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Interactive comment on "Dynamics of deep soil carbon – insights from ^{14}C time-series across a climatic gradient" *by* Tessa Sophia van der Voort et al.

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Understanding the dynamics of carbon in deep soil layers is an important issue, and this study uses an excellent sequence and provides a rare dataset: soil 14C measurement at two dates using archived samples brings a precious information of C dynamics. One of the interesting results is the demonstration of the occurrence of rock-derived carbon. Another concerns the age of water extractable carbon. The analytical methods are high standard and highly relevant. I therefore consider it is worth publishing the data in Biogeochemistry. Unfortunately, there are major concerns that need revision. The most important is that the mathematical and numerical interpretations look

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inappropriate, and this leads the authors to give conclusions that are in contrast with what the data show, whereas some unprecedented results could be derived. I finally suggest two alternative solutions: either the authors drop the modelling part and make a semi-quantitative interpretation of the data, either they use another model. I also noticed miscellaneous improvements to be done. The discussion should be updated according to these major points. The title and summary are nevertheless appropriate.

1. The chosen model is unlikely to simulate observed data.

Most of samples below 10 cm show an increase in Δ14C between 1990's and 2010's, by several 10‰ (Figure 3), and even some above 10 cm do. As seen in the FIGURE below, which was built for this review, the 14C content of well mixed compartments directly fed from atmospheric C has DECREASED with time since the 1990's (or increased by less than 4‰ for slow pools). The sum of two parallel pools cannot have a Δ14C increased between 1995 and 2014.

FIGURE: Simulated Δ14C of a well-mixed compartment under steady state as a function of compartment turnover rate, for two dates of sampling.

I finally understood (from 14C data in Figure 3 and turnover time data in Table S5) that the the "mosty reliable' kWSOC value is more or less the arithmetic mean of two kWSOC values, one calculated in the 1990's and the other in the 2010's. The authors must invoke other processes to explain an increasing Δ14C. These processes may act together and interact:

- Transit of carbon in another horizons or pool before entering the observed layer. This might be associated with either bioturbation or DOC production from an above layer, movement, and insolubilization. The data tend to indicate that carbon movement is a significant cause of the increase in Δ14C across the sequence.

- non-steady state, e.g. increased bioturbation due to warming, change in NPP and/or decay rates.

To me, the fact that the Δ14C of WSOC of all samples (except Othmarsingen 0-5 cm and Lausanne 0-5 cm) inceased is a proof that WSOC is a by-product of SOM aged several 10th of years (usual age of OH horizons), and not directly fed by vegetation decomposition. This would be a bright finding and merit appropriate modelling.

2. Consistency in model implementation (to be confirmed).

I tried to calculate by myself turnover time values, based on 14C data in Figure 3 and turnover time data in Table S5, and didn't find the author's results. This may arise from the fact that the basic differential equations of the model (equation 5 = SI.7) looks false, or at least do not correspond to authors' hypotheses. Equation SI.7 states:

$F(t) = k \cdot Fatm(t) + m1 \cdot F(t - 1).(1 - \lambda - k1\ ) + m2 \cdot F(t-1).(1 - \lambda - k2)$

This equation indicates that the flux of 14C leaving the system (out of desintegration) is:

$(m1.k1 + m2.k2).F(t -1)$, i.e., $k.F(t)$

Since the corresponding flux of carbon is $k = m1.k1 + m2.k2$, this equation says that the 14C activity of carbon leaving the system is $F(t - 1)$. So the equation would IMPLICITELY considers that the activity of the flux out is the same as that of the compartment itself. This is typically the assumption of a so-called 'well mixed' compartment, and is not the case of a system with two compartments. It would only accept the solution $k1 = k2$. Making this implicit assumption is a current mistake or at least a source of disagreement in isotope geochemisty. As a consequence, I guess that the authors have calculated a mean turnover time corresponding to a single compartment for bulk carbon, and an independent specific turnover time of WSOC. The error might be linked with my point 3 below. See a proposal for the correct equation as an appendix of this review. The authors are invited to check how eq SI.7 was implemented and how the couple (k2 , m1) was inferred from bulk F14C.

3. Mathematical (and semantic) misuse of "turnover time'.

Let us call the turnover time of carbon in the compartment T = 1/k Mathematically, the carbon input to the system is m1/T1 + m2/T2. The size of the compartment is m1 + m2. So, the turnover time, which is the ratio of pool size to the input, is:

$T = (m1 + m2)/(m1/T1 + m2/T2)$

In Table SI.5, which presents the main result, i.e. the values of turnover time, the authors calculated the bulk turnover time as:

$T = (m1.T1 + m2.T2)/(m1 + m2)$, which is wrong.

