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Biogeosciences Discussions

Interactive comment on "Organic carbon transport and impacts of human activities in the Yellow River" *by* L. J. Zhang et al.

Anonymous Referee #2

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Review of Zhang, L. J., et al., 'Organic carbon transport and impacts of human activities in the Yellow River' for Biogeosciences Discussions, 9, 14365–14405, 2012

The study of Zhang et al., aims to use a new dataset of particulate and dissolved organic carbon measurements from the Yellow River to examine the role of anthropogenic activities (mainly reservoir construction and management) on carbon transport and flux. The Yellow River is a major fluvial system in terms of suspended load flux to the ocean, and drains a vast continental area. The study is impressive in terms of the scale of the sampling, and some of the finer temporal resolution datasets, which are not common for large river systems. The drainage area also includes semi-arid biomes which, as the authors point out, have been somewhat neglected in sampling schemes to date. Therefore, the new combined hydrometric (suspended solids and

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water discharge) data and organic carbon load measurements are extremely valuable.

However, I cannot recommend publication in Biogeosciences at present. In my opinion, the role of natural versus anthropogenic factors needs to be explored in more detail. This could be done at the expense of the discussion and use of the 'COD_Mn' proxy, which based on my analysis below seems flawed. I have outlined three main comments and a series of other comments as they appear in the manuscript, which I hope will aid the authors in the production of a revised manuscript.

Main comments:

1. Role of 'natural' versus 'human' events: Given the high temporal resolution of the data collected from it was rather frustrating that not more analysis was done on how POC and DOC were mobilised and transport through the system. For example, on pg14374 Lines 21, the authors note that TSS and POC increase during ice-breakup. How does this dynamic compare to the WSR events which are the focus of the figures? Also, what happens during flood events in this semi-arid setting. The source and transport rates of POC have been shown to be strongly influenced by flood events and ice break up, and reference to this literature would be informative. Second, the authors undermine their interpretation of the role of WSR events on pg 14375 Lines 5-10 stating that it was 'also the wet season' when the samples were collected. Why not use a precipitation dataset to identify flood events (and snowmelt based on temperature) to allow the reader to clearly see the role of natural versus anthropogenic events? Also, the authors need to explain what the purpose of the WSR events are earlier in the manuscript. It would be useful to provide more background on what these hope to achieve (removal sediment from channels, removal of sediment from reservoirs) and their un-intended consequences (sediment delivery to the esturary, refilling of channels) to evaluate the impact of the management scheme on POC transfers.

2. The use of the 'Permanganate Index' COD_Mn: In my opinion this is fundamentally flawed. The equations 1 and 2 are simplified to equation 5 based on an assumed

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constant stoichiometry of organic matter. However, C/N and C/P ratios of particulate organic matter can vary hugely in river systems because of both: i) variation in the source of POC delivered to the river (e.g. plants with C/N>100, soil C/N$\sim$15, marine sedimentary rocks C/N<10); and ii) and cycling of POC in the water column at low flow rate, invalidating the assumptions of this calculation. Also, as a direct result of the assumptions, COD_Mn becomes related to TOC with constants derived from the molar mass of C (Wc) and # of C atoms (m), meaning relationships between TSS and COD_Mn are misleading due to autocorrelation. I would prefer to see this removed from the manuscript in favour of a more detailed discussion of point 1 above.

3. Explanation for the sampling design: It was rather unfortunate that the two high temporal resolution sample sets (from the Huayuankou and Lijin Stations) were made at different times (2006 vs 2008). I appreciate the logistical challenges of sampling a river system like this, but in the text it needs to be clearer why the decisions to sample at the times chosen (e.g. why the certain months during the 'snap-shot' temporal sampling scheme). Also, how were the samples collected from the rivers? Large rivers display significant sorting of particles with depth – the authors need to consider how this may impact their results (depending on what type of samples these are, surface versus integrated).

