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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 Troy for his thoughtful and thorough review. The original comments are shown in italic below, and our response in normal typeface.

This manuscript presents a valuable new C-14 dataset that develops well-constrained soil C turnover estimates for a tropical forest site in Puerto Rico across slope positions, and integrates these with understanding of soil redox conditions. The C-14 dataset is made particularly valuable by the combination of replicated samples at both 1988 and

2012, which make it possible to follow the 'bomb C-14 spike' through the soil C fractions isolated in the study. As the authors point out, the resulting soil C turnover estimates have considerable value globally for understanding the nature of soil C cycling (with quantitative uncertainty estimates) and its relationship to redox. Overall, this is a well-written, topical and useful paper. I suggest only minor revisions.

We appreciate your interest in and support for our work.

The main thrust of the paper where I would suggest revisions is the way in which the two pool model is emphasised. I've been a strong advocate that single pool models yield misinterpretation of residence times and should be used with great caution. I'm pleased therefore to see Baisden et al (2013) used to emphasise this conclusion. 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. But I think the the amount of space used for this emphasis itself can bere-duced, and the emphasis better underscored by incorporating some discussion of other studies that have good soil C-14 data and modelling. Given that at least some studies have been undertaken in the temperate and boreal zones, this work now enables some integration of robust turnover estimates across the global range of soils and climate. Such a discussion would allow less emphasis on previous work that is essentially being criticised as 'single-pool' residence times, and more on what we learn from improved data and methods.

Agreed. Given the substantial number of recent papers (cited in the manuscript) that continue to interpret mineral-associated C turnover in terms of a single-pool model, we think that it is important to emphasize the potential pitfalls of that approach. However, we will condense our critique of single-pool models and incorporate additional discussion of insights from more robust multiple-pool models.

Here, I provide some suggestions what a paragraph or two bringing the soil C turnover *estimates into a global context might include. (I note two pieces of my own recent work intended to highlight or demonstrate how C-14 can be better connected to rates of C cycling.) First, if the author's narrative wants to begin by outlining assumptions that undermine some single-pool single-time-point C-14 turnover calculations, then Baisden and Canessa (2013) may also be useful to cite.*

Yes, Baisden and Canessa (2013) is relevant here and we will include this ref.

Methodologically, it is useful to point out that relatively few studies of time-series C-14 have been carried out in forests on colluvium – because forests soils and soils on colluvium are inherently more variable than grassland (or cropland) soils and soils on alluvium or loess, etc.

Agreed, we will emphasize this point; this is important in light of the small proportion of samples with very low Δ14C which raised difficulties during modeling.

Schrumpf and Kaiser (2015) had a useful recent paper that illustrates the problem, but confirms that resampling periods of at least 10 years and considerable replication are likely necessary. But the long resampling interval (24 years) in this work is not entirely required.

Agreed; the resampling interval used here was opportunistic based on the availability of paired archived samples. However, this interval is useful in that it corresponds to a period of greater atmospheric 14C, and thus greater sensitivity, than if a narrower time interval were used.

Regarding the use of NPP to make sense of the flow of C through the soil pools in the C-14 model, Baisden and Keller (2013) introduced the term "synthetic constraint' for this approach, which is now well handled by SoilR 1.1 (Sierra et al 2014).

Here, we did the converse and used soil C turnover times to make inference about the partitioning of NPP to the mineral-associated C pools at steady state. Despite its value, we do not have the high temporal resolution NPP data that would be necessary

for duplicating the approach of Baisden and Keller (2013).

It may be worth commenting more solidly on the consequences of considering a third fast cycling pool as in Baisden and Keller (2013)?

Agreed; there is good evidence that a small portion of mineral-associated C is even more highly dynamic (i.e., turnover of months – several years), and thus a three-pool model would be interesting to test. We will include text to that effect. However, we could not fit parameters for a third pool using the two available 14C timepoints. Also, density separation removed most of the material (< 1.8 g ml-1) that would presumably comprise the fastest-cycling pool. Finally, Baisden and Keller (2013) note the poor sensitivity of 14C for resolving pools with turnover times < 2 years.

Moving to understanding of global patterns where robust two-pool models have been applied to fractions or bulk soil C-14 time series, it may be useful to recognise that Baisden et al 2002 is carried out in a xeric grassland chronosequence, but one where the oldest sites are so highly weathered and poorly drained that they may provide a useful comparison to the tropical sites such as those reported by the authors. I would also encourage the authors to include C turnover from Harvard forest (Gaudinski et al 2000; Sierra et al 2012; Sierra et al 2014) if possible (given the different but still robust method). Froberg et al (2011) provides a good example of a boreal organic soil in which mineral associated organic matter is not present and a single pool model works across time-series C-14 measurements.

Agreed; we will synthesize these studies in the revised manuscript.

An obvious discussion point is that there seems to be relatively little variation in mineral-associated organic matter turnover globally, but that non-mineral-associated organic matter follows the expected temperature response from Arrhenius relationships.

