

Dear Editor and Reviewers:

Thank you very much for the pertinent comments of the article. I have read several times carefully, and the responses to the reviewers comments are listed as followings.

Thank you very much.

With best regards

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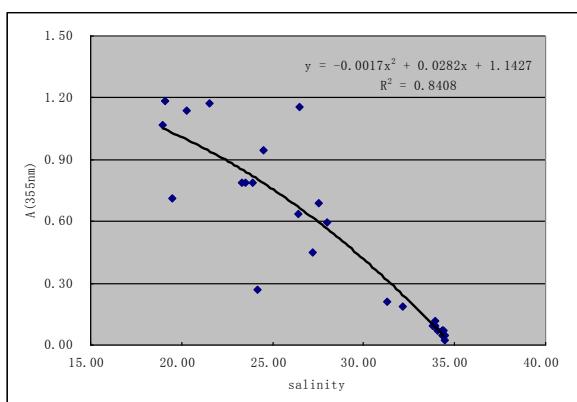
Reviewer #1:

No data have ever been published about relative research of relationship between DOC and CDOM in Changjiang Estuary, this research is designed as a preliminary study in this area for the topic, we all think it's important for this biggest river in Asia. We carried out the field investigation in low tide as an ideal environment, when the area is mainly controlled by Changjiang Diluted Water, exhibiting gradually mixture of CDW and saline oceanic water, avoiding typhoon and seasonal variation of hydrodynamic current so far as it might yet be possible. But we met unexpected big wind during winter sampling. Then do found the effect from resuspension and seasonal variation of primary production. Our intention is to give some rudimentary knowledge of relationship between DOC and CDOM, but not to supply an overall inversion model for oceanic color remote sensing.

For the review "The quadratic fit between aCDOM(355) and salinity is statistically insignificant $R^2 = 0.075$ (explains less than 1% variability in data set). Statistical results of the linear mixing model are much better but its significance is undermined by small number of data points and a big gap in the data sets in the salinity range 0-10."

We redo the quadratic fit, both have good relationship of 0.84 without XM section (salinity range is 0-10) and 0.94 for the all data.

It should be a negligence when ask the master student to draw the figure. Apologize.

**Reviewer #2:**

.....So to be brutally frank, there is not much that is new here, except that these measurements were acquired in a different geographical region that had not been previously examined to any great extent. Whether this is sufficient justification for

publication in Biogeosciences is not clear to me. Further, in my opinion, the paper needs major revisions to improve the clarity of the presentation: the text could be cut by half without losing content, and many figures could be combined or summarized within the text or a table. In some sections, the manuscript reads more like a review article than a research paper.

The text have been polished and cut off part of reviews.

1. Figure 1 is essentially useless, as the acronyms are neither defined in the figure caption nor discussed in the text.

Figure 1 is important to give an overall outlook of the hydrodynamic situation around the research area, the figure have been adjusted and make it in accordance with the acronyms in the text.

2. Page 12221, line 16: Meaning??

The expression is not appropriated, deleted.

3. Page 12221, line 25: Don't you mean "fitted" not "simulated"?

Fitted, revised

4. Page 12222, line 1: Why was k discarded? Doesn't a non-zero value (within the spectrophotometric accuracy) indicate that you have a baseline offset that you have not accounted for?

To remove residual errors from both scattering and instrument noise

5. Equation 2: Spectral slope is defined as S here, S_g in text.

Forgive the carelessness, have revised

6. Page 12222, lines 16,17: What reference? Where was this reference obtained??

Deep sea reference water, bought from University of Miami, RSMAS/MAC

7. CDOM absorption spectra: Why don't you plot both CDOM absorption and spectral slope vs. salinity on a single plot?

We ever tried, but found the figure was in a disorder, if necessary, we would redraw all the figures.

8. Page 12224, lines 1,2: A value for the absorption coefficient of 0.0461 m^{-1} translates to an absorbance of 0.002 for a 10 cm cell. This value must close to the detection limit of this spectrophotometer and thus be a highly tenuous measurement. You certainly are not achieving three significant figures in this measurement, lucky if it's one (see also below).

We tried to do linear regression without the three points, found R^2 is about 0.54, although it may be useless for only 6 data, but we do found the tendency.

9. Page 12226, lines 9,10: Phytoplankton are not known to consume CDOM, which is what you appear to be stating here.

10. Page 12226, lines 19-24: This so-called good correlation is driven by a single high point, while the lower CDOM absorption measurements are highly suspect (see comment 8 above).

Further, if the CDOM is being produced by phytoplankton, while do you see an inverse relationship with salinity (Fig. 7)?

We tried to illustrate the relative amount of CDOM which is absorbed by phytoplankton and biodegrading of metabolite. It's not our intention to disturb. Since the data of three stations is very near to limitation of the instruments, we decided to not discuss this part in the article.

11. There are several errors in the referencing in Table 1.

In summary, in my opinion this paper needs major revisions before it could even be considered for publication.