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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 $^{14}\text{C}$ analysis” by S. J. Hall et al.***

**T. Baisden (Referee)**

t.baisden@gns.cri.nz

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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

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quantitative uncertainty estimates) and its relationship to redox.

Overall, this is a well-written, topical and useful paper. I suggest only minor revisions. 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 be reduced, 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.

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. 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. 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 neces-

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sary. But the long resampling interval (24 years) in this work is not entirely required. 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). It may be worth commenting more solidly on the consequences of considering a third fast cycling pool as in Baisden and Keller (2013)? 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. 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.

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. 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

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appropriate, but the methods should be further clarified. There's no need for italicised 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. The decision I take issue with is choosing not to model points with  $\Delta^{14}\text{C}$  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 bulk  $\Delta^{14}\text{C}$  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.

More detailed and technical comments:

P893 L26 It might be useful to include a sentence relating density fractions such as Sollins et al 2009 to the issue of global soils and Al/Fe and C interrelationships.

P896 L4 The use of 'historical' in this sentence is strictly speaking not quite correct. It would be if the C-14 measurements were reported in the historic literature, but should be replaced with 'archived sample' or a similar term in this case.

P898 L13 'associate'-> associated

P901 L17 Lags are first mentioned much later, but would be appropriate to mention here given the introduction of the model and related discussion on assumptions.

P904 L15 I've already mentioned the need to better clarify methods in these paragraphs. Here, I can perhaps guess what Taylor series expansion you refer to, but this should be specified if it matters. If it doesn't matter quantitatively at the scale of the uncertainties reported, this use of approximations can be removed to avoid unnecessary confusing detail.

P912 L18 I'm not sure why this is 'intriguing'. I don't see why this is surprising given acceptance of a two pool model where the bulk  $\Delta^{14}\text{C}$  is dragged down by the presumed

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millennial pool. If they occur in low %C samples, this implies the more stable millennial pool is more dominant in these samples. If they are not modelled, discussion should be introduced of how their exclusion in the modelling method introduces potential bias.

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