What authors call "turnover time" is in fact the MEAN AGE of carbon, which is different of the mean turnover time in non-well mixed compartments. The error in not only semantic because it possibly have interfered in model and 14C equation (point 2). Sierra et al. (2016), whom you cite lines 161-162, recommends the use of "age", not "turnover time" for this variable. See also Manzoni et al.(2009).

4. Data availability.

The authors must provide in SI a table including the primary data, i.e., Δ14C, C stock by horizon, WEOC stocks. Reference that were used to estimate atmospheric Δ14C (post bomb and pre-bomb) should be indicated (e.g. Reimer , Hua etc.)

5. Hypothesis on WSOC as the labile pool.

Line 180-182 and 190-191: A major (if not the major) assumption of the model is that the dynamic pools has the same decay rate as that of WEOC. The 'dynamic' pools contains as much as 88% of soil C (on the average 34%), whereas WEOC only a few %. Assigning the constant k of WEOC to the dynamic pool is therefore a surprising and very heavy hypothesis. (see also point 1.)

Alternatively, the study may have targetted the study of WSOC dynamics for itself, e.g., considered that both WSOC and bulk C are heterogenous pools, each with a labile and a more stable component, but in varied proportion. Many other models use particulate

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organic matter (i.e. either sand-size primary organic particles or light OM, which has been described as having a good fit with labile carbon.

6. Conclusions on correlation with MAP.

Projecting conclusions on the effect of MAP on the basis of a "wet" sequence, i.e., where the water deficit is probably low if not nil, may look brash. The driest site is 800 mm, but with a MAT 1.3°C and probably a small PET. Furthermore (Lines 360-361), authors state that 'The only climate-related driver which appears to be significant is precipitation' whereas the r2 coefficient between MAP and turnover 0-20 cm is 0.04! I would recommend here to cite Carvalhais et al. (2013) and Mathieu et al. (2015), who highlighted the role of precipitation in SOM stabilization or ecosystem carbon turnover. I sfinally uggest to mederate the conclusions, but maybe discuss the role of precipitatioon on DOC movement (see point 1).

7. Presentation of model and equations.

The presentation of both the model and the optimization process is obscure throughout the text and should be more precise, in either text or SI. In the cases with four radiocarbon dates (2 sampling dates x two fractions), the optimization of three dynamic parameters is not a formal solution, but a best fit. The type of adjustment (least squares ?) and a criterion of the fit (e.g., RMSE) should be indicated. Harmonize the name of variables throughout the text and SI. For consistency with SI, please use m insteaf of F in eqn (3), (4) and (4); and possibly F instead of R. Also use the same character k in SI and main text. Harmonize M (Figure S2) and m, etc. How were single points managed ? (Line 194-195. " Due to limited availability of archived samples, there are only single time points available for some samples as indicated in Fig. 4.")

8. Miscellaneous.

lInes 51-52 note the pioneer studies by Jenkinson et al (1992) on long-term experiments. The models by Braakkeke et al. (2014) also simulates 14C profiles in rather similar podzols, using WSOC as well, and may receive more attention in the discussion section. Also note (e.g. Line 34) the conclusions of Mathieu et al. (2015) concerning soil versus climate drivers of 14C, and (lines 39-40) the recent paper by Balesdent et al. (2018), which improved the understanding of the significance of deep soil C to the global C cycle.

Move lines 126-128 (WEOC) to the end of 2.1. (WEOC extraction). Note that extraction with Na 0.86 M is not exactly Water extraction, since it moves some exchangeable calcium, disperses clays and therefore moves sorbed organic compounds that would not have been mobilized by water.

Line 252 ' Deeper soil bulk stock and turnover positively...' and table S5: avoid "turnover " alone standing for "turnover time" in such sentences, because the common sense of turnover is turnover rate, i.e., the inverse of turnover time. This may lead to a reverse understanding of correlations.

Line 262. Balesdent et al. (2018) reported that 21% of world subsoil C (30-100 cm) is less than 50 years old.

The amount of WEOC (while not used in the modelling experiment) would be welcome.

Surprisingly, the section of Material and methods indicates that NPP and its components were measured, which is a rare information in SOM studies. As a result, authors have an indicator of the true turnover time of soil C, i.e. the ratio of Soil C stock to C input is known, that they do not use.

Figure 4 contains the main primary result of the study. Polices Should be enlarged. The square signs for Aptal WEOC 1997 are misleading. Table S5 is the main final result and should take place in the main document.

Note that the bi-exponential age distribution is factually the age distribution of C in current "four pools" models such as RothC (or Century). All coupling of these models with radiocarbon more or less managed bi-exponential age distribution and 14C; e.g.,

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Jenkinson et al. (1992).

9. Appendix

The differential equation should consider F1 and F2 the 14C fraction in pools 1 and 2, respectively, as illustrated in your Fig S1.

