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Discussions

Interactive comment on "Variability in ^{14}C contents of soil organic matter at the plot and regional scale across climatic and geologic gradients" *by* T. S. van der Voort et al.

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This study reports a large set of data collected on soil C and radiocarbon profiles from a range of sites in Switzerland. Soil studies are often conducted according to 'state factor' analysis; i.e. climate- or chrono- sequences. However, the important of state factors should hopefully also fall out of statistical analyses that include sample covering a wide range of state factors. This study has two goals: (1) to analyze spatial variability of C and 14C within single "sites", and (2) to document spatial variability among different soils that vary in factors like temperature, precipitation, NPP, etc. for forested soils across Switzerland. There is an exploration of how microtopography may be responsible for spatial variability within forest plots; this showed very interesting differences in variability for different soil types (e.g. Podzols versus Cambisols).

Overall, the study provides a valuable data set, especially very few data are available that deal with the spatial heterogeneity of SOC and 14C and how that might impact comparisons made at broader spatial scales.

As with other studies documenting variability across sites (e.g. Schrumpf et al. 2013, Herold et al. (2014) and Mathieu et al. (2015), the variations in the vertical are always larger than variations laterally for 14C (and C). Although the soils studied differ in many respects (e.g. parent material geology, climate, etc), all are apparently quite young soils (developed on moraines or outwash fans). This is pointed out in the paper (lines 329-330), but perhaps could be highlighted a bit more than it is as an explanation for similarity among soil profiles.

The authors should add more information to Table 1, including total soil depth - are these also all shallow soils, or do the soils continue deeper than the depth-specific sampling? Although the authors investigated the predictive capability of a number of factors, such as clay content, pH, etc., the reader never knows the range of these values (they are not given in Table 1, please give at least a profile average here for the factors used in the multi-regression). Maybe the lack of difference (except for the Podzols) arises from the overall similarity in these factors of many of the soils studied? The differences in C content would seem to indicate not, but the reader is not able to judge.

Also, although all of these are forested sites, is there any evidence that they were previously unforested (e.g. Ap plow layers)?

A second issue that affects variability is something like the presence or absence of earthworms (for example, these tend to be found in Cambisols but not in Podzols, and they also affect the thickness and age of C in the litter layer. The 'biota' state factor includes in-soil fauna, it could account for some of the differences in variability among the different soil types. Normally such things are noted in profile descriptions, and are semiquantitative; nonetheless they may be important.

Similar findings regarding similarity of vertical profiles of 14C in different soils were obtained by Mathieu et al 2015, which came out around the time this was submitted; while 14C characteristics are similar at the surface, deeper soils reflect the influence of soil order (something that can be related to geology and vegetation/climate regime and time together). However, that study used global soils, and mixed in with soil order is soil age (there are not young oxisols, or old inceptisols). A more comparable study to this one would be Schrumpf et al. 2013, which is cited here but it would be interesting to compare their estimates of spatial variability with yours (as a function of depth).

The use of %C as the metric for C content is problematic, especially in litter layers, which can have highly variable bulk density. Is there information to report carbon density gC cm-2 for each of the depth intervals?

Some more detailed comments:

Line 119. Were samples stored in glass jars or paper bags?

Lines 150-155. If the 14C signature of bulk C was above the contemporary atmosphere 14C, there will be two solutions (two values of k) that can reproduce that value with a single pool model. Which one did you choose, and what reasoning did you use to decide? This needs to be described in the paper.

Line 172. When you say variables such as clay content, pH, etc were taken as "fixed effects", does that mean you used some profile-averaged value in statistical comparisons? I found this description confusing, can you make it clearer? Also, please give the values for pH, clay, etc in Table 1. If available, cation exchange capacity might also be a useful variable.

