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Biogeosciences Discussions

Interactive comment on "A Global End-Member Approach to Derive $a_{\mathrm{CDOM}}(440)$ from Near-Surface Optical Measurements" *by* Stanford B. Hooker et al.

Anonymous Referee #1

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Hooker et al. assess the performance of a Kd ratio-based empirical acdom(440) algorithm developed in Hooker et al. (2013) by using a large in-situ dataset collected across different water types ranging from turbid inland waters to clear oceanic waters. The use of spectral end member Kd ratios (Kd(320)/Kd(780)) is an interesting approach to estimate acdom(440). The authors extend the measurement suite (Beaufort Sea, Mackenzie River outflow and the Southern Mid-Atlantic Bight; Hooker et al. 2013) to a global perspective by sampling "extremely clear to turbid waters (e.g., North Pacific Ocean, the Arctic Ocean, Hawaii, Japan, Puerto Rico, and US east and west coasts) that span three decades of dynamic range in optical properties. The dataset was classified into two main categories, namely conservative and non-conservative water masses, with

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non-conservative dataset further divided into 15 sub-categories to clarify and contrast with the more robust relationship between Kd(320nm/780nm) and acdom(440) for conservative water masses. The worldwide field dataset of downwelling irradiance (Ed) profiles collected with advanced optical instrumentation of C-OPS and C-PrOPS and the spectrophotometer-measured acdom for corresponding water samples presented in this study are valuable to investigate the CDOM contributions to Kd variations in several important marine ecosystems. However, the authors spend too much effort on data classification with text descriptions but did not present any visualized information of these valuable datasets, which are more interesting to the general readership. Further, it appears that the Methods section needs to be better organized as it is hard to follow. For Results section, discussion contents accounted for large portion of each sub-section, and some of the discussions appear speculative as it did not go deep enough to illustrate the real optical mechanism behind the relatively worse Kd ratio-acdom(440) correlations. The overall impression about the article is positive, however, I would suggest the authors to reorganize the contents and work through the paper once more to revise the text and figures, making a worthy publication of such a valuable dataset.

Below are some specific comments: 1. Abstract, Page 1, line 18, 'e.g., increased vertical resolution to less than 1 mm. . .' Would like to see a better justification for this high vertical resolution even for turbid waters. Does the pressure sensor used in the C-OPS achieve such a resolution?

2. Introduction section: Some of content appear redundant, for example: (1) Page 2, line 27-28 and Page 3-4, the authors spend much effort describing the global perspective of sampling sites, which is a simple concept and can be properly combined and shortened. (2) It's better to combine the descriptions on the algorithms developed in Hooker et al. (2013) in Page 2, line 1-9 and Page 3, line 11-18 to avoid repetition and make it more clear to readers. (3) Page 2, line 17-18, looks not that accurate as there are many new sensors with high spatial resolution that have been used to study inland

and estuarine waters (e.g., Sentinel 2-MSI, Sentinel 3A/B-OLCI and Landsat 8-OLI), the authors may want to check on that and add some references. (4) It's better to move the summarized importance of acdom(440) in Page 2, line 19-25 to the first paragraph after line 3 ". . ..by Nelson and Siegel, 2013" ; it would be more coherent in content.

3. I would like to suggest the authors to reorganize Data and Methods section and again shorten the descriptions on global perspective sampling sites to avoid repetition. (1) Page 7, line 9, please use detailed number to replace "almost all". (2) Page 8, line 13, please add time period of your field dataset. (3) Page 11, line 9, please add reference after ". . .converted to absorption coefficient". (4) In section 2.5, Data Subcategories, can you provide more quantitative information of some of the criteria used for subcategories, such as, what's the Chlorophyll a value used to define algal bloom? what's the dominant species of HAB? You may want to mention this information as some of the HABs, like red tide species and cyanobacteria blooms display totally different optical properties, and further distinct Kd spectra; in addition, the authors mentioned that a sample sometimes satisfied more than one subcategory; so can you explain more about why classify data in this way which uses little quantitative information since the authors already utilized K-mean classification of Kd spectra, which make more sense. In the Results, the authors may want to compare Kd ratio with acdom(440) for each K-mean classified cluster to see if the correlations can be improved for each group.

4. In Results section, I would strongly recommend the authors add one more section to display some of interesting dataset (e.g., Kd and/or Ed spectra) collected in conservative and non-conservative water masses, such as, Hypersaline Lakes, river mouth, HAB.

5. In Results section, the authors displayed the correlations between Kd ratio and acdom(440) for different categories, however, they did not evaluate the performance of the algorithms, it's better to keep some dataset for validating their algorithm and study the errors and uncertainties using statistic parameters like RMSE, ARE, R2.

6. Page16, line3-7, the authors mentioned better correlation for hypersaline or alkaline lakes compared to the overfilled lakes, and explained turbidity could be the possible disturbance. More information should be provided, such as, how the turbid water modified the spectrum of Rrs, Ed and further Kd. What type of sediment, like mud, clay or silty increase the turbidity? Also, what's the "atypical constituents" in line 7, and can you add a reference here to support? How does this constituent influence the Kd spectra? Same thing for section 3.2, explain more about the influence of sediment-resuspension on high turbidity and on the variations of Kd.

7. Page 17, line 19-23, what's the atypical algal bloom, please add more information here.

8. Page 18, line 21-22, the author mentioned UV attenuation, which is likely due to production of UV-absorbing pigments (e.g., Mycosporine-like Amino Acids (MAAs)) by phytoplankton in response to UV stress. Please add more information and a reference here. Also, the authors may want to add information of the dominant species of algal bloom, because there are some species that can also strongly modify the spectrum in 700-800 nm range, like Trichodesmium.

9. The description on NASA NOMAD data should be moved to Data and Method section.

10. In section 3.7, some of contents relevant to method of K-mean classification seems to fit better in Methods, section 2. Further, it's better to move the whole section 3.7 up to the first sub-section in Results, which could help the general readers to better understand the algorithm performance (or nonperformance) of non-conservative waters.

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