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

Anonymous Referee #2

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Review – HESS-D, July 2013

Song et al., “Spatiotemporal characterization of dissolved carbon for inland waters in semi-humid/semiarid region, China.”

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

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[Interactive Discussion](#)

[Discussion Paper](#)



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Comment

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 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?
- (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 are not clearly written.
- (3) Along similar lines, statistical methods were not given.
- (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

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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.

(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.

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

(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.

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(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.

(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.

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". 2. "Semi-endorheic region": not defined, and this is important
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. 4. Suggest avoid use of subjective poorly defined categories in Results section. For 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). 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. 6. Discussion is mixed into Results, for example p. 6569 lines 16-17, reference to Spencer et al. and Helm et al. findings. 7. Table 1 and throughout: suggest arranging lakes in order by salinity. 8. Table 1: what is the residence time of these lakes, this is crucial information? 9. The terms NOF vs. OF, saline vs. fresh, open vs. terminal are all used and highly correlated, this serves to confuse things – suggest sticking mainly with one set of terms throughout. 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? 11. Figure 1: Too small to be legible. 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. 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. 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. 15. There are many other details but the important place to start, I would submit, is with the more fundamental issues raised above.

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

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