

## ***Interactive comment on “Age distribution, extractability, and stability of mineral-bound organic carbon in central European soils” by Marion Schrumpf et al.***

### **Anonymous Referee #2**

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Overall, I thoroughly enjoyed this manuscript. However, I am concerned that much of the language in the discussion is speculative in nature. I do not believe that reasonable speculation should not be allowed, as true statistical replication in soil studies is sometimes difficult to achieve due to measurement cost (NMR) or sample availability. But the number of statements basing interpretation of mechanism or process which are based on data from one sample or site seems very high in this manuscript. The authors could focus solely on interpretation of how extraction chemistry influences the portion and composition of extractable organic matter, and that would be sufficient.

The comments linking NaF-extractable C with Al/Fe oxyhydroxides don't seem well

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supported by the data. Figure A2 illustrates a significant relationship between H<sub>2</sub>O<sub>2</sub> residues and extractable Al/Fe. Why are there not similar plots for NaF residues?

From the abstract it isn't clear from the abstract why these two particular extractants were chosen. This may be a major gap in my knowledge base, and if that is the case, please ignore. I have never previously seen an experiment that utilized a mixture of NaOH and NaF to evaluate SOM extractability/solubility. Is there a reference that can be associated with this method? If the abstract included an explanatory statement, it would be very helpful. It sounds as if H<sub>2</sub>O<sub>2</sub> is meant to represent oxidizable SOM, which the authors may be arguing is not very representative of the portion of SOM which is readily exchangeable under field conditions. In comparison, a NaOH and NaF solution presumably extracts SOM which is actually bound on the exchange surfaces of minerals. If this is indeed the case, it needs to be stated more explicitly in the abstract. Maybe something similar to the statement on lines 102-104 of the introduction.

In the abstract, the language describing trends in radiocarbon data uses all the following terms: 14C, 14C content, MO14C, 14C-depleted, older, and age. It would improve the clarity of the results if the language was more consistent when describing this data. Since depletion and enrichment are relative terms (depleted in comparison to what?), it seems necessary to include actual values with associated error terms when describing significant differences between extractions and residues.

The abstract fails to mention that the soils were density fractionated prior to conducting the extractions.

It is unclear in the abstract how the experimental results could lead to the conclusion that MOC was dominated by OC interactions with pedogenic oxides. This seems like a complete non sequitur.

Throughout the manuscript, it would increase the clarity of the arguments and results if the language regarding radiocarbon analysis was cleaned up. I hate to argue semantics, but I believe the terms 'stability' and 'lability' are now often rejected by the

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community due to lack of specificity. Colleagues have been insisting on using the term 'persistence' as far as I know. I also believe 'older', 'younger', and 'age' are not appropriate for use in a manuscript, unless in reference to a 'mean system age'.

In hypothesis #5, it is unclear what the term 'organic acids' is referring to.

In the figures, it would be helpful to readers if the soils were referred to according to their dominant physicochemical attribute rather than the site name. Hesse = loess/Cambisol; Laqueuille = grassland/Andisol. . . or something similar. That way the reader can immediately draw inferences based on differences in soil physicochemical characteristics. Again in section 3.3, using site names gives little information, and forces the reader to continually check back to table 1 for context.

What was the pH of the NaF/NaOH solution (13-14, correct?), and what influence do the authors think this had on the amount and characteristics of the extracted C? Perhaps the effect of using such a strongly basic solution masked any influence of F on the amount of exchangeable C displaced on mineral surfaces.

Butnor et al., 2017 (doi: 10.1016/j.foreco.2017.01.014) is an additional reference that explores H<sub>2</sub>O<sub>2</sub> residues and Fe oxyhydroxides.

It would offer additional insight if the authors could measure  $\delta^{13}\text{C}$  and  $\delta^{15}\text{N}$  for the extracts and residues to see if there were significant differences in the degree of microbial processing. Perhaps it would be acceptable to use the  $^{13}\text{C}$  values from the AMS measurements? I know these aren't typically considered publishable due to possible fractionation effects, but it seems as though they could be used for intercomparison purposes if it can be assumed that all samples experienced similar degrees of fractionation during AMS measurement?

Additional NMR data would also be interesting. Would it be possible to measure the H<sub>2</sub>O<sub>2</sub> residuals?

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