4. Full dataset: There were some substituted water discharge values in the dataset as discussed Pg14370. Since no appendix was given this makes it very difficult for readers to assess the overall quality of the data, and makes reusing the dataset problematic. I would recommend providing a full electronic appendix of the all samples collected and the analyses reported in this manuscript.

Other comments:

There were numerous grammatical and typographic errors throughout the manuscript (e.g. just for illustration pg 14366 line 19: 'form' instead of 'from'; pg 14367 line 8: accumulation misspelt). Most of these are relatively minor, but the manuscript would

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benefit from a thorough proof read and edit.

Pg 14366: Given the size of the flux, refer to in Mega tons rather that Gt. Are the POC and DOC fluxes per year? Use consistent units, g or t.

Pg 14367 line 27: qualify this statement – forth largest in the world?

Pg 14368 line 3: remove, a conclusion from this study. This whole paragraph is a bit awkward as it contains a collection of previous work done by the authors and description of sample collection. It needs to be better linked to the rationale that precedes it.

Pg 14369 line 26: 'Suspended River reach' is not a good term. How about 'Downstream reach' for simplicity.

Pg14370 Line 1-5: Why were these sampling dates chosen (the specific months). It would be useful to provide a typical annual hydrograph of these rivers and indicate the relative timimg of these collection dates. Line 22: this is an important detail – using other data to fill in the gaps. All data should be provided as an appendix so that the published results can be verified and furthered by subsequent research. Without this, how do we know what data processing has been done?

Pg14371 How were the samples collected from the rivers? Large rivers have strong gradients in hydrodynamic sorting that should be carefully considered. If the samples are surface grab samples, then we need to be careful about interpreting temporal patterns which may instead reflect grain size variability. Line 9: please provide a reference for this procedure. Line 17: please define COD_Mn the first time it is referred too.

Pg14372: I'm not sure how this procedure works for riverine POC which is known to have marked variation in C/N and C/P ratios because of both: i) variation in the source of POC delivered to the river (e.g. plants with C/N>100 and soil C/N$\sim$15) and ii) and cycling of POC in the water column at low flow rate. This will impact the assumed element ratios (which give equation 3 and 5) and the values calculated. It seems this

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could invalidate the assumptions of this calculation, making the calculation of COD_Mn from only the molar mass of C (Wc) and # of C atoms (m) very problematic. See comment 2 above.

Pg14378 Line 25: What does this background represent? Fossil organic carbon contained within the eroded sedimentary rocks?

Pg14383: This trapping of POC is a first order estimate (which needs to be spelt out in the abstract), but it does seem to be regionally important. It would be useful to briefly comment on the potential impact of this storage (is it a net carbon sink, or source if the POC is remineralised in the reservoirs?), bearing in mind the broad assumptions in the calculations.

Pg14385: It needs to be much clearer how the DOC and POC fluxes were calculated (averaging method? Rating curve method?).

I would recommend overhauling the discussion, paying much closer attention to the novelty (and strength) of the dataset – the temporal resolution of sampling allowing events to be isolated and interrogated.

Figure 1: Can the station names referred to in later figures be made a little clearer against the topographic map? Why are the open symbols slightly off the Yellow River? Is this the true sampling locations? What is the filled triangle?

Figure 2: label more clearly where the reservoirs are on this distance to river mouth plot.

Figure 5: What is the difference between the filled and open symbols? Please explain in the caption. What station are these samples from? What are the trends? Again, the caption needs to be clearer.

Figure 6: from which river station and sampling period have these measurements been made?

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Figure 8: Are these data for a reservoir? Or a station on the river? needs clarification in the caption. Needs a figure legend to explain the points. Also, I'm not sure this figure is needed? Is the point to say there is no relationship between Qw and DOC?

Figure 9: given this figure refers to the DOC/POC ratio, I'm sure most readers would find it beneficial to be in a linear y-axis.

Figure 10: Autocorrelation is probably behind these plots. Remove based on point 2 above.

Interactive comment on Biogeosciences Discuss., 9, 14365, 2012.