

## ***Interactive comment on “Aggregates reduce transport distance of soil organic carbon: are our balances correct?” by Y. Hu and N. J. Kuhn***

**Y. Hu and N. J. Kuhn**

yaxian.hu@unibas.ch

Received and published: 28 August 2014

Dear Referee:

Thank you very much for your time and comments. Your suggestions are appreciated and helpful to improve the manuscript. Below are our replies to the individual questions.

(1) One of the few ways I think this paper could be improved would be by including a slightly more involved explanation of the Equivalent Quartz Sizes approach and the settling tube apparatus. The authors appropriately cite their earlier work introducing these techniques and I understand their (or the journal's) reluctance to include extraneous text. However, it is difficult to understand what the authors did without reading the Hu et al 2013b paper.

C4755

Answer: We agree with you that a brief description of the settling tube apparatus is necessary to elaborate the experimental rationale. Relevant parts in the Method will be changed as following:

“A 1.8m settling tube (Fig. 1b) was used to fractionate the eroded sediment fractions according to their respective settling velocities. The settling tube apparatus consists of four components (Figure 1a): the settling tube, through which the soil sample settles; the injection device, by which the soil sample is introduced into the tube; the turntable, within which the fractionated subsamples are collected; and the control panel, which allows an operator to control the rotational speed and resting/moving intervals of the turntable. Details about the settling tube apparatus were described in Hu et al. (2013b).”

“Six particle size classes, based on the concept of Equivalent Quartz Size (EQS) described in Hu et al. (2013b), were selected according to their likely transport distances after erosion (Table 2). EQS represents the diameter of a nominal spherical quartz particle that would fall with the same velocity as the aggregated particle for which fall velocity is measured (Loch, 2001).”

(2) I was somewhat skeptical of the extrapolations the authors made from their findings from one experiment to the global impact of erosion and deposition. However, they were careful to note the shortcomings of such projections. For example, they note in the Methods section that although they used a suitable soil for the purposes of the study, more soils need to be investigated. In addition, they discuss that the respiration rates of the soil fractions were estimated from very brief observations, but the authors note that these are instantaneous respiration rates that don't represent long-term decomposition processes. It might be seen as a weakness, but the fact that they are even considering how aggregate protection plays into the carbon balance of erosion is a strength of the paper.

Answer: Thank you very much for thorough reading this manuscript and your generous

C4756

appreciation.

(3) Section 3 L15–20: Can the authors provide evidence that SOC in the various classes didn't differ over time? E.g. means and variation for the time points or a P value if this was explicitly tested?

Answer: Both the weight distribution and EQS specific SOC content did not significantly differ across the six sediment collection intervals over rainfall time (ANOVA, single factor,  $P > 0.05$ ,  $n = 18$ )

(4) Technical concerns Page 8839 L6: typo in “eroded”

Answer: Typo will be corrected in the revised manuscript.

Yaxian Hu and Nikolaus J. Kuhn

---

Interactive comment on Biogeosciences Discuss., 11, 8829, 2014.

C4757