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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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Dear Prof. Trumbore,

Many thanks for the valuable input on this paper, it is much appreciated. We have attached a supplement pdf with formatting which may be easier to read. In the following we provide responses to your comments and suggestions:

"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. "

–> thank you for this suggestion, we have incorporated this point in: line 282 "Soil formation for the soils studied here initiated after the last glacial retreat and can hence be assumed to have started to form simultaneously, which may explain the similarity in their 14C distribution with depth."

–> we have also incorporated the Mathieu paper (which indeed came out as this was submitted). Line 256 "Mathieu et al., (2015) also found that the carbon dynamics in deeper soils are not controlled by climate but rather by pedologic traits, whereas topsoil carbon dynamics were found to be related to climate and cultivation."

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.

–> thank you for this suggestion. We have added this information in Supplement Table 1 as there was insufficient space in the original table. (1) to Supplement table 1 has been added. a. Clay, sand, silt, pH content of the 0-5 cm layer average of a single profile. The 0-5 cm layer was chosen as it was the most heavily studied. b. Range of total soil depth as determined during the 1990's sampling of the WSL LWF campaign c. Bulk density in a single profile for the 0-5 cm layer as determined in the field. Description of method can be found in method section. d. Carbon stocks for 0-5 cm layer calculated by multiplication of density (gsoil/cm3) × carbon concentration (gC/gsoil) × layer length (cm) = (gC/m2). This data was not included initially because the bulk density measurements were derived from single profiles. But as you requested we now added it. The bulk density method is now described in the methods section:

Line 89 “These samples were taken using steel cylinders of 1000 cm3 volume (for layers with a thickness of at least 10 cm) or 458 cm3 volume (for thin layers with less than 10 cm thickness). Volumetric samples were dried at 105 °C for 48 hours minimum until the resulting mass remained constant. The density of the fine earth was determined based on oven-dried volumetric soil samples and sieving the samples in a water bath to quantify the weight of stones >2 mm. The volume of stones was calculated by assuming a density of 2.65 kg/m3 for stones (Walthert et al., 2002).”

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

–> thank you for this suggestion, these sites have been forests for at least several hundreds of years (Gosheva et al., in prep, personal communication) as shown in older Swiss maps. Furthermore, the sites in the WSL LWF campaign are specifically selected to be mature forests (LWF).

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.

–> thank you for this suggestion. We found information on soil biota in some of these sites that we were previously unaware of (Ernst et al., 2008), and have incorporated this suggestion:

Line 271: “Furthermore, the presence of soil fauna (earthworms) at some sites (Bettlachstock, Schaenis, Lausanne, Alptal, Visp and Novaggio) may also complicate the response of carbon cycling to climate due to physical reworking and transport (Ernst et al., 2008).”

Line 325: “Ernst et al (2008) described the presence of earthworms in the Gleysol and Cambisol, but not in the Podzol. Because of constraints on the dataset size, no conclusive quantitative relationship can be established, but we hypothesise that the ubiquitous presence of in-soil fauna and associated transport activities would contribute to an overall increase in homogeneity rather than heterogeneity.”

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).

–> Thank you for this suggestion, the Mathieu paper indeed came out as this manuscript was submitted, we have incorporated it as stated above. Line 256 “Mathieu et al., (2015) also found that the carbon dynamics in deeper soils are not controlled by climate but rather by pedologic traits, whereas topsoil carbon dynamics were found to be related to climate and cultivation.”

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).

–> Schrumpf et al. (2013) HF and oLF values fall within the same Δ14C range the profiles measured in this paper (Fig.8). Because the Schrumpf et al. (2013) paper only refers to values of the fractions and in this current paper only bulk is concerned, we chose not to include a direct comparison. We will be sure to include this information in planned future papers that include fraction-specific radiocarbon data.

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?

–> (1) Although this is not part of the normal WSL LWF database, we were able to acquire information of the approximate litter layer bulk density, and we have now included it in Supplement Table 1. (2) Additionally, we have added estimated carbon stocks (N.B., the bulk density is determined based on a single profile proximal to the plot where samples described here were collected), and have included them in the linear mixed effects models. (Supplement Table 1, extended Table 5, 6)

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

–> Samples in the WSL Pedotheque are stored in plastic containers. This has been incorporated: lines 64-65: "The LWF sites are all located in mature forests and samples were collected in the during the 1990s and have been stored in plastic containers (Innes, 1995)"

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.

–> This is a very good point. In the cases where two options were possible we chose the option which corresponded with the turnover estimates of the layers above and below, as we assume deeper soil layers to always have slower turnover than shallower soil layers: Line 135 "For the 0-5 cm topsoil layer two MRT were frequently possible, in which case it was assumed the true MRT value of the deeper layer is the one that exceeds the MRT value of the accompanying litter layer, as carbon turnover rates decrease with increasing soil depth."

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. –> This indeed required clarification. First, we used the clay and pH values measured for the layer depth interval identical to that which was measured. Hence, it is not a profile average but sample-depth specific value. We have clarified this in the text: line 156 “The compositional parameters (e.g. clay, pH) are depth interval-specific.”

