

Interactive comment on “A model based on Rock-Eval thermal analysis to quantify the size of the centennially persistent organic carbon pool in temperate soils” by Lauric Cécillon et al.

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We thank Reviewer 1 for his/her stimulating and constructive comments on our manuscript.

Major point 1: use of the concept of residence time in the manuscript

(1) Comment from Referee 1

One important source of confusion in this manuscript is the use of the concept residence time. Notice that in soils one must distinguish between the concepts of age and transit(residence) time (Bruun et al., 2004; Manzoni et al., 2009; Derrien and Amelung,

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2011). What the authors are trying to estimate here is an indication of the age of the SOM, not the residence time. These should be more clearly treated in the introduction and the discussion. Currently, the use of these terms is ambiguous.

(2) Response to Referee 1's comment

We agree with Reviewer 1 that it is important to distinguish between the concepts of age and residence time of organic carbon in soils. However, our methodology does not focus on either the estimation of the age nor on the estimation of the residence time of soil organic carbon (SOC). Our methodology aims at estimating the size of the centennially persistent SOC (CPsoc) pool, a SOC pool whose mean age and mean residence time are both assumed to be high (e.g. several centuries) but for which precise definitions are not necessary. The only required property for defining the CPsoc pool is not its mean age or its mean residence time, but the non-significant change in its size in periods inferior to the century. Specifically, we defined the CPsoc pool (gC.kg⁻¹ soil) at each site as the constant term of an exponential plus constant model fitted on the temporal evolution of SOC under bare fallow treatment. The only assumption that we made was that, given our data set (temporal evolution of SOC under 5 to 8 decades of bare fallow treatment or associated non-bare fallow treatment), the size of the CPsoc pool remained constant (i.e. did not change significantly, see also our response to major point 2 below).

(3) Proposed changes in manuscript

Overall, we thus did not make any specific estimation of the age or the residence time of the CPsoc pool, but we propose to treat this point more clearly in a revised version of our manuscript (introduction and discussion sections) to avoid any confusion regarding the concepts of age and residence time of organic carbon in soils.

Major point 2: centennially persistent soil organic carbon is not inert

(1) Comment from Referee 1

The other important issue in this manuscript is also conceptual. The model presented in equ. 1 and used to compute the centennially persistent pool is, in my opinion, inappropriate. It assumes that an amount of 'inert' carbon c is sitting there doing nothing and it will never decompose. This is highly unlikely, because there's always some small probability that carbon that is stabilized either by mineral association or protection in aggregates, would get consumed by microorganisms and respired as CO_2 .

(2) Response to Referee 1's comment

We fully agree with Reviewer 1 that organic carbon is not biogeochemically inert in soils vis-à-vis microbial decomposition, and that the CPsoc pool is also (though very slowly) gaining and losing carbon. However, regarding the CPsoc pool, its high residence time (e.g. several centuries; not precisely defined here, see above) masks changes in its size at the time scale of our sample set (5 to 8 decades). We thus pragmatically and operationally considered the CPsoc pool to be mathematically inert (the constant term in an exponential plus constant model) given our data set, though we agree that the CPsoc pool is a biogeochemically stable but not inert SOC pool. We argue that since all models are simplifications of reality, assuming an inert SOC pool is acceptable for a SOC pool with very low decomposition rate, as implemented in many widely used SOC models such as RothC. We have considered performing a double exponential (without constant) as suggested by Reviewer 1, but we think that a constant is mathematically the most appropriate way to model the CPsoc pool with our data (maximum 80 years of decomposition). Furthermore, we think that the argument that a double exponential model does not add parameters is not true: $y(t) = y_0 \cdot a \cdot \exp(-k_1 \cdot t) + y_0 \cdot (1-a) \cdot \exp(-k_2 \cdot t)$ has 3 parameters – but $y(t) = a + y_0 \cdot (1-a) \cdot \exp(-k_1 \cdot t)$ has only 2. Parsimony (Ockham's razor) and equifinality of more complex model lead us to propose keeping a single exponential plus constant model to estimate the size of the CPsoc pool (gC.kg^{-1} soil) in the bare fallow treatments of each study site.

(3) Proposed changes in manuscript

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We propose to revise the manuscript to clarify that the CPsoc pool is not biogeochemically inert in our view, but that it is mathematically more sound to mathematically simulate it as a constant on such a dataset.

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