

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

Anonymous Referee #7

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This paper approaches a classic observation – C stabilization as a consequence of soil destabilization – and tries to determine the degree to which the physical structure of the soil controls the amount of C transported, and its contribution to soil C stocks and gas fluxes. This is an important, and probable uncertainty. I have some concerns and questions, which I would like to see the authors respond to.

Concerns: 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.

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

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minutes? Is slaking not a function of time? And wouldn't slaked aggregates release differently reactive C?

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?

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.

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. 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 μ m"? I missed that connection.

Finally, the observation that the finer EQS fractions had instantaneous respiration rates lower than the original soil, while the other fractions had higher rates is fascinating, and suggests further study is needed into how the C in those fractions is protected. This is a nice demonstration that could powerfully inform how these stable, fine aggregates

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protect C over periods of decades to centuries.

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