Line 271. Schrumpf et al. (2013) found a relationship between the slope of the radiocarbon-depth relationship and dithionite extractable Fe; Herold et al. (2014) also found that Fe(d) was a good predictor of C content. This indicates that a common stabilization mechanism may be operating across their soils, which could also be an explanation for the similarly of depth profiles. Is there any similar measure for these soils (even cation exchange capacity, which is more frequently measured than Fe(d))?

Line 293-4. The link of 14C to MAP as reflecting waterlogging is a bit speculative at the larger spatial scales, though you do have possible evidence from the intra-site variability in soils that have evidence of redox variability (e.g. Figure 6). But at larger spatial scales, would not clay content be expected to be related to drainage (e.g. does this relationship trace to Gleysols and Stagnosols?)

The next lines, about relief, are also a bit speculative. How was "relief' reported in Table 1 determined? At the microtopographic scale, or the macrotopographic scale? While I agree it may indicate something about erosion in general, it may also be correlated with other factors like parent material, temperature, etc. You need a separate measure (e.g. 137Cs) to say something like this definitively.

Line 303. Typo, should be "noted"

I did not understand lines 304-305: " but when assuming a steady state system, it is reasonable to assume that the speed of incorporation of carbon and hence turnover is directly related to carbon stocks." Do you mean the larger the C stock the faster the turnover should be (e.g. as it is with soil depth, most C and fastest C at the surface?) or do you mean the more 'standard' sense, of largest stocks having overall slowest turnover (e.g. integrating low C concentration over the large volume of deep soil means it has the largest stock, which is associated with slowest turnover). This is a place where it is important to give C stocks, not just concentrations.

Line 334 " the relative independence on climatic parameters may persist in deeper soils" However, you did have a relationship with MAP – which could indicate some kind of effect of redox-related stabilization (see above). Overall, stabilization mechanisms appear to operate on similar timescales, independent of the amount of C being stabilized?

The discussion of microtopography is a little frustrating for the reader to follow, as there is never really a good definition of what the authors mean by it. We can visualize ‘hummocks’ and ‘hollows’, but can their spatial dimensions be better quantified? Were they really traceable to tree-throw? Or perhaps (in young soils) to variations in the underlying till structure (e.g. the presence of a large underlying boulder)?

Lines 374-378. How were the semivariograms constructured? Did you try to use a specific depth (e.g. 0-5 cm) or integrated depth profiles (e.g. kgC m-2, or C-weighted mean 14C)? Would it make a difference? (perhaps soil depths also vary, but this was not captured in your sampling scheme..)

Lines 386-7. Soils subjected to fluctuating redox conditions might be expected to overall cycle C faster (if the major stabilization mechanisms have to do with Fe-oxides). Also, sampling across mottles (reduced and oxidized Fe) can mix C of quite different ages (see Fimmen et al. 2008)

Line 390. “Overall, the geochemical characteristics...” You have mentioned only one indicator, the C/N ratio. This is a good indicator of decomposition in organic layers, but I am not convinced it is so good deeper in the mineral soil (though you are mixing different stabilization mechanisms together, low-density and mineral-associated material). It would be nice to have some factors that more directly relate to stabilization mechanisms themselves (e.g. cation exchange capacity, or surface area; see Lawrence et al. 2015).

Lines 408-9. “the speed of C incorporation may be relatively insensitive to changing climate conditions” However all soils had bomb C – so the speed of C incorporation is relatively fast overall; it is just that it is similarly fast. Also, you do have evidence of sensitivity in the factors that create microtopography (erosion/redox variation) both of which can change with climate conditions.

Figure 3. Error bars for the vertical axis (%SOC) are not visible – are they small or just not shown?

Figure 4B. It is apparent that the Nitrex site used to study microtopography (is not sampled at constant depth intervals; in other words, 14C samples are integrating different depth intervals. Thus, especially for the deepest horizon, it is difficult to see that the resulting trends are due to microtopography rather than sampling (lowest 14C has the largest integrated depth interval). Or am I missing the intent of this figure?

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