

## ***Interactive comment on “Spatiotemporal characterization of dissolved carbon for inland waters in semi-humid/semiarid region, China” by K. S. Song et al.***

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Received and published: 14 August 2013

### Responses to comments from referee #2

General comments Overview. This study presents data on dissolved organic and inorganic carbon concentrations in the Songnen Plain region of NE China. A contrast is drawn between freshwater lakes, most of which are “open” and have stream or river outlets, and brackish waters, most of which are “terminal” with no outlet. Interestingly, the researchers find that DOC and DIC are highly enriched in the brackish waters, and this is attributed to a concentration effect, presumably due to evaporative concentration

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in the absence of lake outlets for the brackish waters. This is in contrast to results from some other areas/other studies which often find the highest DOC in the freshwater endmember of a freshwater-saltwater continuum, generally attributed to high loading of terrestrial DOC from the watershed. The study also examines variation in spectral characteristics of CDOM and finds that some of these vary with lake type as well.

The topic is relevant in the context of aquatic carbon cycling and the global carbon cycle, and certainly of interest to readers of HESS. The dataset and findings are intriguing and I believe will ultimately make a contribution to the field. As the authors state, this study is of value for this region in order to predict DOC and DIC concentrations using salinity as a predictor for lakes which haven't had the C measurements. Many of the figures are excellent, and compelling. However there are many substantial underlying issues which I believe will need to be addressed before the article could be considered for publication. Some of the most important are:

(1) It is not clear how the sites were chosen, or if these 26 lakes are representative of the 9000 lakes in this region, which is important in the context of the discussion and potential extrapolation of the results. Were they randomly selected? If not, how chosen?

Response: The authors thank for the insightful comments. The 26 lakes were chosen according to two criteria, 1) the gradient of precipitation and outflow condition; 2) the size of these lakes is another concern as being chosen for sampling. Although more than 9000 lakes are present in the region, the average area only 0.85 km<sup>2</sup>, so our sampled lakes generally have large area and representing the fresh and brackish waters in the region. Also some of the lakes are ephemeral lakes (around 2000 small lakes), which are not chosen as targets for sampling.

(2) It appears as though multiple samples were taken from each lake on the date of sampling, and treated as replicates in statistical analysis; if so, this pseudoreplication and not appropriate. If not the case, this should be explained better as the methods

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are not clearly written.

Response: Thank for the valuable comments. At least three samples were collected for each lake, and they were not treated as replicates in statistical analysis. Data statistical analyses were done for each lake as they grouped into fresh or brackish waters. Details were added in the revised manuscript.

(3) Along similar lines, statistical methods were not given. Response: The authors thank for the comments. Details on statistical methods were added in the revised manuscript. (4) The proposed mechanism (concentration of DOC and DIC along with salts, for the brackish waters) is mentioned throughout the paper but is never really developed in a coherent way. The key for this mechanism to play out, would relate to water balance of the lakes: Is the water loss rate by evaporation high relative to in-lake loss rates of DOC and DIC, such that DIC and DOC are substantially more concentrated in lakes with a longer residence time (i.e., terminal lakes) than they are in lakes with a shorter residence time (i.e., "open" lakes as termed in this study). It may be possible with existing data to develop the necessary data on residence times, water balance and C processing rates to test the mechanism using a simple model; if so that could be included here in this work, or in another study and referenced here. Why are the waters brackish, that are brackish? Direct evidence should be presented for this. As it is, with what seems to be mostly anecdotal evidence for the mechanism, it should be mentioned as a possibility in the Discussion section and should not find its way into other sections.

Response: The authors thank for the very instructive and valuable comments. The reviewer raised a very good question. Right now, the authors have the same thought about the high DOC and DIC in these lakes, i.e., "the water loss rate by evaporation was high relative to in-lake loss rates of DOC and DIC, thus DIC and DOC are substantially more concentrated in lakes with a longer residence time (i.e., terminal lakes) than they are in lakes with a shorter residence time (i.e., "open" lakes as termed in this study)". However we are not ready to solve the problem for this question because we

