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Biogeosciences Discussions

Interactive comment on "Fire-derived organic carbon turnover in soils on a centennial scale" *by* N. Singh et al.

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The authors analyze available data for the turnover time of fire derived or pyrogenic carbon (PyC) in soil. There are two points in their study that deserve further attention.

(1) The term 'meta-analysis' is often loosely used for any type of study analyzing an array of data. More strictly, however, meta-analysis searches for effect sizes by weighting the influencing factors. An example for that can be found in Luo et al. 2010, Agriculture, Ecosystems and Environment 139:224-231. I suggest to reconsider the terminology used.

(2) A single first order kinetics approach is applied to derive PyC turnover times. The authors refer to the literature and argue that 'because that allowed us to compute and

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compare turnover time of PyC despite the differences in type of PyC, experimental set up, or analytical method employed in various studies. Moreover, first order kinetics, meaning the rate of mass loss is a constant proportion of mass, is a simple and robust formulation that is commonly used to describe the turnover of SOM'. Here are two issues. First, the authors make an a priori assumption about the decay kinetics and the size and number of pools rather than to test different models and to select the best. It is true, second, that first order kinetics are commonly used to describe the turnover of SOM but it is also true that all recent SOM models rely on more than one pool, each of it having with its own reaction rate constant. This is because long term decay kinetics rarely follow a single first order kinetics. The introduction of a fast and a slow component would, if such a model equals or even outperforms the single first order kinetics, substantially change the computed overall turnover time of PyC. The authors seem aware of this point (under 2.1 they discuss this as a possible reason for differences in TT between short term and long term studies. This can be taken as evidence for the existence of more than one PyC pool) but do not take consequences. A more reliable decision on the type of decay could be reached by using those longer term studies where PyC content was measured more often than just twice. Also related to the number of components or pools is the history of land use and thus decomposition. Some of the reviewed studies represent field sites with historical input of PyC. Hence, at the onset of the 'experiment' (i.e. cessation of further PyC input) the soil PyC reflects different stages of decomposition, making any assignment of decay kinetics even more challenging.

Finally, I suggest to add the timings of measurements for the single studies in the supplementary table.

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