Input flux to pool1 is k1.m1; input flux to pool2 is k2.m2

$F1(t) = k1.Fatm(t) + (1 - k1 - \lambda).F1(t - 1)$

$F2(t) = k2.Fatm(t) + (1 - k2 - \lambda).F2(t - 1)$

which give: $F(t) = m1F1(t) + m2.F2(t) = k.Fatm(t) + m1.(1 - k1 - \lambda).F1(t - 1) + m1.(1 - k2 - \lambda).F2(t - 1)$

And needs numerical resolution of F1 and F2.

10. Cited references Balesdent J., Basile-Doelsch I, Chadoeuf J., Cornu S., Derrien D. Fekiacova Z., Hatté C. Atmosphere-soil carbon transfer as a function of soil depth. Nature, 559, 599–602. (2018) doi.org/10.1038/s41586-018-0328-3

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Manzoni, S., Katul, G. G. & Porporato, A. Analysis of soil carbon transit times and age distributions using network theories. J. Geophys. Res. 114, G04025 (2009)

Mathieu J., Hatté C., Parent E., Balesdent J. Deep soil carbon dynamics are driven more by soil type than by climate: a worldwide meta-analysis of radiocarbon profiles. Global Change Biology 21, 4278-4292. (2015) doi:10.1111/gcb.13012.

11. Figure.

Simulated Δ14C of a well-mixed compartment under steady state as a function of compartment turnover rate, for two dates of sampling. Compartment has a single exponential distribution of ages; system start 8050 BP; atmospheric Δ14C after Reimer et al. (2009) and Hua et al. (2013); Northern hemisphere zone N2; May-August.

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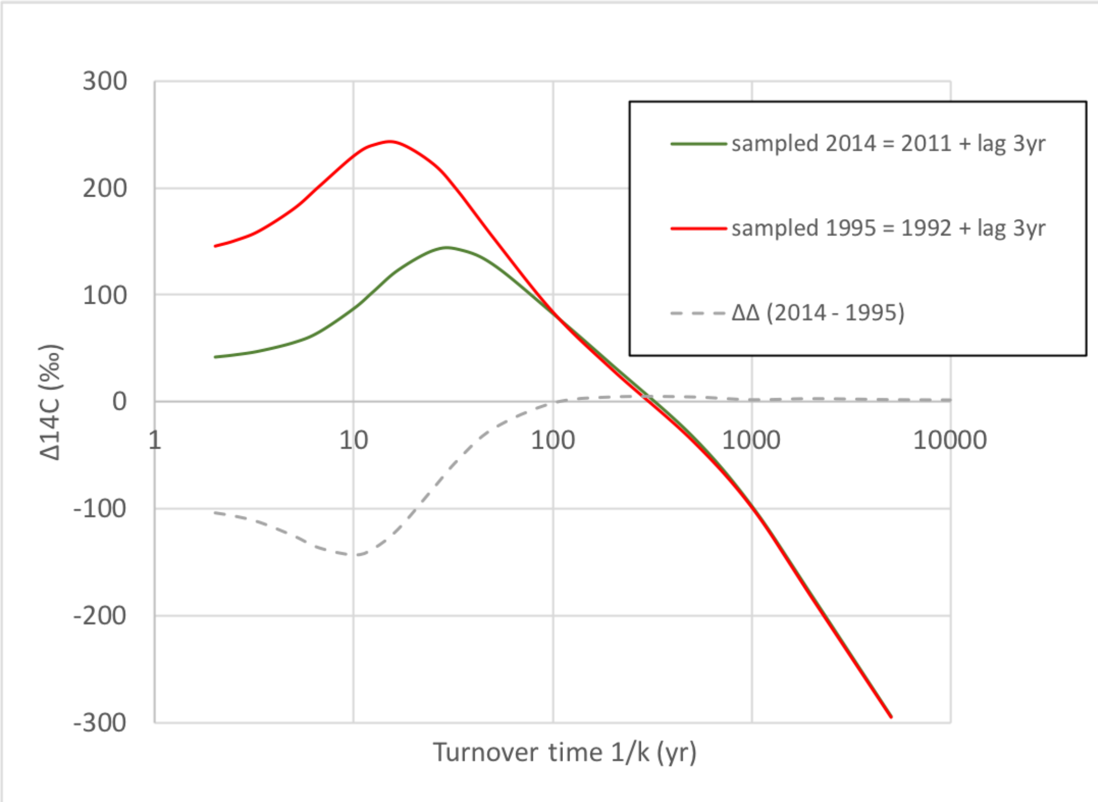

Fig. 1. Simulated Δ14C of a well-mixed compartment under steady state as a function of compartment turnover rate, for two dates of sampling. Compartment has a single exponential distribution of ages;

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