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did not collect the necessary data for building a simple model to relate DOC and DIC concentration with water balance. Actually, we are recruiting team member in our laboratory with expertise on environmental hydrology, hoping he or she could strengthen our hydrological background. Also, our coming manuscript is trying to analysis the impact of climatic and hydrological conditions on DOC and DIC concentration and dynamics in these waters across the semi-arid and semi-humid region, in addition, the linkage between DOC and DIC and landscape pattern with each lake watershed will be considered as well. However, due to lack necessary data during the field campaigns carried out in 2011, we could not built the model to explain the mechanism for concentrated DOC and DIC in these brackish waters in the current study. We do know that these brackish waters are formed due to water flowing through high saline-alkaline soil around these lakes carries salt into these lakes, while there are not outflow, thus brackish water was form due to condensed salt along with DOC and DIC accumulation with evaporation.

(5) It is not clear that the study exhibits any spatial patterns in a regional sense, although this is suggested by the title and by Figure 1. Yes, there is an east-west gradient in sunshine hours but the difference is rather minor and the argument for its importance via photochemistry is not convincing. Rather, it seems like the key for brackish vs. fresh is whether the lake has an outlet or not; that seems like an individual lake characteristic rather than a regional one, or if there is a regional pattern this is not well described.

Response: The authors thank for the very instructive and valuable comments. Actually there is a spatial pattern, which might not be well interpreted in the previous version of the manuscript. Most of the brackish lakes distributed in the western part of the Songnen Plain, i.e., in the semi-arid region, while these fresh water lakes are mostly distributed in the eastern part of the plain, or along rivers, streams with outflows in the western part of the plain. So the pattern is that high concentrations of DOC and DIC are more often observed in the western part of the Songnen Plain, while low concentrations of DOC and DIC are dominant in the eastern part of the Songnen Plain.

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(6) Variation in primary production in the lakes could be very important in determining some of the patterns observed, but this is not developed.

Response: The authors thank for the valuable comments. Our study carried out in three productive inland waters, drinking water sources for Indianapolis, USA, indicated that primary production exerted strong effects on CDOM and DOC concentration (Song et al., 2013, submitted to Journal of Chinese Geographical Science). However, the authors did further analysis and found that no obvious spatial pattern for the DOC and DIC could be attributed to primary production through linking with Chl-a concentration in the Songnen Plain. The evaporative condensed effect overtook the pattern even though there was a contribution from primary production.

(7) The paper needs a fair bit of work in tightening the language; there are some problems with e.g. definite vs. indefinite articles and incorrect wording and grammar, though the meaning usually comes through. The bigger issue was a lot of the terminology was just not explained clearly, and is vague. For instance, it was not clear to me what was meant by “semi-humid/semiarid”. Does this mean part of the region is semi-humid and part is semi-arid? Or that the region is semi-humid during part of the year and semi-arid during the other part? Or is there a climate that is actually defined as the category semi-humid/semi-arid? This is a central idea of the paper, needs to be developed clearly to set up the study.

Response: The authors thank for the helpful comments. One native English speaker (Professor Lenore Tedesco from Indiana University-Purdue University, Indianapolis) has been asked to go through the revised manuscript, and additional information about some of the terminology has been added. In this case study, “semi-arid/semi-humid” means part of the region is semi-humid, while part of the region is semi-arid.

(8) In general all sections were longer than they needed to be, some quite a bit longer and the organization did not flow in a way that made the paper easy to follow.

Response: The authors thank for the comments. The authors made great efforts in the

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revised manuscript to keep it in a concise style.

(9) There was an abundance of speculation throughout, and while some level of speculation is appropriate for the Discussion section, it was also mixed in to the Results section.

Response: The authors appreciated the comments. The authors tried best to avoid speculation and particularly that appeared in the Results section; please see the detail in the revised manuscript.

Detailed comments (in addition to those listed above) 1. DOC/DIC is sometimes used to mean a ratio of the two, sometimes to just mean “both DOC and DIC”.

Response: The authors thank for the valuable comments. For the consistency, the authors have changed to “both DOC and DIC” for the revised manuscript, since the authors do not mean DOC/DIC ratio in this specific context.

2. “Semi-endorheic region”: not defined, and this is important

Response: Thank for the valuable comments. The authors have defined “Semi-endorheic region” in the revised manuscript. Here we mean that both endorheic and exorheic lakes exhibit in some part of the Songnen Plain, in this case we call this region as semi-endorheic region. We clarified this in the revised manuscript.

