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***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 #2

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General comments:

The authors are to be applauded for undertaking an investigation involving such a large amount of field and lab work. The dataset presented in this paper is an extremely valuable addition to the global pool of information describing soil organic matter distribution and mean residence time. The authors attempt to identify the soil physiochemical characteristics most strongly correlated with soil organic matter parameters across a range of ecosystems. This is a very ambitious undertaking, and I feel that even a dataset as large as the one presented is not sufficient to undertake the sort of correlation analyses the authors attempt or to sufficiently bolster the claims made in the discussion.

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The manuscript may benefit from the development of specific hypotheses and objectives. As is, the data analysis is presented somewhat as a “fishing expedition” where many variables have been measured then thrown into a correlation analysis and the significant r values picked out. The fact that some significant correlations were found is not sufficient evidence to prove that the statistical relationships among variables are actually ecologically interesting. The paper makes heavy use of simple linear regression, but the correlations may be spurious since an overwhelmingly large number of confounding variables are involved. Correlation is not causation, of course. Yet it seems like many times the text of the manuscript seems to assume that it is. More specifically, the main goal of the investigation, as stated in the abstract and intro, was “to test whether general controls emerge even for soils that vary in vegetation, soil types, parent material, and land use”, or to test for common controls that are exhibited in a large range of soils. But this is not really what the authors do. Their analysis looks for explanatory variables that are significant *across* broad environmental gradients, but does not test whether these variables are important *within* individual vegetation or land-use types. If they wanted to establish whether explanatory variables are important in many soil types, they would need to do their statistics differently, for example using mixed-effects models to test for correlations between ^{14}C or pool size and potential explanatory variables, while modeling land use or vegetation type as a *random* effect. Currently, they lump together all the soil types into simple regressions without accounting for soil type in their statistical models. This makes it impossible to tease apart the effect of land type as separate from the explanatory variables of interest. This was particularly problematic in the discussion of vegetation impacts. The authors state that “roots rather than above ground litter are the main sources for LF-OC” (p.13101). I would contend that this pattern could be due to the fact that the authors examined vegetation types with widely differing ratios of above to belowground biomass (e.g. Forests have a much greater proportion of their biomass aboveground than grasslands, so litter inputs may be more important in forests, while root inputs are more important in grasslands). Furthermore, foliage chemistry varies a lot among vegetation types, and this is

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a factor that may mute "universal" relationships. In any case, the greater importance of root biomass than litter inputs is a pattern that may occur across vegetation types, but there is no compelling evidence presented that this is a consistent pattern within vegetation types.

The discussion appears somewhat unfocused, and though a great number of interesting concepts are discussed, it isn't always convincing that the data presented in the paper actually significantly supports the concepts discussed. The manuscript may benefit from an overall reduction in the length of the text and the number of figures. It's more repetitive than necessary, and 17 figures is excessive to illustrate key points. The organization could be improved and streamlined by combining the results and discussion, with subsections for each of the topics listed in their summary. As an organizing principal they could aim to describe the key findings summarized in their conceptual figure (Fig. 16) as efficiently and simply as possible. All of the concepts discussed in this manuscript are well-established theories of soil organic matter cycling/stabilization, though the results of the current manuscript seem to confirm these previous findings, the fact that these are familiar concepts makes it possible to describe the current investigation's findings more efficiently.

Additionally, many times the data is presented in the form of somewhat unintuitive indices (e.g. percentage contribution of 10 cm depth increments to total stocks in 0-60 cm, contribution of fraction in 0-10 cm to total OC in fraction, contribution of roots in 0-10 cm to roots in 0-60 cm soil depth, etc.). It takes quite a bit of effort to tease out what these derived indices are supposed to represent conceptually, then an additional period of time to tease out what the correlations among indices are supposed to prove. Complexity is of course not a bad thing, but perhaps derived parameters should be used for very specific reasons to support a specific course of inquiry. The connection between these complicated correlational analyses and the initial research questions isn't stated in an explicit way anywhere in the manuscript at this point. I would suggest that the authors stick with straight-up pool size if possible. I would also suggest that

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qualitative description of patterns may be fine in some instances and more intuitive than the regressions the authors have calculated. They do not necessarily need to quantify the strength of every relationship to make their points.

Specific comments:

It was unclear to me what variables the authors equated with SOM "stability." For example, HF pool size? The relative proportion of total C in the HF? Pool 14C abundance? The authors should be more explicit and consistent about how they define stability. The word "age" is used throughout to describe differences in radiocarbon values among different soils. Since soil organic matter is composed of a variety of materials with widely varying radiocarbon values, the term "mean residence time" may be more appropriate. Depth is an extremely (and most of the time overriding) important factor in determining the bioavailability and mean residence time of organic matter in soils, yet depth is often ignored as a confounding variable in the correlational analyses.

Why were soils sampled by depth instead of genetic horizon? Many of the indices of soil physiochemical character used here vary more profoundly with genetic horizon than depth (e.g. clay content, Al and Fe oxide content, root abundance). Though this obviously cannot be changed at this point, the motivation behind sampling by depth might be a good point to discuss in the methods.

When mineralization rates were measured, were the samples periodically vented to prevent CO₂ concentrations from inhibiting respiration rates? Incubations under laboratory induced conditions (moderate temperature, high oxygen availability) do not mimic the conditions present in natural environments, therefore the results of laboratory incubations may have very limited applicability to interpreting organic matter trends observed in natural soils.

Section 13101-13102: The paper states "Increasing LF-OC with decreasing clay contents, and increasing LF-OC at higher OC loadings of clay particles or pedogenic oxides in the uppermost soil layers indicate a greater importance of LF-OC for OC storage

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at sites with limited sorption capacity of the HF". This is a very complicated statement, and it may be confusing two very different phenomenon that actually have very different causes. It is true that the amount of total C allocated to the light fraction is almost always higher in sandy soils than in clayey soils since mineral surface area is small in the former case. It is also often the case that soils with higher organic inputs have high "C loading" values. However, these are two different scenarios and have little bearing on each other. The former is caused by a lack of available surface area for sorption of organic inputs. The latter is due to high organic matter inputs. In both cases, it could be argued that an increase in free/light fraction does not really represent any type of "importance in storage" since free/light fractions (especially in surface horizons) have short turnover times.

Section 13101-13102: "Our study confirmed previous observations that secondary hydrous Fe and Al phases are generally more important to OC accumulation in the HF than clay-sized particles." Fe and Al oxides often are clay-sized particles. It may be more clear to say "secondary phyllosilicates" or "total clay content".

In addition to the points made in their summary, it would be very interesting to see an additional section dealing with transport mechanisms. It would be good to see this topic brought from the supplement to the main paper (e.g. It's discussed briefly on p. 13103). Regulation of SOM inputs and transport could have very important influence on 14C profiles, and this is treated too cursorily.

The figures and tables in the supplementary materials are mislabeled in the text (i.e. numbers are mismatched between text and actual figures/tables).

Figure 4: This figure is missing a key for the symbols.

Figure 7: One cropland is included in this figure, but it's difficult to interpret the influence of root abundance on OC stocks given that crops may vary from year to year and root abundance will certainly change drastically with season/harvesting.

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Figure 8: Why is the “Ca” site labeled in this figure? If it is being considered an outlier, was it included in the correlational analyses or not?

Figure 9: Outliers in the dataset are giving falsely high r values. The data needs to be transformed and the linear regression reapplied.

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