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Discussions

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

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This is a reply to Anonymous Referee #2. Below, we have copied in the entire text from the review, alternating review paragraphs and response paragraphs, so that the context for each response is clear. Each response paragraph is prefaced by [response]. We thank both referees for their very helpful comments and suggestions.

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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. (1997) 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.

[Response] Thank you for the comments and suggestions. Please also see the response to Referee #1. We have added new modeling results of non-steady-state as-

sumption in the elevated CO2 treatment (in the form of physically realistic increases in plant inputs during the experimental period 1992-1997 compared to the ambient CO2 treatment) and compared them with results of steady-state assumption. The results of both cases are given in Table 4. Overall, the results were similar between steady-state and non-steady state except that the turnover times of the fast pools in both LF and DF are slightly longer under non-steady-state assumption. Therefore, our main conclusions (i.e., about the dual-isotope method, and the comparisons of turnover times among soil types, depth intervals, and density fractions) were not changed. Note that we still assume the turnover times of SOM pools do not differ between the control and the elevated CO2 treatment, which is required to solve the two-pool model using 13C and 14C constraints from both ambient and elevated CO2 treatments. We have provided a more detailed description of the modeling approach in the revised manuscript, see Lines 189-203, 268-281, and 447-449.

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.

[Response] We agree with the reviewer that our goal was not to compare SOM dynamics between ambient and elevated CO2 treatment, but to constrain our two-pool model for each soil density fraction using 13C and 14C data from both ambient and elevated CO2 treatments (Table 4). We have more clearly explained this goal in the revised manuscript, and downplayed the effect of elevated CO2 in the introduction as recommended. We provide details about the assumptions of the modeling approach in Lines 189-203 and 268-281.

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 signif-

icance 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.

[Response] A caveat of this study (any many other studies that use 14C data) is the limited number of replicates due mainly to the high cost of 14C analysis. We measured 14C on the composite soil sample from 10 replicated plots for each treatment/soil/depth/fraction combination. Fortunately, in this experiment we could use variation in 13C as an independent measure of variation in the incorporation of plant carbon inputs under elevated CO2, which would be the largest source of variation in 14C values. The low variation in soil δ13C among the treatment replicates (Table 2) showed that the amount of carbon from elevated-CO2 inputs in each SOM fraction was relatively uniform. Therefore, the uncertainties of the estimated turnover times (Table 4) should be small and should not meaningfully 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.

[Response] Thank you for the good suggestion. We have added t-test results of comparing soil carbon, nitrogen, and isotopic values between ambient and elevated CO2 treatments in Table 2.

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.

[Response] We have deleted the statement "In both sandstone and serpentine topsoils, there tended to be more LF organic material in the elevated CO2 treatment than in the control treatment." as the CO2 treatment did not significantly change soil C stock (t-test, Table 2). We have added t-test results to Table 2 following your suggestion.

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.

[Response] We acknowledged that the standard chloroform fumigation-incubation (CFI) method for (Jenkinson and Powlson, 1976) microbial biomass carbon measurement has caveats (as pointed by the reviewer) and mentioned this uncertainty in our method section (Line 366-368).

While I had no major problems with the discussion I would probably include references in lines 458-472.

[Response] We have added references where indicated.

Also, I would consider deleting lines 462-464 and 470-472. These lines seemed redundant.

[Response] We have deleted these lines as suggested.

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.

[Response] Compared to sandstone soil at the same depth (0-15 cm) and same density fraction (light fraction), serpentine soil had a higher proportion (54% versus 30%) but much slower turnover time (8.8 versus 1.6 years) of fast-cycling carbon. The overall turnover time is 3 times slower in serpentine soil than in sandstone soil (5.2 versus 16.1 years). We have more clearly explained this point in the revised manuscript (Line 525-541).

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.

[Response] It is correct that we homogenized samples and do not have any direct evidence about hot spots. On the other hand, the observation from this study, Swanston et al. 2005, and others that there is some very fast cycling carbon within the (overall very slow-cycling) mineral-associated fraction is interesting and remains unexplained. For the light fraction, we note that the high root and rhizosphere density of California annual grasslands would affect the isotopic value of the homogenized LF samples as in this study. In both fractions, if there were hotspots of fast cycling material, they would show up in our homogenized mixture as a small amount of much younger carbon. Thus, we thought it worth mentioning that our results are consistent with the existence of hot spots of faster cycling within the soil mineral matrix. We cannot go beyond the language of consistency however, for the reasons the reviewer points out.

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

[Response] We have deleted these lines as suggested.

In Table 1 and 5 it would be helpful to put some spacing between the rows representing

different categories.

[Response] We have added spacing between the rows as suggested.

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?

[Response] We have revised the symbols to match the equations 12-14 in 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.

[Response] We have used consistent terminology ('carbon') as suggested. We also made slight changes to Figure 1 (we chose a turnover time of 10 years as an example of light fraction carbon based on the modeling results in Table 4) and Figure 2 (we added both steady state and non-steady state results).

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