3. Introduction: too much text related to general importance of DOC and factors influencing C cycling; suggest focus on the issues that make this study unique: that is, info on inland brackish waters, endorheic systems and C cycling.

Response: The authors thank for the valuable comments. As suggested by the reviewer, the authors focused on the carbon cycling for inland brackish waters, particular in arid or semi-arid regions, thus some parts of the introduction without strong link with our case study were removed in the revised manuscript.

4. Suggest avoid use of subjective poorly defined categories in Results section. For

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instance, “Freshwaters exhibit low DIC concentrations (xx mg/L) while brackish waters exhibit extremely high DIC concentrations (yy mg/L).” These are only high or low relative to each other, so it would be better to say “Concentrations of DIC were higher in brackish (yy mg/L) than freshwaters (xx mg/L).

Response: The authors thank for the detailed comments. Suggestions have been adopted in the revised manuscript, thanks again for the helpful comments.

5. Results section should be MUCH shorter, there is a huge amount of text just listing numbers, this is not really helpful and instead I suggest the authors just refer the reader to the tables where data can be found, or to the figures. Only for a few key very important parameters should the numbers be repeated again in the text.

Response: Thank for the valuable comments. The authors have tried best to shorten the manuscript and kept it in a concise style.

6. Discussion is mixed into Results, for example p. 6569 lines 16-17, reference to Spencer et al. and Helm et al. findings.

Response: Thank for the valuable comments. The authors have reorganized some parts in the Results section to Discussion section.

7. Table 1 and throughout: suggest arranging lakes in order by salinity.

Response: Thank for the valuable comments. The authors have rearranged the data according to lake salinity, also DOC and DIC.

8. Table 1: what is the residence time of these lakes, this is crucial information?

Response: Thank for the valuable comments. Residence time is indicating how long it takes to renew the water in a specific lake. It is important in this case study since it indicates how long DOC or CDOM can be replaced with new arrival DOC or CDOM in a specific lake.

9. The terms NOF vs. OF, saline vs. fresh, open vs. terminal are all used and highly

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correlated, this serves to confuse things – suggest sticking mainly with one set of terms throughout.

Response: Thank for your valuable comments. The author have reorganized these terms and consistency has been achieved in the revised manuscript.

10. Table 4: To do an analysis it would make the most sense to use a time period of analysis similar to the residence time of the lakes, but here 1 month is used. Is that similar to residence time of water in the lakes?

Response: Thank for the valuable comments. All the lakes being investigated have much longer residence time; some of them even have residence time more than 30 years. A trial regression analysis were conducted in this study, and it turned out that climatic variables in one and two months before field surveys showed statistically significant relationship with DOC and DIC in this case study, thus climatic variables one month before field survey were determined for regression analysis.

11. Figure 1: Too small to be legible.

Response: Thanks for the comments; Fig.1 has been reproduced in the revised manuscript.

12. Figure 2, Figure 4: suggest removing these figures which show the individual lake values. The same data can be found in Fig. 3 and Fig. 5, summarized. Also it is not clear what the boxplots on Fig. 2 and 4 represent in terms of variation. Is this spatial variation within-lake on the day of sampling? If so, this is not particularly relevant to the issues raised in this paper.

Response: Thanks for the comments; Fig.2 and Fig.4 have been removed in the revised manuscript. These relevant values are summarized in Table 1 and Table 2. Yes, the spatial variation is within-lake on the day of sampling.

13. Fig. 8. Suggest remove. Also, what stats test were used, it is very surprising that these showed significant differences based on the way the graphs look.

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Response: Thanks for the comments; the authors have removed Fig.8 in the revised manuscript as suggested by the reviewer.

14. Fig. 9. This relationship between salinity and DOC, DIC is compelling and impressive. Suggest add explanation in caption about what is different between 2011 and 2012 samplings.

Response: Thanks for the comments; the authors have added explanations for Fig.9 in the revised manuscript as suggested by the reviewer.

15. There are many other details but the important place to start, I would submit, is with the more fundamental issues raised above.

Response: Thanks for all your valuable and helpful comments; the authors have tried best to address all these comments kindly raised by the reviewer.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6559, 2013.