

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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We thank Jon for his thoughtful comments. The original text is shown in italic below, and our response in normal typeface.

This well written manuscript presents a study demonstrating that in humid tropical forest soils with varying texture and reactive metal concentrations, O₂ 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

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which directly limit heterotrophic microbial activity are more important than factors which just retard organic matter availability.

Thanks for the interest in our work.

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.

This is an important point that we will clarify in the revision. Irrespective of the 1988 data, our 2012 data show that there were no differences in $\Delta^{14}\text{C}$ values across the topographic positions, implying that there was no systematic topographic variation in mineral-associated C turnover times. Thus, parsimony would suggest that there was similarly little topographic variation in ^{14}C values in the 1988 samples. As stated in the Methods (p 899 line 26-27), we have only four samples from 1988, two each from valleys and slopes, respectively. Previous paired ^{14}C studies have relied on similar or even sparser replication for comparing sites over time (e.g. Trumbore et al. 1996, Science). This is due in part to the substantial costs associated with accelerator mass spectrometer analyses. Here, we use the 1988 samples to constrain realistic turnover times of 2012 samples in a two-pool model, acknowledging the uncertainty inherent in pairing ^{14}C measurements over time given spatial heterogeneity. Given the observed variability among ^{14}C values from the 1988 samples (indicative of spatial heterogeneity but not necessarily related to topography), simply averaging the two values for each position to derive a mean value by position could bias the model results. Furthermore, this would not allow us to model ridge samples, given that archived samples for this position could not be analyzed for this study.

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Thus, for our modeling, we combined the four 1988 $\Delta^{14}\text{C}$ values and sampled from a normal distribution defined by their observed mean and standard deviation to serve as a constraint in each of 1000 model realizations for each 2012 sample. This approach allows us to empirically assess the impact of varying the assumed 1988 $\Delta^{14}\text{C}$ values on the modeled parameters of interest (slow pool turnover time and fraction slow pool). As we mention in the text (p 908 1:7), there was relatively little impact of varying assumed 1988 $\Delta^{14}\text{C}$ on our modeled parameters; varying 1988 $\Delta^{14}\text{C}$ across this distribution generated a typical standard deviation of 4 years in the modeled slow pool turnover time (relative to a mean value of 20 years). Thus, we feel justified in this approach to dealing with uncertainty in 1988 $\Delta^{14}\text{C}$.

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.

This is a relevant point that we will clarify with additional text and modeling in the revision. Fine roots and DOC are likely to be a major if not primary source of C inputs to the 10 – 20 cm depth increments, as opposed to particulate leaf litter (e.g. Rasse et al. 2005, *Plant and Soil*, 269:341–356). Fine roots are abundant in this depth increment (Table 2), and fine roots and DOC should have a predominantly recent ^{14}C signature (within 1 – 3 years of atmospheric values), especially given the extremely rapid root turnover and decomposition of buried roots and surface leaf litter in this ecosystem (mean residence times of 0.9 and 0.8 years, respectively; Cusack et al. 2009, *Global Change Biology*, 15:1339–1355). We note in the manuscript that our present turnover estimates are conservative, in that the presence of multiple-year lags in C inputs would decrease modeled turnover times (p 911 24:27). Modeled ^{14}C dynamics in a different tropical study adopted a similar approach of reporting conservative turnover times and acknowledging the possibility of lags, which would further decrease turnover times (e.g. Marin-Spiotta et al. 2008, *Geoderma*, 143:49-62). However, in response to your

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comments, we will explore the impact of imposing a relatively short (three-year) time lag on modeled slow pool turnover times and the fraction of slow pool C.

