

Interactive comment on “New insights into mechanisms of sunlight-mediated high-temperature accelerated diurnal production-degradation of fluorescent DOM in lake waters” by Yijun Liu et al.

Anonymous Referee #1

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Major comments

The authors measured diurnal changes in CDOM components, DOC concentrations, and nutrients concentrations in two small lakes in Tianjin University, China to identify the biogeochemical processes controlling the diurnal DOM variation which is feasibly related to global warming. I think the research topics described in the manuscript would be of great interests to readers in Biogeosciences. However, I also think that the manuscript is not clearly written and difficult to follow, not technically sound and not appropriately discussed in the context of previous literature. Some of figures are not

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clear. Please see comments listed below

(1) The authors ran PARAFAC modeling to determine fluorescent components for each time period and compare the components among time periods. First of all, since there is no description regarding how the authors determined the number of components and validated PARAFAC models, I cannot evaluate whether the conclusion derived from PARAFAC are scientifically/technically sound or not. My opinion from our experiences of PARAFAC modeling is that it is not reasonable to apply PARAFAC for a dataset comprising the small number of samples ($n < 20$). It seems that the authors used small dataset for PARAFAC modeling (lines 197-200; I cannot understand the sentence though. . .). While, since the validity depends on the dataset for PARAFAC modeling, the authors should describe the validation method to identify the number of PARAFAC components and show the results of the validation.

(2) The authors concluded that some components were disappeared over a 24-h diurnal period (e.g., lines 29-32) from the qualitative analysis by PARAFAC. I do not agree with this conclusion, because it may be results of artifact by PARAFAC modeling. For example, if the authors apply new PARAFAC with slightly large number of samples, the disappeared components may appear. If the authors want to conclude disappearance of components quantitatively, the authors should use single PARAFAC model and determine diurnal changes in fluorescence intensity of individual PARAFAC components. I also disagree the conclusion of “degraded into four FDOM components (lines 32-35)” with the same reason.

(3) The authors described temporal changes in PARAFAC components quantitatively (e.g., lines 232-234). However, I cannot follow the time periods which involving the temporal changes. Again, it is difficult to quantitatively compare abundance of PARAFAC components derived from different PARAFAC models.

(4) The authors seemed to conclude that EPS produced during early morning partially degraded at midday and remineralized at night time (lines 32-35). Is this conclusion

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consistent with temporal changes in DOC concentrations and nutrients concentrations (Fig. 6)? I don't think so. To verify the authors' conclusion, careful, quantitative, and statistical comparisons of diurnal changes in PARAFAC components with diurnal changes in DOC and nutrients concentrations should be made.

(5) It seems that the authors generally consider/discuss degradation of EPS to explain daytime changes in PARAFAC components. Why don't the authors consider DOM production including the PARAFAC components with primary production during daytime?

Other comments

I listed some other comments below. Similar errors with some of the comments were found throughout the manuscript.

Lines 69-72: What is "key DOM components"? It's not clear.

Lines 99-101: I could not understand the message of the sentence. The authors would like to mention "diurnal degradation processes depended on the photosynthetic activity of primary producers"?

Lines 102-105: "a significant decrease of its fluorescence intensity with increasing water depth" is due to photoinduced degradation? I don't agree with it.

Line 105: Which overall day-night degradation of FDOM corresponds "such"?

Fig. S1: The figure is not clear. It may be better to show a campus map with map of Tianjin city.

Line 134: Do seagrasses occur in inland closed lakes

Line 149: Add filter name and company name supplied it.

Lines 152-153: "a combustion total organic carbon (TOC) auto-sampler analyzer" is awkward.

Lines 154-155 & 168-169: The authors described a method for UV-Vis absorbance

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twice. In addition, the authors described different spectrophotometers, respectively.

Lines 171-180: How did the authors calibrate the fluorescence intensity? With quinine sulfate? Also, what is “700 v” in line 174.

Line 209: Define “early stage DOM”.

Lines 223-226: I think no reference cited here describe “FDOM production by degradation of EPS would occur during the gradual increase of SI”.

Lines 245-247: I could not follow how the authors estimate the abundance of EPS quantitatively.

Line 254: The citation of Ma and Green (2004) is not appropriate for the sentence, because their work was carried out Lake Superior.

Line 415: What is “photoinduced respiration of phytoplankton”? In addition, it seems that the authors discuss the photosynthesis rather than respiration at the rest of this paragraph.

Lines 432-465: I think the authors do not discuss “A global view of production and degradation pathways of FDOM in lake waters” in this paragraph.

Line 435: I could not find Figs 12 and 13.

Fig. 2: The figure caption, in particular about (b), does not explain the figure.

Fig. 6: Describe which Y-axis corresponds to each plot in all figures except the figure(e). Why does the Y-axis on right side of Fig. 6d show negative values?

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