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**Hydrology and
Earth System
Sciences
Discussions**

Interactive comment on "Dissolved inorganic carbon export from carbonate and silicate catchments estimated from carbonate chemistry and $\delta^{13}C_{\mathrm{DIC}}$" *by* W. J. Shin et al.

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I think this is a significant scientific contribution, clearly written manuscript and deserves publication. I think it would benefit from some moderate and minor revision.

Comment: I think the conclusion that attribute the variability of d13C-DIC of the silicate spring to CO2 degassing needs to be back up from other evidence (Line 12, Page 1807 – Line 5, Page 1808) or other controlling mechanism needs to be sought. CO2 degassing commonly occurs under open-system conditions along stream flows, which is quite different in the studied silicate spring setting that locates at the bottom of the sandstone cliff. Authors argue that the slope of the regression line between d13C-DIC

and pCO2 agreeing with the increasing rate of d13C-DIC expected for CO2 degassing (Doctor et al., 2008) suggests the effect of CO2 degassing in the silicate spring. In my opinion, however, authors need to provide a discussion for the reason why the magnitude of degassing varies seasonally even though it maintains a closed system. It seems that the correlation between d13C-DIC and pCO2 can also be explained with the mixing between atmospheric CO2 (d13C-DIC of rainwater) and soil CO2 as indicated in the Figure 4a. Low d13C-DIC of summer season with enhanced microbial activity supports this possibility.

Reply: By taking the above comments, we will make following modification in the revised manuscript (section 4.1 and 4.3). First, we will clearly describe the possible degassing processes occurring both at the soil pores and the groundwater/stream water interfaces. Therefore, the degassing is considered as a process occurring at (semi-) open system (4.1). Second, seasonally varying effect of degassing will be discussed as the result of varying residence time in the soil pores associated with the precipitation regime changes (4.3). Third, two-component mixing model may not be appropriate since the water systems were mostly over-saturated with respect to atmospheric CO2. Therefore, mixing and exchanges with ambient CO2 are unlikely to occur at the studied spring water systems. We will add discussions on this possibility in section 4.1.

Comment: In section 4.3 (line 23 – 25, page 1809), authors attribute the seasonal variability of d13C-DIC in the silicate spring water to the effect of soil-derived CO2. In this view, sections 4.1 and 4.3 conflict each other for the cause of correlation between d13C-DIC and pCO2.

Reply: We consider that the main source of spring water DIC was the soil CO2 variably affected by degassing and the degree of degassing was affected by the residence time in the soil pore water which in turn affected by the precipitation regime. Therefore, both explanations do not contradict. However, we will add more explanation in the revised manuscript regarding this aspect to avoid confusion.

Comment: Doctor et al. (2008) is not listed in the reference list. Also note that it is published in 2008 not in 2007.

Reply: We will add the reference and correct the year in the revised manuscript (Doctor, D. H., Kendall, C., Sebestyen, S. D., Shanley, J. B., Ohte, N. and Boyer, E. W. (2008), Carbon isotope fractionation of dissolved inorganic carbon (DIC) due to outgassing of carbon dioxide from a headwater stream. Hydrological Processes, 22: 2410–2423. doi: 10.1002/hyp.6833).

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