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

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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) There are several grammatical errors and inconsistencies. For the purposes of further scientific discussion, I won't itemize this, but please carefully edit the paper.

Answer: Thank you for your notice. The manuscript has been carefully edited, and you will find the minor corrections in the revised manuscript.

(2) The use of only the first 10 minutes of run-off: Why run out to 30 minutes to ensure

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full breakdown, if you aren't going to study the most difficult-to-destroy (ie, most stable, potentially most C) aggregates: What was the point of separately pooling all the fractions captured in that last 20 minutes while considering a detailed fractionation of the first 10 minutes? Is slaking not a function of time? And wouldn't slaked aggregates release differently reactive C?

Answer: We understand your concerns on the temporal variation of aggregate breakdown over rainfall time. The runoff and sediment suspensions were collected every 30 min throughout the simulated rainfall events. Within each of the 30 min intervals, only samples from the very first 10 min were used to carry out settling fractionation, limited by the volume of the injection device. In total, there were six sediment collection intervals over the 3 hours rainfall events, and a settling fractionation test was carried for each of the six sediment collection intervals. Therefore, any potential breakdown of the aggregates during the simulated rainfall was not ignored, but examined through the consecutive sampling.

In addition, the possible changes of slaking effects, as concerned by the referee, did not differ significantly over rainfall time (ANOVA, single factor, $P > 0.05$, $n = 18$). Therefore, the variations of the proportional composition of eroded sediments were on purpose simplified to accentuate the differences across EQS size classes.

(3) You reported that 95% of the sediments settled during your 1-hour pre-treatment. But this was not a random sampling of 95% of the particulates. The remaining 5% are a fraction sharing the common trait of low density/settling velocity; it is entirely possible and consistent with your own hypotheses that this 5% features uniquely in the C accounting you are attempting to resolve. This suspended fraction appears to be retained and analyzed, but it was added back with the finest settled fraction. Why?

Answer: The collection beakers used in pre-treatment had height of 20 cm, which, in theory, were short settling tubes as compared to the 1.8 high settling tube apparatus. Hence, the pre-treatment of settling in the collection beakers can be considered following the same theory of settling velocity. Thus, this pre-treatment was designed to save the time for the fine fractions from settling over long time in the 1.8 m settling tube. The supernatant and remaining suspended sediment after 1 h of settling in the collection beakers corresponded to EQS < 8 μm. Fine suspended fractions are considered as one group "export" out of the terrestrial system. Hence, the fine fractions of EQS < 8 μm from the pre-treatment were added back to the fractions of EQS < 20 μm suspended in the 1.8 high settling tube, for the sake of terrestrial or aquatic perspective. Where settling tubes of different lengths are applicable, sediment fractions of distinct settling velocities should be separately treated.

(4) One flaw in your arguments (to me), is that the C associated with the different EQS fractions is not all the same in terms of decomposition risk. As you make the link from C stocks to fluxes, instantaneous respiration from a subfraction is not likely representative of the respiration observed at the deposition site. Some discussion of the C forms and stabilization mechanisms is needed to put the predictions of gas fluxes in a more relevant context.

Answer: We agree with your concerns on the quality and the mineralization susceptibility of SOC stored in various sediment fractions. The instantaneous respiration only partly reflects the mineralization potential of the eroded SOC. The scope of this study will be clarified in the revised manuscript. Future investigations, such as determining the SOC quality using isotope, and monitoring the long-term respiration potential of different SOC fractions, are required to quantify the net effects of erosion on global carbon cycling.

(5) How can you use the classic water-stable aggregate profile for this soil to tie this experiment to broader experiments in the literature? The WSA data you provide in soil characterization isn't really used. The calculations of percent change against various denominators are loosely discussed.

Answer: The aggregate stability of the original soil showed that 67% of the soil frac-

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tions were stable aggregates > 250 μm (Table 1), whilst the settling velocities of the eroded sediments indicate that only 4% of the fractions were of EQS > 250 μm (Figure 2). Such remarkable differences between the two fractionation methods are partly induced by the bias of using Stokes' Law to estimate the settling velocity of coarse aggregates. But more importantly, the differences clearly show that aggregates experienced sufficient breakdown during the simulated erosion events. This, furthermore, suggests the relevance of using the settling velocities of actual aggregated sediment to estimate the likely transport distances of eroded sediments rather than texture or any arbitrary stress induced when measuring aggregate stability.

(6) I'd like to see the calculations presented more crisply. And how did you make the leap from "approximately 41% of the eroded SOC from the silt loam used in this study would be re-deposited along eroding hill slopes" to "Our data show that 41% of the eroded SOC from a silty loam was incorporated into aggregates of EQS > 63_m"? I missed that connection.

Answer: According to the conceptual model developed by Starr et al. (2000) (Figure 6), the six EQS classes can be further grouped into three separate groups, each with a different likely fate: EQS < 20 μm would be likely remain suspended in runoff and hence, transferred to rivers, and all EQS > 63 μm would be re-deposited along eroding hill-slopes (Table 2). The intermediate EQS of 20 to 32 μm and 32 to 63 μm can have either fate, depending on localised flow hydraulics. Figure 3b shows that approximately 41% of the eroded SOC from the silt loam used in this study was stored in aggregates of EQS > 63 μm. Therefore, according to the conceptual model described above, approximately 41% of the eroded SOC from the silt loam used in this study would be re-deposited along eroding hill slopes (Figure 7b).The clarity the figures presented in our paper will be improved in the revised manuscript.

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