Biogeosciences Discuss., 10, C4163–C4166, 2013
www.biogeosciences-discuss.net/10/C4163/2013/

Biogeosciences
Discussions

Interactive comment on "A dual isotope approach to isolate carbon pools of different turnover times" *by* M. S. Torn et al.

Anonymous Referee #2

Received and published: 8 August 2013

This manuscript describes an interesting study using 13C and 14C signatures to assess turnover times of light and heavy soil organic matter (SOM) fractions. The authors use an isotope dilution technique taking advantage of the isotopic signatures of the CO2 used in the Jasper Ridge open-top chamber study in combination with natural abundance 13C and 14C. One important contribution of this work is that, using isotope data, the authors show that both light and heavy OM fractions each contain labile and stable fractions. Previously, each of these fractions was considered to represent a rather uniform pool each having their distinct turnover time. The authors acknowledge though that OM is probably best represented by pools/fractions having a continuum of turnover times rather than few discrete pools. Yet, to date modeling approaches have focused on discrete pools. The paper was well written and the work appeared to have been done carefully.

I did have some questions about assumptions made for the modeling approach. It was unclear to me from a process understanding why the assumption was made that the percentage of new C for the ambient CO2 treatment to be the same as for the elevated CO2 treatment especially given the results by Hungate et al listed in the introduction suggesting that under elevated CO2 more C was being partitioned to rapid cycling pools. I understand that these assumptions greatly simplify the calculations as the authors suggest. However, the assumptions seem somewhat difficult to maintain especially when one tries to assess the effect of elevated CO2 on SOM dynamics. By making assumptions about similarities between ambient and elevated CO2 in terms of inputs and steady-state conditions, the approach presented here may be difficult to use when specifically assessing treatment effects. I realize that this paper does not necessarily focuses on assessing effects of elevated CO2 on SOM dynamics but the applicability of this approach to control/treatment comparisons will be limited if these types of assumptions have to be made in order to resolve the two-pool model. In addition, the introduction specifically deals with effects of elevated CO2 on OM dynamics. I think it would be good to address this issue and perhaps downplay effects of elevated CO2 in the introduction but rather state that elevated CO2 data are used to resolve the model and allow for new ways of thinking about OM dynamics. Still, it seems like the applicability in control/treatment comparisons is limited which probably should be acknowledged in the discussion unless the authors disagree with me.

Another challenge with these types of analyses is the limited number of 14C analyses that can be done. As a result it is difficult to determine reproducibility/statistical significance of the results. It would be good to perhaps discuss this more and provide the reader with a sense of the uncertainties encountered with the analysis and how these uncertainties may affect the overall conclusions. In cases where replication was used, statistical treatment of data was lacking which I think needs to be addressed. On several occasions the authors use qualitative statements such as 'variation in 13C is relatively low' (Line 232-233). While this is supported by the data in table 2, a proper statistical treatment of the results would further strengthen these statements. In addition, the authors make statements with regard to amounts of LF and DF materials between soils/depths that may or may not be significant. For instance, in line 369-370 the authors state that in both sandstone and serpentine topsoil there tended to be more LF material in the elevated CO2 treatments. However, I did not see that when looking at table 4. I doubt differences in %C and/or C stock are significantly different and in the serpentine soil average values are actually lower in the elevated CO2 treatments compared to the ambient treatments when looking at the %C and C stock numbers. I think doing some simple ANOVA would help remedy this issue and would allow the authors to be more quantitative about their statements.

The microbial data is interesting and somewhat surprising but I was concerned that perhaps the sample pre-treatment may have biased the results. For instance, removal of roots and leaves caused an elimination of fresh organic substrate (and associated microbial biomass). While several studies separate relatively fresh litter from SOM, the contribution of litter to the total heterotrophic soil CO2 flux can be considerable. In addition, homogenizing samples probably increased availability of relatively stable, physically protected, organic matter as the authors suggest. These issues should probably be acknowledged and mentioned in the discussion section.

While I had no major problems with the discussion I would probably include references in lines 458-472. Also, I would consider deleting lines 462-464 and 470-472. These lines seemed redundant. In line 473-480 the authors discuss the differences between the two parent materials. It appears that the serpentine soils show lower productivity and slower turnover. However, the amount of new C is higher in serpentine soils but turning over slower than in the sandstone soils. This appears to be contradictory but I may be missing something. In lines 583-586 the authors talk about hotspots. While several studies have shown presence of hot spots in this study bulk samples were taken which were later homogenized so I am not sure if the term hot spot is appropriate here.

I would delete lines 142-144; this seems a little redundant.

In Table 1 and 5 it would be helpful to put some spacing between the rows representing different categories. In Table 5 I was not sure what Fa, Fe, Ra, Re, Ma and Me refer to since these symbols are not explained in the text. Perhaps the model description was removed from the text?

Finally, the authors sometimes use ‘carbon’ and in other cases ‘C’ is used. I’d check to make sure consistent terminology is used.

Interactive comment on Biogeosciences Discuss., 10, 10189, 2013.