

Interactive comment on “Large fluxes and rapid turnover of mineral-associated carbon across topographic gradients in a humid tropical forest: insights from paired ^{14}C analysis” by S. J. Hall et al.

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This well written manuscript presents a study demonstrating that in humid tropical forest soils with varying texture and reactive metal concentrations, O_2 availability was the only factor that could explain variations in soil carbon turnover time. This is an important finding in that it suggests a hierarchy of controls on decomposition and that factors which directly limit heterotrophic microbial activity are more important than factors which just retard organic matter availability.

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While this finding is important, I have a major reservation about the methodological approach – the authors use time series radiocarbon measurements interpreted using a steady-state two-pool model which is a very powerful way of assessing decadal scale turnover time. However, instead of matching the 1988 values with the samples collected in 2012 along the different topographic positions (which represent 3 different soil orders) they have chosen to average the 1988 values in the modeling. The reason for this averaging was never explained and it seems to invalidate the importance of looking for differences in carbon cycling along the toposequence.

A lag time in the model should be considered for the 10-20 cm samples as it is highly unlikely that the current year atmospheric $^{14}\text{CO}_2$ value is being directly transferred into the C in this soil layer. This may perhaps help constrain the model for the samples where the model struggled to find a solution.

The authors make a point to say that it is important to focus on multiple pools within measured fractions versus just the bulk sample and they have focused on the mineral-associate pool in this study. However, is a fraction that contains nearly 90% of the organic matter really a distinct fraction from the bulk OM? Is there perhaps a more meaningful fractionation method for these soils?

I would suggest that the entire section on comparing one-pool versus two-pool model results be dropped. This point has been made in numerous papers and it seems to detract from the main focus of this one.

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