Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-179-RC1, 2017

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

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

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 flaws that limit its value.

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 at 275 nm or 440 nm. The paper would be improved if it were structured around testable hypotheses, which I think the authors could do

Printer-friendly version

Discussion paper

without too much additional work.

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. 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.

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.

I am a bit concerned about the holding time (up to 2 days) before filtration. 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?

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?

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.

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.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-179, 2017.

Printer-friendly version

Discussion paper