the insightful comments, as you may see in the revised manuscript, we addressed most of the questions you raised in the review. We deleted the section 3.3.1, i.e., the regression based on SUVA254. Also, we added a figure to deal with the relationship between DOC and aCDOM(440), which could be used for remote sensing of CDOM or DOC since that is the most reference spectral band in this community.

SPECIFIC COMMENTS (Note that grammatical errors are not being addressed/corrected)

Introduction: Lines 46-47: Statement needs clarification. Response: thanks for the comments; the authors have rephrased the sentence to achieve a clarification of the statement. Lines 55-56: Statement about mineralization needs rewording. Response: thanks for the comments; the sentence is rephrased. Line 70: “Gulf of Mexico” is the proper name. Response: thanks for the suggestion; the suggestion has been incorporated in the revised manuscript. Lines 87-92: This has already been mentioned in previous paragraphs of the introduction. Eliminate unnecessary repetition. Response: thanks for the comments; this part was removed in the revised manuscript. Line 89: CDOM and DOC are not “two forms of carbon”. DOC is a component of the Earth’s carbon pool; CDOM is a fraction of the natural organic matter pool, defined according to its optical properties and contains not only organic carbon but also nitrogen, phosphorus and sulfur. Response: Thanks for the comments, which help the authors to understand the difference and link between CDOM and DOC. However, this sentence was removed in the revised manuscript to responding to the previous comment. Lines 95-98: I do not see a difference between objectives 1 and 2. Response: Thanks for the comments; it is true that objectives 1 and 2 overlap, thus the second one is removed in the revised manuscript. Lines 105-107: this idea has been mentioned once or twice already in the introduction; it would be better if the authors were more specific about how this study could inform remote sensing data for continental China, mentioning for example the data gaps or the limitations of previous studies. Also, prediction of DOC concentration from optical properties is not only useful for feeding remote sensing models. The authors could highlight other positive outcomes of this kind of analysis, especially for fluvial systems where the remote sensing techniques are more limited. It would be useful to add a paragraph about expected results to the end of the introduction. Response: The authors really thank for the very valuable comments; several sentences about the expected results of the research is added in the revised manuscript.

Materials and methods: Line 112: is the data set for freshwater lakes the same as the one used by Zhao et al in Biogeosciences, 13, 1635–1645, 2016? If so please indicate this and clarify that these results, although corresponding to the same data set do not represent previously published work. Response: the authors thank for the concerns. The data set for freshwater lakes is collected across China, e.g., the data set covers lakes at national scale, while the data set used in Zhao et al., in Biogosciences, 13, pp 1635-1645, 2016, was sampled only in a few lakes in Northeast China (see sampling locations in Figure 1 of this manuscript, and also the study area in Zhao et al., 2016 for details). Although some of the data used in Zhao was used in this study, it is only a small of part of the data set used in this study, and also the looking angle is different from Zhao et al. (2016), where fluorescence feature from CDOM is more concerned. Lines 124-125: this statement does not belong to this section; it needs to be eliminated or moved to the end of the introduction (see comment above about expected results). Response: the authors thank for the suggestion, and this sentence is removed in the revised manuscript. Line 136: GF/F = glass fiber filters have a nominal pore size of 0.7 um. Please correct the pore size or the filter type. Response: the authors thank for the comments, it turned out that we used Whatman cellulose acetate filter with pore size of 0.45 μm, and the information was corrected in the revised manuscript. Line 151: Samples for DOC where filtered through 0.7 (or 0.45 [not clear]) um filters according to Line 136, on the other hand, samples for CDOM were filtered through 0.22 um filters. How can this difference in sample treatment affect the results? Response: the authors thank for the concern. The standard protocol for DOC determination is generally filtered through 0.7 (early study applied this pore size) or 0.45 (nowadays applied this pore size); however, samples for CDOM absorption determination is generally filtered 0.22 um filters to avoid fine particle that might have scattering effect on CDOM absorption spectra (Babin et al., 2003). We did not think too much about this issue in the previous work, we presume that the filter pore size may have but very minor effect on the relationship between DOC and CDOM. Also, in our study, all the samples were pre-process with the same methods, thus the sample treatment should affect the result in very limited manner.

