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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 #1

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The paper by Zhang et al. presents an interesting set of suspended matter and organic carbon data from the Chinese Yellow River. They collected data along the course of the river and also sampled four reservoirs during several expeditions. With this set of data they aim to discuss the "Organic carbon transport and impacts of human activities in the Yellow River". While the data set is appropriate for this purpose the manuscript itself suffers from major drawbacks. First of all, the English is unacceptable. Syntax and word meanings are often wrong and make it in some passages extremely difficult to follow the reasoning. This is particularly disappointing, because one of the authors is working in the United States for a long time. Second, the discussion is mostly a lengthy listing of numbers and description of conditions rather than discussing own findings related to processes and putting it in the global context. The discussion remains at the surface and oftenly simply describes well-known phenomena.

Therefore, I cannot recommend this paper for publication in Biogeosciences. However, the data set is worthwhile being published and therefore the authors might be given a chance to revise their manuscript. Detail comments are following below.

General:

Besides the major drawbacks mentioned above, there are a few other general points to mention. The abstract is too long, contains too many details and too little conclusions. What do we learn from this study? What is the take home message? At the end of the introduction the gaps in knowledge should be defined and then the major goals of the study should be identified. This is missing. What are the major hypotheses/objectives? In the methods section the authors describe a very unusual method for determining labile organic carbon. The described calculation simply from chemical oxygen demand appears to be inappropriate.

Besides remaining mostly at the surface the discussion is far too long. The chapters 4.3 and 4.4 can be deleted and the discussion should be completely reorganized. The discussion has to go into more detail with regard to the large variations in discharge and TSS and organic carbon transport related to the WSR (water and sediment regulation) and also to the use for agriculture. I suppose that irrigation is not constant throughout the year. There must be large seasonal variations in water withdrawal that will affect affect the river transport. The discussion should focus more on that and needs to be more detailed in that respect. This is the interesting new aspect of the study. In contrast, the other processes described in the paper are mostly very well known phenomena (TSS – POC relations, dilution/concentration). The discussion related to these does not add new knowledge. It should be pointed out which effect the water withdrawal and WSR events (incl. seasonal variations) have on the various sections of the river and the final discharge into the ocean. The conclusion chapter is simply a summary and does not contain any conclusion.

Title:

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The "organic carbon transport" and the "impacts of human activities" are only connected by an "and". Is there a causal link? You may rephrase it to "Impact of human activities on organic carbon transport in the Yellow River".

Abstract:

It is too long and simply describes numbers and conditions. Present conclusions and a take home message.

Introduction:

As mentioned above, the final part that should define the gap in knowledge and the major goals of the paper is weak. Write some clear hypotheses and/or objectives in order to prepare the reader for what is coming.

Data sets and methods:

P. 14270, l. 10: What is meant by "downward Lijin station was also taken into consideration"?

P. 14270, l. 17-24: I am concerned about the use of the terms "natural discharge" and "actual discharge". It is not yet clear to me how this "natural discharge" is calculated. I understand that you add withdrawn water to the measured discharge, but what do you mean by "reservoir variation"? Moreover, this type of calculation can only be used at or downstream of a reservoir. Anyway, I find the used terms misleading.

Results:

P. 14373, l. 7-12: I disagree with this line of reasoning. I understand that you calculate material transport (TSS and organic carbon) with your "natural discharge". That means you do not use the amount of water that is actually running dowm the river, but you calculate with a discharge that would be there, if there were no reservoirs and irrigation. Is that right? To my opinion, you can only calculate fluxes with the factual amount of water that is transported by the river.

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P. 14374, l. 13-14: I am getting confused, now you are using "actual discharge" instead of "natural discharge". Why?

P. 14374, l. 18: What is meant by "exogenous"?

P. 14374, l. 19: What is meant by "POC% increased significantly"? Do you mean statictically significant?

P. 14375, l. 2: What is the difference between "periods during which reservoirs started to release water" and a WSR event?

P. 14375, l. 14-15: What are the drainage and desilting periods?

P. 14376, l. 5: The date must be wrong. I suppose, you mean 6 July, don't you?

Discussion:

P. 14376, l. 24: What is meant by "some floods happened"? Were these periods of strong rainfall?

P. 14377: These are only comparisons and descriptions of conditions. What are the conclusions/inferences?

P. 14378, l. 11-14. How much is it? What is the reason for the difference? This is where the interesting part starts. Expand on this. And in the following three possible reasons are presented, but no discussion/justification is following. Is there a specific reason that you compare to the Mississippi and Nivelle rivers? It looks arbitrary.

P. 14378, l.27-29: Quite unfortunate discussion. You probably have a dominance of small sized particles (which contain the higher amount of POC) simply because of the loss of energy in the resrevoir. The river enters the reservoir, energy dissipates and flow reduces, hence, the energy is lost that is needed to hold large particles in suspension. As a consequence the large particles with low POC settle and the small particles remain in the water column.

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P. 14379, l. 1-2: A POC of 2% is not very high. Moreover, this is a circular argument ("POC% high. . .due to high organic material content").

P. 14379, l. 21-23: Previously, you mentioned (and show in your figures) much higher POC% contents. What is correct?

P. 14379+14380: "Several possible reasons. . ."; This is no discussion, just a listing of processes/reasons, not more.

P. 14380, l. 7: What is meant by the "severe human disturbances"? In which way did they affect DOC transport?

P. 14380, l. 22-24: This is a poor correlation and in the figure it looks more like a point cloud. How can you draw the conclusion on "dilution" from that?

P. 14380, l. 28: You mention the "end of the winter" and the "beginning of spring". As these seem to be periods which have some characteristic differences it needs to be very clear how they are defined and why they are defined like that.

P. 14381, l.1-2: But what is the reason for the DOC increase? This should be the starting point of the discussion, not the end.

Chapter 4.3 does not provide any information required for the purpose of the paper. It would be sufficient to mention the variation in the DOC/POC ratio in one or two sentences.

Chapter 4.4 also adds little to the story of this paper. Moreover, the method of defining "labile organic carbon" is rather doubtful. Also, the statement that "90 % of POC and 70 % of DOC cannot be degraded" lacks any rationale.

P. 14384, 1st paragraph: What does that mean in the global context? Are these numbers high or low? Why did you calculate them? Why did you compare to the Verde River? Where is that? It seems to be an arbitrary choice.

P. 14385, l. 15: What is meant by "a transformation from POC to DOC"?

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P. 14386, l.1-2: In which way? Now we need a discussion, else it is just pure speculation.

P. 14386, l. 13: This could be a nice conclusion of the paper. However, from the discussion as is this cannot be concluded.

Figures:

Fig. 1: The map is too small, font size needs to be increased.

Figs. 2+3: The X axis is hard to read. Just a few dates and too little ticks are given.

Fig. 5: I don't see the necessity of this figure.

Fig. 6: You should think about plotting the world rivers from Ludwig et al. (1996) to have a comparison, particularly as you mention that the Yellow River POC-TSS relation is different from Ludwig's.

Fig. 8: What does it tell us? I see point clouds.

Fig. 9 can be deleted. It simply says that POC is dominant. That can be mentioned in one sentence in the text.

Fig. 10: What is the use of it? It shows simple linear relationships. Moreover, as the f-COD and m-COD parameters are highly questionable, I wouldn't use it.

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