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Biogeosciences Discussions

Interactive comment on "Predicting decadal trends and transient responses of radiocarbon storage and fluxes in a temperate forest soil" *by* C. A. Sierra et al.

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This manuscript presents a valuable follow-up to the Gaudinski et al. (2000) study. The original dataset and model, and this extension are of great importance to the understanding of C cycle because of the use of radiocarbon to look at the entire throughput of C through the soil and litter components of the ecosystem. As a result, this is very important work that allows us to use radiocarbon as a check on the consistency of our knowledge of the C cycle at the site, and vice versa. This process of gaining consistent information on the flow of radiocarbon through the soil plant system is challenging in forest ecosystems, and as a result there remains uncertainty in the conclusions of

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this study. Despite this, the work is well structured and deserves to be widely read by researchers interested in the temperate C cycle. Some minor but significant concerns need to be addressed prior to final publication.

General comments

First, I would like to elaborate that the structure of first examining the success of the Gaudinski et al (2000) model at predictions over the last ∼10 years is very useful, and fits neatly with the evaluation of the three hypotheses. I have only two concerns with this work – both expressed under Specific Comments below.

Second, the effort taken to publish the data with an associated model – all in R – is to be applauded. This is a very significant benefit of the approach taken by the authors. Although the evaluation of the hypotheses is somewhat inconclusive, the data and tools are all available for the research community to work on. This is an excellent approach to science in challenging areas, such as integrative understanding of the C cycle. It makes the inevitable result that it is difficult to provide all the answers in any given study entirely forgivable!

Specific comments

Regarding the evaluation of Gaudinski et al. (2000) model and subsequent evaluation of hypotheses, I have one concern that is not detailed sufficiently in the methods, and examination of the supplementary material supports my concern. This concern is that the Δ14C value of atmospheric CO2 in the vicinity of Harvard Forest is typically depressed significantly by fossil fuel emissions over the Eastern and Midwestern regions of the US. The scope of this issue was probably not fully understood in the late 1990s, but is now clear. The air measurements associated with the respiration measurements show this clearly. Studies undertaken at Harvard Forest (Turnbull et al, 2006) and more widely (Miller et al, 2012) make this evident. Wider studies suggest a deviation from clean air sites (e.g. Niwot Ridge, etc) of perhaps 10-20‰ and the air samples collected in this study suggest differences between the actual and expected

Δ14C in the supplementary material at the upper end of this 10-20‰ range. These differences could cause significant turnover time calculation errors if not appropriately included in modelling. The Δ14C variation associated with the timing of plant uptake in the seasonal cycle could also a more minor concern. I emphasize this is a potentially important addition to the previous modelling that could be significant in interpretation. Both issues should be detailed and their implications on this study described in this manuscript.

In the evaluation of the three hypotheses, I have two requests for revisions. First, it would be useful if the authors could clearly describe in what circumstances differences between the hypotheses, not observed under the present conditions, might be observable in the future or in a redesigned study. Second, the authors identify an additional hypothesis (changing pool sizes), and suggest they it may provide a better explanation of the data than the three hypotheses they formalize. Why not formalize this hypothesis as well? If this hypothesis is not formalized, a clear rational should be given for this. I agree that it should be considered however, as it remains an interesting and potentially valid hypothesis.

In the caption to Figure 3, it is stated that fractions were combined for this figure. The choice of fractions combined seems difficult to justify, as both the A-LF (< 80 μm) and A-LF (> 80 μm), as well as Oe/a L and Oe/a H fractions, seem incompatible based on the turnover times in Figure 1. In both cases, a fraction with a residence time of a few years or less has been combined with a fraction with a residence time of roughly 100 years. These should be separated if possible, as any such combination is likely to decrease the ability of the study to resolve meaningful differences between hypotheses, or between the model and observations.

Technical Corrections

I take issue with the use of the term “radiocarbon signature” used in this work (p2199 L6, etc). In my view the term signature should be used in situations where a signature is unique and recognizable, almost like a fingerprint but perhaps more subjective. In isotope science, this usually at least requires a bi-plot, and often multiple isotope systems. For bomb radiocarbon studies, it is particularly hard to claim a signature both because of the mix of residence times in operationally defined fractions (which the authors briefly and eloquently acknowledge p2213, L2) and the possibility of fits on either side of the “bomb C-14 spike”. It would be simplest and more correct to simply use "Δ14C value" in most of these instances.

P2210 L3-11. Statistical results are given but I am unable to clearly see what test was used. A minimum of an ANOVA should be used if multiple comparisons are made, and the statistical approach clearly stated.

Figure 8. It is difficult to differentiate the different hypotheses and observations on this figure. Something, such as a slight offset may need to be introduced.

References

Miller, J. B., et al. (2012), Linking emissions of fossil fuel CO2 and other anthropogenic trace gases using atmospheric 14CO2, J. Geophys. Res., 117, D08302, doi:10.1029/2011JD017048.

Turnbull, J. C., J. B. Miller, S. J. Lehman, P. P. Tans, R. J. Sparks, and J. Southon (2006), Comparison of 14CO2, CO, and SF6 as tracers for recently added fossil fuel CO2 in the atmosphere and implications for biological CO2 exchange, Geophys. Res. Lett., 33, L01817, doi:10.1029/2005GL024213.

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