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Discussions

Interactive comment on "A systematic examination of the relationships between CDOM and DOC in inland waters in China" *by* Kaishan Song et al.

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Anonymous Referee #1 Received and published: 1 May 2017

This paper presents a series of regression equations between DOC concentrations and optical properties of the DOM across a range of water bodies in China. The authors have amassed an impressive data set, and applying this data set to questions of DOC biogeochemistry could make a useful contribution. Unfortunately the paper, as

currently written, has some ïňĆaws that limit its value.

Responses: The authors thank for the positive comments on the impressive dataset, and also pointing out the flaws listed below, which we have addressed in detail after each comment or suggestion forwarded be the reviewer.

The paper focuses on two objectives stated in the Introduction, and a third objective that, for some reason, is presented in the Methods (lines 157-159). The objectives all are targeted at examining the relationship between DOC concentrations and optical properties, particularly absorbance at275nm or 440nm. The paper would be improved if it were structured around testable hypotheses, which I think the authors could do without too much additional work.

Responses: The authors really thank for the reviewer's instructive comments, we added testable hypotheses in the revised manuscript, and structured the layout of the manuscript according to the testable hypotheses. Thanks again for the valuable comments that really help for the improvement of the manuscript.

The primary means of data analysis is simple linear regression, and it appears that perhaps multiple linear regression was attempted (line 279-280). Surprisingly, no description of data analysis is provided in the paper (or the supplemental information). In fact, P values are not even provided for the regression analyses. Nor is there any indication of testing for normality or other assumptions for linear regression.

Responses: The authors thank for the comments. In the revised manuscript, descriptive statistical analysis were conducted for the data set, and assumptions for the linear regression were also tested for these regression analysis, in addition, P values for each regression model also were also provided in the revised manuscript. Many of the graphs show that a single data point, or a couple data points, appears to be leveraging the relationship (e.g., Fig 3c, Fig 3e, Fig 3f, Fig 6d, Fig 6f, and others). In these cases, the validity of the regression equation is highly questionable.

Responses: The authors thank for the comments. We agree that a single data point or a couple data points might have improved the R-squares for these regression models, however, these data points are in situ measured values, and thus they reflect the natural situation. In the revised manuscript, we also did the regressions without these data points, and the results indicated that the R-squares did not affected much. We really appreciated your thoughtful comments. We could provide these regression metrics with and without these points in the revised manuscript.

In the case of Fig 8, it is not clear how the groupings were selected. The text mentions "trial and error" which suggests to be it was a very subjective process of selecting the M ranges for the groups. Responses: The authors thank for the comments. In the current manuscript, the results presented in Figure 8 were derived based on trial and error testing of the regression modeling. The M value is used to classify CDOM into different groups, which might have similar CDOM absorption efficiency or absorption ability in each group, thus the CDOM absorption coefficient in each group should have similar relationship with DOC. However, how to determine the range for each group is still very subjective, we will further investigate and try to find a more reliable method for the grouping process. The testing results will be presented in the revised manuscript, thanks again for the comments. I am a bit concerned about the holding time (up to 2 days) before ïňĄltration. Do the authors have any evidence that there was no degradation of DOC during the holding time? Some concern for chlorophyll-a. Also, it is questionable to collect and store DOC for optical analysis in HDPE bottles. Why was HDPE used instead of glass?

Responses: The authors thank for the very thoughtful comments. All the water samples ship back to laboratory and then stored in refrigerator at about 4âĎČ in the dark, thus the biodegradation should be very limited for DOC at low temperature. Similarly, the photo-degradation is also avoided since samples were kept in the dark. Some literatures also addressed this issue, and found that DOC is relatively stable, its change in two days at low temperature without photo-degradation should be neglectable. As

for the HDPE sampling bottle, according to my knowledge, it is quite common to use HDPE bottles for field sampling to test water quality parameters, which has been previously cleaned by soaking in 0.5 mol LÂň-1 HCl followed by 0.1 mol L-1NaOH for 24 h before heading to the field. According to Zhang et al. (2007), samples kept in two day before filtering would not cause obvious degradation for DOC concentration. Using glass bottle is not easy to ship back from field to laboratory during the bad road conditions, especially in Tibet or other remote areas where county roads are very common, which could cause severe damage of the glass bottles, thus HDPE bottles were used.

In the end, the authors state that SUVA is not an appropriate metric for the purposes of their study because its calculation includes DOC concentration. This left me wondering why it was included at all?

Responses: The authors thank you for the comments. Actually, we used both SUVA and M value (a250/a365) to characterize CDOM molecular weight qualitatively, and particularly SUVA is a very effective index for characterizing the molecular size of CDOM, thus we prefer the keep this part in the manuscript, but its linkage with CDOM grouping will be removed in the revised manuscript. Thanks again for your kind concern.

I think the Introduction could be shortened by as much as a third without any loss. Much of the introduction deals with remote sensing for DOC, but this paper does not address remote sensing directly; the background information on remote sensing could be greatly reduced within the Introduction and also the Discussion. I think developing some testable hypotheses and keeping the Introduction (and the whole paper) focused narrowly on those hypotheses would make for a shorter, and more readable, paper.

Responses: The authors really thank for the reviewer's very instructive comments. As you may see that there is another reviewer who also suggests to shorten this part, thus, the Introduction will be shortened in the revised manuscript.

I would strongly suggest separate Results and Discussion sections. As I read the paper

it was not always clear when the authors were making statements based on their data, versus general statements from literature.

Responses: Again, the authors thank for the very thoughtful comments, and similar comment were also raised by the third reviewer (Professor P.K.Kowalczuk), we separate the Results and Discussion sections in the revised manuscript. We really appreciate this comments, which would definitely strengthen this manuscript.

Try to avoid vague statements such as “massive organic matter” (line 22) and “big variation” (line 230). The English in the paper is mostly correct, but it could certainly be improved if edited closely by a native English speaker.

Responses: The authors thank for the comments, your kind comments were adapted in the revised manuscript, further, and the authors have requested Professor Lin Li from IUPUI (Indiana University Purdue University, Indianapolis) edit the English in the revised manuscript.

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