Line 156: This corresponds to the Napierian absorption coefficient, please specify this in the text. Response: the authors thank for the comment, we added the information in the revised manuscript as suggested. Line 163: Zhang et al 2007 does not really explain the use of optical density over 740-750 nm as a correction factor for aCDOM, please cite a more appropriate article. Response: the authors thank for the comment, we cited the right paper (Babin et al., 2003) in the revised manuscript as suggested. Lines 171-177: Why to describe the determination of the spectral slope if it is not presented in the results? This section is unnecessary. Response: the authors really thank for the comments; this section was removed in the revised manuscript.

A better description of the sampling locations or regions is needed, perhaps a table with detailed information about all water bodies sampled (location, dates, number of samples at each site, classification in this paper) in the supplementary material. Also, clarification is needed about how urban waters were classified as so; in other words, what parameter(s) was (were) used to define water bodies as urban? Response: the authors thank for the valuable comments; a supplementary table was produced in the revised manuscript, and the information mentioned in the comments were provided in the supplementary table, please check the table in the supplementary material. Here, we would like to point out that the information for rivers or stream was not listed in the supplementary table due to the multi sampling points were collected along rivers or stream, thus the reader can reference the relative position through Figure 1. The definition of urban waters were added in the main text, please check it out in the revised manuscript.

Results and discussion: Be consistent with the use of units, for example: do not mix ug/L with ug L-1. Also, there should be a space between a number and the unit in all cases, i.e., 10 mg not 10mg Streams and rivers are also freshwater systems (unless they are estuarine systems). Response: thanks for the comments; the authors have checked these issues throughout the manuscript, and corrections were made correspondingly. Thanks again for the suggestions. It is confusing to use "freshwaters" to refer to freshwater lakes, please be specific if you are referring to lakes or streams/rivers, this applies to Figure 1 as well. Response: thanks for the comments; your recommendation was adopted in the revised manuscript, the authors have also checked out the whole manuscript to clarify these confusions. Line 194: this statement needs rewording. It is unintelligible. Response: thanks for the suggestion; the authors rephrased this sentence in the revised manuscript to make it clear. Line 207: “inverse trend were” is not the appropriate wording, the authors are not describing a trend. A more suitable wording would be: “the opposite was found”, or something along those lines. Response: the authors thank for the comment, and the suggestion was adopted in the revised manuscript. Line 228: a more appropriate title would be “freshwater lakes and reservoirs” Response: the authors really thank for the suggestion, and the title was modified as suggested. Line 234-248: it is unclear how the authors make conclusions about DOC biogeochemistry in different regions of China (i.e., North China and East China; Northeast China, etc.) based on a regression analysis of the data set from different regions. Where are these results presented? Where are these regions? Response: the authors really thank for the comments, a table with these related information was added in the revised manuscript, please see the details in the revised manuscript. Lines 256-263: How were these slope values obtained? It would be useful to see the regressions for each of the regions that are mentioned in this section, and the slope values should be tabulated. Also each of the regions the authors are referring to should be shown either in Figure 1 or in a separate figure to clearly show where the regions are. This is related to the previous comment. Response: the authors really thank for the comments, a table with these related information was added in the revised manuscript, please see the details in the revised manuscript. Lines 266-267: This last statement is vague and gives the idea that the authors have also collected and analyzed remote sensing data. Please reword and focus. Response: the authors really thank for the suggestion, and the title was modified as suggested. Lines 247-280: See comment about lines 256-263. Response: thanks for the valuable comments, as mentioned in the previous responses to the comments, a table with these information was added in the revised manuscript, please see the details in the added table. Line 286: there are many other publications that are more appropriate citations for this statement than Jaffe et al 2008. For example: Williams et al 2010 L&O; Grae-