–> w.r.t. cation Exchange capacity: We found more ancillary data that we were previousy unaware of, from which we calculated the CEC (after Blume et al (2002), Lehrbuch Bodemkunde chapter 5) and have now included that in the linear mixed effect models analysis.

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))?

–> We also found ancillary data for Fe and other metals (Fe, Al) extracted by HNO3. within the WSL LWF database for this, and have included it in the linear mixed effect model. However, only in the 0-5 cm layer the linear mixed-effect model indicates a significant positive relation between Δ14C and Fe content, for all other depths the correlation is not significant.

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?)

–> Since we only have two sites to compare in this context we feel that we cannot test this, but we adjusted the wording to more accurately reflect the nature of the statement. Line 258 “The strong negative Spearman correlation of Δ14C and MAP at 10-20 cm depth implies a slower turnover that could potentially be caused by increased water-logging or anoxic conditions induced by higher precipitation.”

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.

–> The slope in table 1 was determined on a scale of the larger WSL LWF sites, i.e., several tens of meters in both directions. –> The relief of two sites (Lausanne, lowest variability and Beatenberg, highest variability) have been monitored and the curvature of the larger area has been calculated using ArcGIS. This information was not available for Alptal. From this we can quantitatively observe that the degree of variability varies significantly. The surface in the Lausanne plot hardly has any curvature whilst Beatenberg has strong irregular microtopographic oscillations. This will be added to the supplemental documentation. Line 303. Typo, should be “noted” incorporated

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.

–> This issue was addressed by adding the C stocks. We have clarified the formulation. We meant the "standard" sense, i.e. that larger stocks are associated with slower turnover. Furthermore, the Spearman correlation between MRT and C stocks in the Litter layer gave a strongly significant positive relation (0.77**) indicating that larger stocks are associated with a higher MRT thus slower turnover. This will be incorporated into the results section. Line 275 "in a steady state system it is reasonable to assume slower turnover is coupled to larger carbon stocks".

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 mecha- nisms appear to operate on similar timescales, independent of the amount of C being stabilized?

–> This is a valid point. We have adjusted the wording to fit more appropriately: line 309: "the relative independence on temperature and primary production may persist in deeper soils" This paper does not provide a detailed discussion on stabilisation mechanisms, as it focuses on Δ14C and less, for instance, on organo-mineral interactions, but in future work are seeking to also examine this.

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)?

–> Thank you for the suggestion: (1) The description for the Gleysol is as quantitative as possible, with descriptions of mound/depression height and width of the mounds and depression. Unfortunately, no ancillary data is presently available. We agree that could be better to have radar images of the surface, but acquiring that is beyond the scope of our present project. (2) For the Cambisol and Podzol we have curvature plots, which have been added to the appendix. (3) Tree-throw has been observed visually in the field. (4) We do not think variations in till structure play a role as we took numerous cores in 2014 from the same sites and did not see any significant structural variation. Again, however, we can only provide a qualitative indication.

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..)

–> The Semivariograms were only for the 0-5 cm interval because available spatial variability data was most abundant for that depth. For deeper samples, we do not have sufficient data, but this would certainly be interesting to look into in the future.

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)

–> Thank you for the helpful suggestion. Indeed, Fimmen et al (2008) found a positive correlation between changing redox conditions with an increase in C breakdown (especially when Fe is high such as in this site). In our case the intermediate system (mottled) has the oldest signal, which assuming the results of Fimmen (2008) are true, would indicate that this system would be under more stable redox conditions, as opposed to the stronger depression. From the topography and groundwater flow, we can only suppose that the deeper the soil the more permanently it would be submerged, i.e. we would expect the deepest soil to have the most stable redox conditions. However, we do not know enough about the groundwater flow to make any conclusive statements. We do not think it is likely to be a local mottled/non-mottled effect, as the sample is the average over a depth interval and several cores. Given the Fimmen et al. (2008) results are inconsistent with our results, and that our evidence is too inconclusive to go against their conclusions, we have left this discussion out of this present contribution.

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).

–> CEC has been included in the Supplemental Table 1. Unfortunately surface area information is not available, for future work we try to acquire this information.

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. –> Good point, we have adjusted the wording: line 368 "the speed of C incorporation may be similar and hence relatively insensitive to changing climate conditions."

Also, you do have evidence of sensitivity in the factors that create microtopography (erosion/redox variation) both of which can change with climate conditions. Indeed, potentially, with changing climate, extreme weather events like storms and droughts could be more commonplace, which in turn could induce stronger microtopography by forming rills etc. This remains very speculative however, and from the available data we cannot really say what effect that would have on the long-term cycling of soil carbon, except to point out that increased extreme events may increase erosion. As this remains speculative, we have not incorporated this discussion into the paper. Obtaining data pertinent to this question this would be a valuable line of future research.

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

not shown?

–> they are smaller than the point.

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?

–> Indeed, the four types in the NITREX plot serve to assess variability. The horizon-specific sampling method has an advantage because it also allows assessment of the effect of microtopography on horizon development and morphology (Fig. 6), but it is indeed disadvantageous because impedes a direct quantitative comparison with the other study sites. While we can only compare the signals qualitatively, it gives us a better idea on variability on a catchment-wide scale. The specific separate microtopographic effects are shown in Fig. 6.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2015-649/bg-2015-649-AC2-supplement.pdf

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