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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

Anonymous Referee #1

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The manuscript by Hu and Kuhn reports the results of an experimental study on the role played by soil aggregates in the fate of eroded soil organic carbon. This is an interesting and well written article, well referenced and with a topic that is well suited for the BG journal and that adds new insights into the current controversy about the role and impact of soil erosion in the C cycle. The authors use a rainfall simulation experiment on a 150 cm x 80 cm flume filled with a silty loam soil to test the validity of their hypothesis about the role of aggregates in sink/source issue. Overall, the results provided are an important contribution to the ongoing scientific debate but although the experimental setup limits their extrapolation to the landscape and global scale, the authors insist on quantifying the consequences of their observations coming from a single soil type and a single rainfall intensity application. For instance, the main contribution of the manuscript is the finding that particle aggregation reduces the transport distance of

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eroded SOC, resulting in 41% of eroded SOC likely redeposited along hillslopes. This data, together with an interesting reasoning in section 4.2, leads them to conclude that “a risk of overestimating lateral SOC transfer exists when mineral grain size rather than actual size of aggregated sediment is applied in erosion models” and to quantitatively show what that would imply for global estimates of the potential C sink. As the authors already acknowledge, the data provided is very limited data for such large statements, nevertheless, they devote a large part of their discussion to discuss the global implications of their findings. Thus, I would suggest the authors to strongly reconsider the focus of their discussion and put their efforts into explaining the mechanisms behind their observations rather than drawing global conclusions with large uncertainties (not even quantified). In addition, the authors report that there were no differences in the particle size distribution of soil and sediments, contrary to what has been observed in other laboratory studies and field experiments. Given the fact that particles in the flume are moved by interrill erosion, my guess would be that the flume might not be long enough for redeposition of large particles to occur during the transport phase and, thus, a selective transport of fine particles further on. On this behalf, were there signs of sediment deposition along the flume (and not only at the bottom collection point?) How would having a larger flume might have changed your results? Sediment arriving at colluvial sites in agricultural landscapes might often have traveled very large distances. In addition to this, I would also suggest the authors to look at the effect of sediment reaggregation in future experiments. In relation to the respiration measurements, it would be interesting to have some insight into the quality of the SOC within each aggregate size class. Could smaller size particles respire less due to the fact that they contain older or more ‘recalcitrant’ SOC? Altogether, this paper could represent a valuable contribution and be published in BG, but a major revision is needed to refocus the discussion. Adding some explanation on how the methodological constraints which make an extrapolation of the results undesirable (p.e. slope gradient of 15%, leveling of the surface, . . .) might have affected the results would also contribute to strengthen the manuscript.

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Several specific comments:

(1) Can you show the standard deviation of the original soil as well in Figures 3, 4, 5? It is difficult to tell otherwise if the observed differences are relevant or not. In relation to figure 4, if most CO₂ comes from the fine fractions (due to their total mass) part of this is at the same time related to the loess soil you are using, which does not reflect global soil diversity (in conflict with your extrapolation to the global scale). At the same time, where is the burial effect taken into account there? SOC mineralization is physically constrained by burial. How could this change your results? (2) The authors question in a couple of occasions the often reported association of SOC with mineral particles. They show that while 61% of the sediment fractions were in EQS of 32-125 m, containing 65% of SOC. However, the difference between 61% and 65% does not seem enough to support the affirmation that SOC is not associated to mineral particles. (3) Section 4.1 is weak and not supported by any references other than a previous experiment from the same authors. I suggest to either integrate this as part of the results or provide additional discussion and contrast with results from other studies. (4) In page 8832 lines 18-19 explain these “diverse impacts”. (5) In relation to the incubation, did you take into account the effect of re-wetting on the CO₂ initial pulse?

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