



EGUsphere, referee comment RC2
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Comment on egusphere-2022-30

Anonymous Referee #2

Referee comment on "Variations in dissolved and particulate organic carbon dynamics in the lower Changjiang River on time scales from seasonal to decades" by Yue Ming et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-30-RC2>, 2022

Review of 'Variations in dissolved and particulate organic carbon dynamics in the lower Changjiang River on time scales from seasonal to decades' by Ming et al.

General comment

In this work, Ming and colleagues reported the organic carbon dynamics in the Yangtze (Changjiang) River on seasonal to decadal scales. The authors attempted to evaluate DOC and POC concentrations, fluxes, and chemical composition based on monthly sampling results during the 2016-2020 and earlier results retrieved from the literature. OC transport by large rivers has become an increasingly important research topic due to its complex responses to global change. For the Yangtze River studied in this work, clearly its OC dynamics are not an exception and have been substantially modified by both climate change and human activities. The latter may be the dominant factor in consideration of the large number of dams constructed in the river basin. While this research work fits well with the scope of the journal Biogeosciences, OC transport in the Yangtze River has been widely examined in the literature (see a few examples in the specific comment above). E.g., more than 30 research articles have studied DOC and/or POC in this river in the recent 10-15 years. I don't think this manuscript represents any significant research breakthroughs in this field/study area.

When the authors attempted to explain the different, sometimes contrasting, responses of DOC and POC to flow and sediment changes, they tried to use 'flushing' and 'dilution'

effects. I don't challenge these effects and I do believe they have naturally occurred. However, many statements are speculative and lack of solid evidence to support (see specific comments below), and even contradictory (e.g., L249-254).

Overall, the grammar and syntax are shaky throughout the text. There are numerous grammar errors with many confusing statements. I started correcting this in detail, but was soon overwhelmed by the number of changes that could be made. A further language editing is needed before its resubmission.

Specific comments (with line number):

L23: What kinds of changes have been observed in the Yangtze River? Please elaborate.

L24-26: This statement is quite confusing and unclear. The authors need to reword this to make it directly related to the study river.

L27: change 'should also be' to 'has also been'

L30: change 'tremendous amount of' to 'large quantities of'

L44-47: this statement is problematic. For organic matter like OC, there are quite a lot of studies already available in the literature. E.g., Sun et al., 2021. Source, transport and fate of terrestrial organic carbon from Yangtze River during a large flood event: Insights from multiple-isotopes ($\delta^{13}\text{C}$, $\delta^{15}\text{N}$, $\Delta^{14}\text{C}$) and geochemical tracers. *Geochimica et Cosmochimica Acta*. Shi et al., 2016. The spatial and temporal distribution of dissolved organic carbon exported from three Chinese rivers to the China Sea. *Plos One*. Wu et al., 2018. Spatiotemporal variation of the quality, origin, and age of particulate organic matter transported by the Yangtze River (Changjiang). *JGR Biogeosciences*. Also, Wu et al., 2015. Temporal variability of particulate organic carbon in the lower Changjiang (Yangtze River) in the post-Three Gorges Dam period: Links to anthropogenic and climate impacts. *JGR Biogeosciences*; Yu et al., 2011. Impact of extreme drought and the Three Gorges Dam on transport of particulate terrestrial organic carbon in the Changjiang (Yangtze) River. *JGR Earth Surface*. Zhang et al., 2014. The spatiotemporal distribution of dissolved inorganic and organic carbon in the main stem of the Changjiang (Yangtze) River and the effect of

the Three Gorges Reservoir. JGR Biogeosciences.

L64: what is a position well? please provide more details.

L75: while the OC was sampled at Xuliujing station, flow and sediment data were measured at Datong station (500 km apart), how were the flow and sediment inputs between the two stations accounted for, especially there is a large lake (fig. 1) in between the two sites.

L115: I don't think this a good reference to support the DOC measurement method.

L121: for SPM, have the filters been weighed before sampling? If not, this may cause errors as each filter weight is different.

L133: how were the decadal data collected. No details were provided. Also, if they were collected from the literature as indicated later in the text, have the authors checked the data quality and sampling consistency. E.g., are the sampling results at different sites (e.g., Datong and Xuliujing) comparable? How to resolve the lake impact? These should have been clearly provided in the text.

L142: the authors mixed results and discussion together in this section, but also presented a separate 'discussion' section. The structure is quite confusing and different to follow. I would suggest the authors move all discussion words into the 'discussion' section and keep only descriptive results here.

L147: change 'should' to 'may' or 'might'

L158-161: this is a speculative statement. Do the authors have evidence to support this?

L192-194: as the lower Yangtze have been heavily regulated by the Three Gorge Dam, does this distinct seasonal pattern reflect the impact of the dam? Flood seasons with high sediment concentrations (reduced light penetration and turbulent flow conditions) are typically not favourable for autochthonous production.

L241: any references to support this?

L249-254: the two sentences are contradictory to each other.

L265:268: again, these statements are confusing. The authors may also need to pay attention to the upstream dam impacts on flow regulation and sediment flushing.

L297: this is not a good reference to support this statement. Also, this paragraph is quite general.

L308-311: again, a speculative statement. Do the authors have evidence to support this argument?

L323: why not include 1-10 kDa DOC in fig 7?

324: note that you have not yet discussed SPM and POC concentrations until now. The structure here is confusing and difficult to follow.

L333-346: you cannot simply use studies in other rivers to support your arguments in Yangtze.

L355: for the annual fluxes, how were them calculated? What is the uncertainty?

L360: are the authors sure that the finer SPM will be preferentially trapped, not the coarser ones? In my opinion, the increase grain size is more likely caused by the river-bed armouring process after the TGD dam operation.

L369-370: unclear wording.

L375: The POC% is increasing with increasing sediment particle size. This is in contrast to our common understanding that POC% will usually decrease with increasing sediment particles.

L378-380: speculative statement without supporting evidence.

Fig 5a: the legend for POC is different from the one shown in the graph.

Fig 7: why not include 1-10 kDa DOC as the authors defined 'HMW-DOC' in the text?

Fig 10a, if the DOC data before 2003 were considered, the temporal trend will be reversed.