

Interactive comment on “Dissolved inorganic carbon export from carbonate and silicate catchments estimated from carbonate chemistry and $\delta^{13}\text{C}_{\text{DIC}}$ ” by W. J. Shin et al.

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In this study, the authors tried to quantify, with many assumptions, DIC efflux from the groundwater and rivers and its possible variations with catchment lithology. The importance of carbon exchange processes in headwater regions is increasingly gaining attention, as the authors pointed out and therefore, this study may provide useful data and interpretation that have implications on related studies on this subject. However, a few uncertainties need to be clarified as presented below.

- p.9 line 22~line 28: Do you assume that the relationship between $\delta^{13}\text{C}$ changes and log unit change of excess pCO_2 can be applied to any conditions of degassing? Is

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there any established isotopic fractionation mechanism for this seemingly systematic change in $\delta^{13}\text{C}$? Also, does this quantification of the effect of degassing exclude the possibility of incorporation of C4-derived organic material?

p.10 line 14~line25: Is there possibility that photosynthesis and degassing affect $\delta^{13}\text{C}$ of DIC at different degrees with seasons? For example, photosynthesis could have been major during summer while degassing was more intense during winter.

p.12 line 1~7: Why not discuss the EC data of the silicate catchment?

p.14: The monsoon climate in east Asia significantly affects the hydrological condition and resultantly carbon exchanges as well. While the authors mainly interpreted the data assuming baseflow condition, can there be any notable change in carbon export during monsoon season with stormflow generation? Could this condition be additional uncertainty of the budget estimate in this report?

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