Biogeosciences Discuss.,
doi:10.5194/bg-2015-558-RC2, 2016

Biogeosciences
Discussions

Interactive comment on "Controls of longitudinal variation in δ^{13}C-DIC in rivers: A global meta-analysis" *by* K. A. Roach et al.

Anonymous Referee #2

Received and published: 14 February 2016

General comments:

This paper summarizes current knowledge on factors determining δ13C of DIC in stream water. As shown in many previous studies, the mechanism is highly complicated and the variables are usually inter-correlated. Furthermore, the effects are sometimes nonlinear and this seems to be why the authors adopted the GAMM. I'm not confident that this approach is valid because the mechanism lies on multi-scale (both in space and time), showing a hierarchical structure. In this viewpoint, structural equation modelling or path analysis may be better approach to deal with this type of analysis. If the authors can show a certain advantage of GAMM over a hierarchical approach, it should be explained in text. Another problem is that the authors did not quantitatively show the uncertainty in their results. I think the parameters are too many to explain the observed range in δ13C-DIC. Dissolved atmospheric CO2 and carbon-

ate bedrocks in particular show overlapping δ13C values (ca. $\sim$ 0 permil) in general. Therefore, it is usually difficult to estimate relative contributions of these isotopically similar endmembers to stream water DIC. The use of another isotope (e.g., 14C) may be one of the solutions for this "too many sources problem". For example, Ishikawa et al. (2015) Radiocarbon measured δ13C and Δ14C of DIC and tried to estimate their sources. Although there are not many Δ14C-DIC data available yet compared with δ13C-DIC, Δ14C-DIC may be useful for understanding potential controls of spatiotemporal variations in carbon isotopic compositions of DIC. Overall, I acknowledge the authors' effort for collecting the literature data, but the manuscript is not ready for immediate publication. Since this study is potentially important for the biogeochemical science, the authors should revise the manuscript especially focusing on my comments below.

Specific comments:

L.36: "altered" should be replaced with "determined"

L.53-54: "At isotopic $\sim$ and CO32-" Meaningless sentence so delete

L.55: "DIC(aq)" should be replaced with "DIC"

L.60: "For example $\sim$ δ13C-DIC" You forget to say groundwater DIC is generally 13C-depleted

L.70: "δ13C signature of DIC at isotopic equilibrium with the atmosphere" You already defined "δ13C-DICequilibrium" above so call it hereafter

L.103: Can you show working hypotheses of this study at the last paragraph of the Introduction? Then explain why you focused on each of the variables and how you expected the results

Results section should be re-organized because many topics are scattered and not in order

L.229: "Again, ~ nonlinear" This result is already reported in L. 209-211

Discussion section is relatively long so should be divided by several subsections

L.237: Start with main finding, not objective, of this study

L.254: "low surface:volume ratio" Needs more explanation. I expect high elevation (headwater?) streams are shallow in depth and narrow in width

L.261: "likely low" Remove "likely". Do you mean "near zero"?

L.266: "~ values also were low ~" Do you mean high DIC concentration is due to large proportion of carbonate dissolution?

L.268-270: "$\Delta\delta$13C-DIC ~ δ13C signature" A gap in logic. Carbonate (e.g., limestone bedrock) has higher δ13C value than atmospheric CO2. Weathering (dissolution) of carbonates provides high δ13C into water column. But note that dissolved atmospheric CO2 typically shows a similar δ13C value with that of carbonates

L.277-281: "Although algal ~ by algae" Unclear. The second sentence does not connect well with the first sentence

L.288-289: "The cycling ~ δ13C-DIC" This is a principal of your analysis and should be appeared earlier in discussion

L.289 ", thus ~" this statement is already mentioned just before this clause. Redundant

L.294-295: "Most lotic ~ the atmosphere" This is already mentioned above

L.295: "Therefore ~" Given CO2 in most streams is supersaturated, CO2 output should rise above input. You already mentioned that streams are source but not sink of CO2

L.300: "Our results also ~" Redundant. Is this because of carbonate?

L.303: "buffering capacity" What is this? Unclear

P.323: The authors do not directly answer the question here: why seasonal shift in

δ13C-DIC in high latitude is observed?

L.336: "Mayorga et al. (2005)" Another important contribution of this paper was that they measured radiocarbon (Δ14C) of DIC as well as other organic carbon fractions. I strongly recommend the authors also mention Δ14C because it can separate sources (e.g., dissolved atmospheric CO2 and carbonate bedrocks) that cannot be separated by δ13C. See also Raymond et al. (2004) Marine Chemistry and references therein

L.336-339: "Terrestrial C4 ~ low-water periods" But C4 plants have higher δ13C values than C3, don't they?

L.337: "terrestrial C3" Remove "terrestrial". Redundant

Fig. 3: Are panels A and B same? They look very similar

Figs. 4, 7, and their legends: Please explain how you calculated y axis (Contribution of covariate to smooth for $\Delta\delta$13C-DIC or δ13C-DIC)

Fig. 6: Seasonal pattern seems to be different between northern and southern hemispheres. Why? Just because of number of data?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-558, 2016.