

Interactive comment on “Gone or just out of sight? The apparent disappearance of aromatic litter components in soils” by T. Klotzbücher et al.

Anonymous Referee #1

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I generally acknowledge that the authors try to pinpoint and discuss the apparent discrepancies between the fact that large amounts of aromatic compounds are entering the soil in dissolved phase and the fact that they are not found any more sorbed to the solid phase. The authors attribute this to the problems of analyzing lignin and claim that two methods (although completely independent and different in their analytical procedures, i.e. solid-state ^{13}C NMR spectroscopy and CuO oxidation) fail to identify these aromatic compounds. I suggest to carefully check the literature for solid-state ^{13}C NMR work that shows significant contributions of aromatic compounds (although mostly attributed to charred OM, see work by Knicker and coworkers, Skjemstad and coworkers). The authors need to explain why the technique fails to work for lignin-derived aromatic compounds, but does work well for other aromatic (and even more condensed) structures produced from fire impact. The problem that all compounds en-

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tering the soil after some decades leave the analytical window for and cannot be identified any more as specific plant or microbial derived compounds has been described and discussed previously and is not specific for aromatic compounds (see detailed discussion of the problem in Hedges et al. (2000)). It is also illustrated by the fact that all molecularly identified organic compounds in soil are younger than the mean age of SOM or their turnover is faster than that of bulk SOM (see Amelung et al., Adv in Agronomy, 2008 and later Schmidt et al., Nature, 2011). Thus I suggest to refer in more detail to these discussions. It is necessary that the authors reflect on these already published discussions. Generally, the paper is too simplistic in its reducing the story to aromatic compounds. The authors provide only a selected view on the pathways how organic matter enters the soils. The decomposition of roots is mentioned (although recent references on root biomarkers in soils are missing). However, the input of OM by rhizodeposition is completely ignored. Similarly, the authors consider only sorption of low molecular weight compounds to the solid phase as a mechanism for stabilization of OM in soils. Here again, recent concepts are ignored, e.g. the association of microbial cell wall envelope fragments (see work by Miltner and coworkers). It is necessary to point out that the view of the authors is mainly restricted to acid forest soils, whereas there is also stabilization in neutral pH forest and arable soils where the interaction of acidic compounds with Fe(hydr)oxide surfaces is of minor importance (Kleber et al., 2015). Even if one agrees with all the problems raised, the authors do not provide novel solutions. Solution one is that “careful data interpretation, including critical assessment of experimental and analytical limitations, must become standard”. This is a prerequisite of any scientific work and does not tackle the specific problem. The second solution is to use “combined studies on DOM and SOM”. Here I agree and I encourage the authors to start such investigations. The figure is just terrible; colors are almost not distinguishable from background. Here a more professional graphics approach is needed to improve the figure.

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