

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

**Anonymous Referee #2**

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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 section is poor and many important results are overlooked or addressed superficially. The comparison of

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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.

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

Introduction:

Lines 46-47: Statement needs clarification.

Lines 55-56: Statement about mineralization needs rewording.

Line 70: “Gulf of Mexico” is the proper name.

Lines 87-92: This has already been mentioned in previous paragraphs of the introduc-

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tion. Eliminate unnecessary repetition.

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.

Lines 95-98: I do not see a difference between objectives 1 and 2.

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.

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.

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).

Line 136: GF/F = glass fiber filters have a nominal pore size of 0.7  $\mu\text{m}$ . Please correct the pore size or the filter type.

Line 151: Samples for DOC were filtered through 0.7 (or 0.45 [not clear])  $\mu\text{m}$  filters according to Line 136, on the other hand, samples for CDOM were filtered through 0.22  $\mu\text{m}$  filters. How can this difference in sample treatment affect the results?

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Line 156: This corresponds to the Napierian absorption coefficient, please specify this in the text.

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.

Lines 171-177: Why to describe the determination of the spectral slope if it is not presented in the results? This section is unnecessary.

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?

Results and discussion:

Be consistent with the use of units, for example: do not mix  $\mu\text{g/L}$  with  $\mu\text{g L}^{-1}$ . 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). 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.

Line 194: this statement needs rewording. It is unintelligible.

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.

Line 228: a more appropriate title would be “freshwater lakes and reservoirs”

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

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these results presented? Where are these regions?

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

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.

Lines 247-280: See comment about lines 256-263.

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; Graeber 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.

Line 294: provide citations for this statement.

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?

Line 315: What does the number in parenthesis mean?

Line 322: Müller et al 2011 is not listed in the reference list.

Line 324: Stedmon et al 2009 is not listed in the reference list.

Lines 337-338: This statement is vague and unclear.

Lines 340-341: This is unclear, how can SUVA values "reflect" the regression slope for

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DOC/CDOM.

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.

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 SUVA<sub>254</sub> is used to sort the data. Most likely  $a_{254}$  and  $a_{275}$  are strongly correlated, thus, SUVA<sub>254</sub> is pretty much the equivalent of the ratio of  $a_{275}$  to DOC which is what defines the slope of the  $a_{275}$  vs DOC regression, mathematically. So if SUVA<sub>254</sub> 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 SUVA<sub>254</sub> 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 SUVA<sub>254</sub> 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 SUVA<sub>254</sub> to sort the data. I strongly discourage this approach as a means to improve the correlations between CDOM and DOC.

Line 371: This heading should be 3.3.2

Section 3.3.2 (incorrectly named 3.3.1): see general comments.

FIGURE 4a: "greater than" symbol is incorrect, according to Line 539 it should be SUVA < 1. Correct also in the figure caption.

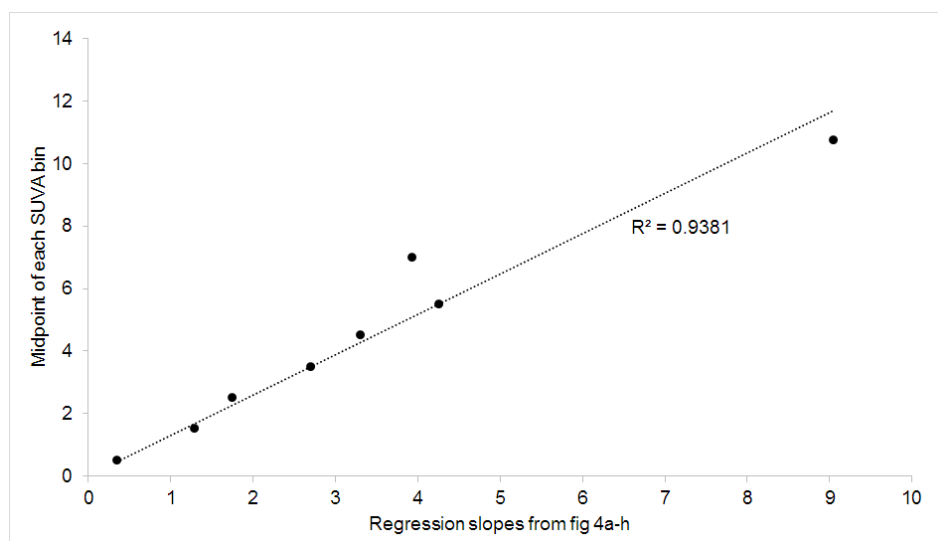
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## REFERENCES

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>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-380, 2016.

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**Fig. 1.** Middle point of SUVA bin vs regression slopes in sorted data.

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