

Interactive comment on “Silicate weathering and CO₂ consumption within agricultural landscapes, the Ohio-Tennessee River Basin, USA” by S. K. Fortner et al.

Anonymous Referee #1

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The manuscript presents a study about silicate weathering and CO₂-consumption within agricultural landscapes for selected catchments in the Ohio-Tennessee River Basin.

To analyse this, dissolved silica (DSi), Ca, Mg and NO₃ as well as alkalinity were measured. Five catchments have been sampled: 1) a forested site 2) a mixed agricultural site 3) an unimproved pasture site 4) a tilled corn site 5) a converted no-till corn-field site

The identification of some positive correlations between the molar ratio (Ca²⁺ + Mg²⁺)/alkalinity to DSi

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at the sites is interpreted as the loss of alkalinity due to nitrification.

In general, the study builds up on a number of studies analysing the effect of abundant conservative anions like NO₃, Cl, or SO₄ on CO₂-consumption-rates by chemical weathering. However, mostly US-American studies are referenced, and studies from Europe or East Asia showing similar effects are less addressed/referenced.

The manuscript could be strengthened by comparing their results with results from other sites in the US or globally, affected or not affected by the analysed effect. This would allow the reader to better understand the relevance of the identified CO₂-consumption-suppression rates or the rates of CO₂-consumption.

For example is the identified CO₂-consumption regionally/globally high or low, or are the identified nitrification rates high or low, etc.

Here, in this manuscript, the focus is on nitrification, which is reflected by the chosen parameters being measured. Other conservative anions or strong acids are not considered. In addition Na and K, important cations are not measured, either.

Because of this it is difficult to assess the quality of the samples, by standard techniques like the ion balance. The usual application of geochemical evaluation techniques is with this data set not possible.

The missing of the named parameters does at the first glance not allow for the evaluation of a bias due to the missing ions.

It is not well justified, or I missed it (?), why the other major ions have not been measured, despite their influence on the analysed carbonate system.

In addition, it was not sufficiently tested if trace carbonate or even abundant carbonate would influence the analysis. May be they are not abundant?

In addition, the potential influence of atmospheric deposition (cations, anions, etc.) on the findings is not sufficiently discussed.

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The raw data and the regression results should be provided in the appendix or the supplement information, to allow in the future for comparing with the presented findings.

I would suggest using rather km² than ha. This allows easier comparison with the literature addressing CO₂-consumption, and is the more modern unit to be used. The text could be rearranged for improving readability.

Considering the readership and the scope of Biogeosciences, I recommend to extend the discussion beyond the Ohio River basin and/or to explain more straight “What is new” and “Why is it important”.

Interactive comment on Biogeosciences Discuss., 8, 9431, 2011.