Unfortunately, adding a lag to the ^{14}C model further exacerbates the problem of fitting model solutions to samples with $\Delta^{14}\text{C}$ less than the 2012 atmosphere. Adding a lag time of five years, for example, further decreases the modeled slow pool turnover times to unrealistic levels (e.g. slow pool turnover times of 6 years for a sample where 2012 $\Delta^{14}\text{C} = 30\%$). These short turnover times are incompatible with measured NPP. As discussed in the response to Troy Baisden's review, the major problem with modeling the samples with low $\Delta^{14}\text{C}$ (5 of the 30 2012 samples) appears to be idiosyncratic spatial variation. This heterogeneity is represented to a minor extent in the 2012 samples, but not in the 1988 samples, likely due to the differences in sample size. It is unsurprising that the four samples from 1988 did not exhibit the same degree of heterogeneity of turnover times as did the 30 samples from 2012. For example, the same samples that had low ^{14}C values in 2012 likely also had low ^{14}C in 1988, yet we do not have an appropriate match in the 1988 data.

Thus, we are left with a situation where we need to estimate two parameters (slow pool turnover and fraction slow pool) without the constraint of a reasonable 1988 ^{14}C value. One approach would be to assume that slow pool turnover is similar to the other samples, and that the fraction of the slow pool declined (i.e., ^{14}C was dragged down by the passive pool as noted in the review by Baisden). This is the approach we used in the original manuscript, as this seems most ecologically reasonable. Alternatively, we could assume that the fraction of the slow pool is similar, but that slow pool turnover decreased. Most likely, there would be a combination of both processes, but unfortunately we cannot evaluate this with the present data, rather only bound the potential changes to slow pool turnover and the slow pool fraction.

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% organic

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matter really a distinct fraction from the bulk OM? Is there perhaps a more meaningful fractionation method for these soils?

We will further justify our approach in the revision. We contend that separating mineral vs. particulate soil fractions by density can be quite useful and meaningful in the case of ^{14}C modeling, because one can remove particulate organic matter with predominantly rapid turnover times (i.e., less than a year) which has a dramatically different isotopic signature than the remaining mineral-associated C. Because of the impact of this recently-fixed C, mineral-associated ^{14}C values are likely to be distinct from bulk soil ^{14}C even in soils with little particulate C, which is important for subsequent ^{14}C modeling.

Others have shown that further separating mineral soil ($> 1.8 \text{ g ml}^{-1}$) by density can yield fractions with differing ^{14}C values (e.g. Sollins et al. 2009 Biogeochemistry, 96:209–231, Giardina et al. 2014, Nature Climate Change, 4:822–827). However, these fractions can still be difficult to interpret in terms of modeled C pools, as they are unlikely to represent pools with uniform turnover times. This point is echoed by Baisden and Canessa (2013), who state that: “the separation of a light density fraction at typical densities of $1.6\text{--}1.7 \text{ g cm}^{-3}$ or particulate organic matter by sieving appears highly useful, but that larger ranges of density fractions consume many ^{14}C AMS measurements with less benefit.” For example, Telles et al. (2003, Global Biogeochemical Cycles 17, 1040) found significant bomb ^{14}C in acid hydrolysis residue of heavy soil fractions, showing that even C that is highly stable to chemical degradation can have a significant fast-cycling component, and thus cannot be treated as a uniform pool in C cycling models.

An alternative approach to physically separating multiple pools of mineral-associated organic matter is to model these pools mathematically using multiple timepoints, as we did in the present study. The long time-series studies of ^{14}C presented by Baisden et al. (2013) suggest that a two-pool model can describe long-term C dynamics quite well.

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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.

Point well taken that other studies have commented on one-pool vs. two-pool models (e.g. Trumbore 2000, Ecological Applications 10:399-411), but few have done so in the context of interpreting mineral-associated C turnover. We think the one pool vs. two pool distinction for mineral-associated C is quite important in that numerous recent studies (cited in the manuscript), including some in the Biogeosciences journal, have continued to interpret mineral-associated C as a single pool. As we show, this has potentially serious consequences for interpreting C turnover times. The other reviewer (Troy Baisden) was also supportive of including this section, stating that “I think this point deserves emphasis because there are still ongoing attempts to publish new single-pool residence times and to restate rather than reinterpret the potentially erroneous results and conclusions involving single-pool residence times already in the literature.” In light of the comments from both of you, however, we will condense our critique of single-pool models and incorporate additional discussion of insights from more robust multiple-pool models.

Interactive comment on Biogeosciences Discuss., 12, 891, 2015.

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