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ber et al 2012; etc. Also it would be interesting to examine in more detail how the variation in slopes compare to other results such as the ones published by Helms et al. and Spencer et al. and provide a more mechanistic explanation for this change in slope. Response: the authors really thank for the valuable comments, and more appropriate citations were added in the main text, also more detailed comparisons were added in the revised manuscript. Line 294: provide citations for this statement. Response: thanks for the comments; citations were added in the revised manuscript. Line 298-301: again, this conclusion is very vague, how exactly may urbanization affect the chemistry of dissolved organic matter in order to result in poor associations between DOC and CDOM? Is there literature showing similar results? What are the potential mechanisms? Response: thanks for the concern; additional statements and literature were provided in the revised manuscript. Line 315: What does the number in parenthesis mean? Response: thanks for the concern; the number in the parenthesis is chlorophyll-a concentration, the information was provided in the revised manuscript. Line 322: Müller et al 2011 is not listed in the reference list. Response: thanks for pointing out the error, this reference is replace by Zhang et al., 2010, and also added into the reference list in the revised manuscript. Line 324: Stedmon et al 2009 is not listed in the reference list. Response: thanks for the comment, and alternative cited literature was added in the revised manuscript. Lines 337-338: This statement is vague and unclear. Response: the authors thank for the comment, this statement was rephrased in the revised manuscript.

Lines 340-341: This is unclear, how can SUVA values "reflect" the regression slope for Lines 341-345: The differences in SUVA suggested by the authors are not as clear as they indicate. A lot of overlapping exists in SUVA values across water types, it might be convenient to conduct an analysis of variance to determine significant differences across groups. Response: the authors really thank for the valuable comment, this paragraph was removed, please see the revised manuscript for the details of revision.

Section 3.3.1: As far I can tell, this kind of data sorting is a redundant exercise and it is obvious that better correlations than those obtained with the pooled data would be obtained if SUVA254 is used to sort the data. Most likely a254 and a275 are strongly correlated, thus, SUVA254 is pretty much the equivalent of the ratio of a275 to DOC which is what defines the slope of the a275 vs DOC regression, mathematically. So if SUVA254 is used to sort the data, you are practically grouping the samples that distribute more closely along a slope value. This can be easily seen in the slope values of each of the regressions of the sorted data in Figures 4a-h: the slope increases systematically as the SUVA254 range increases. I show this graphically in the attached Figure 1. I used the middle point of the different SUVA bins created by the authors (SUVA<1.0, 1.0<SUVA<2.0, 2.0<SUVA< 3.0, 3.0< SUVA< 4.0, 4.0<SUVA<5.0, 5.0<SUVA<6.0, 6.0<SUVA< 8.0, and 8.0< SUVA< 13.1), that is: 0.5, 1.5, 2.5, 3.5, 4.5, 5.5, 7, and 10.75, as a rough representation of the average SUVA254 value for each bin (y axis) and plotted it against the regression slope of each of the binned data sets (Fig 4a-h of manuscript). A clear linear correlation exemplifies the redundancy of using SUVA254 to sort the data. I strongly discourage this approach as a means to improve the correlations between CDOM and DOC. Response: the authors really thank for the comments; this paragraph was removed in the revised manuscript. Line 371: This heading should be 3.3.2 Response: thanks for the comments; series number for this subheading is corrected. Section 3.3.2 (incorrectly named 3.3.1): see general comments. Response: the authors thank for the comments, correction was made in the revised manuscript. FIGURE 4a: "greater than" symbol is incorrect, according to Line 539 it should be SUVA < 1. Correct also in the figure caption. Response: thanks for the comments.

REFERENCES It is suggested to read and incorporate the work by Brezonik et al al 2015 http://dx.doi.org/10.1016/j.rse.2014.04.033 Response: thanks for the comments; the suggested literature is incorporated in the revised manuscript, and has been added in the reference list.

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