Yes, we agree that this is an important point to emphasize.

The only other major issue I see is with the clarity of how the modelling and uncertainty *analysis was carried out. I cannot verify exactly what was done, but if I've interpreted it correctly, I think I agree with all the steps. The main problem with lack of clarity appears to be mixing the description of the modelling method with the method for estimating uncertainties/errors. I recommend rewriting the 2 paragraphs beginning at L5 on P903 and considering restructuring to describe the modelling for a best estimate first, and then the uncertainty second.*

We will further clarify our modeling approach in the revision. In this paper, we have actually combined model fitting with the sensitivity analysis in order to avoid assuming a single passive pool turnover time and 1988 14C value for each modeled sample. This is a useful approach because the mean of a non-linear function evaluated across a range of probable values (i.e., passive pool turnover times varying from 100 – 1000 years) does not equal the value of a function evaluated at the mean of those probable values (ie. 550 years). For example, rather than simply assuming a single value for passive pool turnover, as has been done in previous studies, here we assumed that passive pool turnover varied randomly between 100 and 1000 years, and fit the model for 1000 different randomly chosen turnover times. Then, from the distribution of modeled parameters, we were able to calculate mean parameter estimates and their uncertainty (standard deviation) simultaneously. These mean parameter values are subtly distinct from what we would have estimated using a single assumed passive turnover time (i.e., 550 years).

This issue is highlighted by the other reviewer(Jon Sanderman), but my interpretation is somewhat different in that I think the 1988 data was used appropriately to estimate a mean for each landscape position, and the more extensive 2012 data used to estimate the uncertainty around this mean and ultimately estimate uncertainty in calculated turnover times. If this is the case, I think that's appropriate, but the methods should be further clarified.

This is an important point that we will clarify in the revision. As stated in the Methods (p 899 line 26-27), we have only four samples from 1988, two each from valleys and

slopes, respectively. Given the substantial variability among 14C values from the 1988 samples, simply averaging these two values to derive a mean value for these positions could bias the model results, and would not allow us to model ridge samples, because we were unable to analyze samples for this position.

The results from 2012 samples provide some useful insight here. Given that Δ14C did not differ among topographic positions in 2012, it is parsimonious to assume that it did not differ in 1988 either. Thus, we combined the four 1988 Δ14C values, and sampled from a normal distribution defined by the observed mean and standard deviation to serve as a constraint in each model realization. This approach allows us to assess the impact of varying the assumed 1988 Δ14C values on the 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 Δ14C on our modeled parameters; varying 1988 Δ14C across this distribution generated a typical standard deviation of 4 years in the modeled slow pool turnover time. Thus, we feel justified in this approach to dealing with uncertainty in 1988 Δ14C.

There's no need for italicized emphasis about 'absolute' individual comparisons when sites can't be exactly located 1/4 of a century later. It seems that the uncertainty method is acceptable/appropriate, although it is by no means the only appropriate approach.

Agreed; we included this section in response to one of the preliminary reviewers for Biogeosciences, who was skeptical of the validity of a paired 14C approach given the inherent spatial heterogeneity of soil C.

The decision I take issue with is choosing not to model points withΔ14C below the 2012 atmosphere. This may introduce some bias - which should be avoided. I point out that these results are not unexpected given acceptance of a two pool model where the bulkΔ14C is dragged down by the presumed millennial pool. They mostly occur in the deeper horizon, which is to be expected. Keep in mind that an alternative presented *in Baisden (2013) is to mathematically recombine the horizons before modelling the C turnover.*

We will clarify that we did in fact model the points with $\triangle$14C less than the 2012 atmosphere (5 of the 30 samples from 2012), only that we had to modify our model assumptions to do so, and could only estimate one of the two parameters that we had modeled for the other samples as opposed to modeling both parameters at the same time. For the revision, we will present alternative model estimates for slow pool turnover and fraction slow pool in these samples. Both of these parameters could not be realistically constrained simultaneously using the observed mean and variation of $\triangle$14C from the four 1988 samples, given that they yielded turnover times (and C inputs) that were ecologically unreasonable. 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. In 2012, a small portion of the landscape appears to have either slower C turnover, or less slow-cycling C, or both, which was not represented in the 1988 samples.

Thus, we are left with a situation where we need to estimate two parameters (slow pool turnover and fraction slow pool) using only one constraint, 2012 $\triangle$14C. 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., 14C is being dragged down by the passive pool as stated above by the reviewer). This is the approach we used in the original manuscript, as this seems most ecologically reasonable. Alternatively, we could assume that the fraction 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 fraction slow pool.

We agree that mathematically combining the horizons is a useful strategy in many situations, but this would not escape the fact that the measured 1988 values for 14C are still not apparently representative of these particular 2012 samples. Combining the

horizons in this case would artificially decrease slow pool turnover times if we were to assume that the same 1988 values hold.

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