

## ***Interactive comment on “Storage and stability of organic carbon in soils as related to depth, occlusion within aggregates, and attachment to minerals” by M. Schrumpf et al.***

### **Anonymous Referee #3**

Received and published: 17 November 2012

The work of Schrumpf and co-workers undertook in this effort is tremendous. They have compiled an incredible dataset. However, I have some major concerns about:

- 1) the lack of specific objectives and hypotheses that guided this work
- 2) the lack of justification (or in some cases even descriptions) for why they modified existing methods in one way or another
- 3) the excessive reliance on linear correlations, and in some cases even assuming causation exists just because they found some strong correlations.

Below I have elaborated on some of the major comments I have on this work.

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### Major comments

Page 13086, Line 19 and discussion the authors state that they believe ‘geogenic’ parent material affected the  $^{14}\text{C}$  values of the fLF – both from rock derived C and fossil C. If there is believed to be significant contamination from rock derived C, it is likely that they could have had major contamination of the fLF with minerals. The procedure must have picked a considerable amount of mineral material during the first mixing step that is meant to extract fLF OM. This is serious issue that the authors have to explain. When reading the methods section, the description of the density fractionation procedure doesn’t indicate why  $1.6\text{g/cm}^3$  was chosen as a density of SPT solution. Where there any preliminary tests done to determine that that density level is in fact appropriate for the range of soils that were used in this study? This could explain why they have high amount of mineral matter being picked up during the FL extraction steps. If the density is too low for the soils they are studying, you can float considerable amount of mineral matter with the fLF organic matter.

Starting in Page 13089, Line 1 and many other places in the manuscript the authors state that there are no studies that employed the approach they used here. This is not true, although there are not a lot of studies that consider these two analyses, there are some. For example Berhe and co-workers did just that in work that was published in two papers in 2008 and 2012, both in JGR-Biogeosciences.

Page 13092, Line 21 – Here, the authors are doing at least two things different than the Golchin and others technique. First, they are reusing SPT without any cleaning steps (which basically means they are shaking the samples after the fLF extraction step in water/SPT extractable organic matter solution). Almost all of the studies using this method, do not reuse SPT solution from the fLF steps to oLF and HF extraction steps, even for the same sample in the procedure, at least not without cleaning it first according to the methods of Six and others. The cleaning of the SPT after the end of each sample’s extraction is completed is described in the methods section, but not the reusing of the SPT used for extraction of fLF for OLF and HF steps that they describe

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in the method description. Second, they are incorporating a sequential sonication step instead of the one time ultrasonication step that is recommended in the procedure. In the procedure, typically, one does a preliminary test to determine an appropriate energy level for the specific samples that you are using and you stick with that for the rest of your samples. Although, I don't necessarily disagree with the need for these steps (I actually think the second one especially is a very useful modification of the technique), I think the authors should properly justify why these modifications need to be included in their method. In addition, the authors should go back to this in the discussion and discuss whether it had any influence on their results and how they interpreted them. While doing so, the authors should also indicate whether they meant to say they stopped the sonication when there was no further release of particulate OM or any and all OM (including water extractable, dissolved OM). For the second point, refer to the recently published work of Kaiser et al 2012 (Journal of Soil Science and Plant Nutrition) in regards to maximum energy levels for disruption of aggregates in soils that vary in mineralogy and carbon content.

Page 13094, Lines 3-4 – is there any evidence that addition of H<sub>2</sub>SO<sub>4</sub> directly into tin capsules doesn't have any effects on the results presented in this study? The most common method used for this application is addition of HCl to silver capsules. The authors did not provide a reference for this method.

Mineralization experiment – the authors do not state if during the time they stopped the mineralization experiment they had achieved near constant flux of CO<sub>2</sub> from the incubation jars (the relatively flat part of the curve of CO<sub>2</sub> flux over time, following the rapid, typically exponential decline). If they did not, then what is the justification for stopping the incubation experiment on the 20th day?

Page 13095-Line 28-19, check the first figure a) in figure 1. How is it possible that you have higher concentration of C in the HF compared to the fLF and oLF? Are you reporting

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Page 13101, Lines 6-15 and section 5.1 – the reasoning in this whole paragraph is not well supported by the data. You can have a linear correlation between root biomass and C in fLF, but that doesn't necessarily mean roots contributed more to fLF than above-ground biomass. It could be, but it is in no way certain based on this correlation. Reword this paragraph to restate that this could only be one of the possible (not the one) explanations.

Figure 16 and Summary – the schematic and discussion ignores at least two important factors for mineralization of C and distribution of C in different soil organic matter fractions – bioturbation and lateral distribution by soil erosion. Most of the earths land surface experiences if not both, at least one of these processes on a regular basis. The effect of both on total C in soil, distribution of C in different soil fractions, loss of C through gaseous and/or dissolved fluxes is extensively covered in soil science and biogeochemistry literature. The authors are advised to consider the contribution of these two important processes on their conceptual model and interpretation of their results overall.

Supporting information file

S, Figure 1 – the bar graphs (the left most charts in each line) of figure 1 in the supporting informing information file is confusing. As far as I can tell it looks like you are presenting C concentration of each fraction in these figures. I think these bar graphs are good if you present data on C distribution between the different fractions, but if all you want to present is

S, Table 1 – Mass losses of 20-38

S, Table A2, what bulk density values were used to convert the C in fLF, oLF, and HF values to kg OC/m<sup>2</sup> values? This information should be included at least in the supplementary information file.

Minor comments

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Page 13086, Line 15 – revise the sentence for clarity.

Page 13086, Line 18 – you can't say 'easily available' without testing the decomposability of the FL fraction . . . the best you can say is 'potentially available'

Page 13086, Line 19 – consider replacing 'geogenic'

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Interactive comment on Biogeosciences Discuss., 9, 13085, 2012.

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