

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

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Dr. Feng: This paper focuses on the stability of aromatic litter components in the (mineral) soils and provides some interesting insights on the paradoxical evidence of persistence or lability of aromatic organic carbon (OC; including lignin) in the soil and dissolved organic matter (SOM and DOM). This research topic has received increasing attention in the past decades and this opinion paper is timely for synthesizing the mounting (controversial) evidence for/against the stability of aromatic OC in soils (although I feel that more literatures can be included). In addressing this issue, the authors have put a special emphasis on the analytical limitations of detecting and quantifying mineral-protected aromatic OC, which in my opinion is indeed a key bottleneck limiting our understanding on the fate of plant OC in the soils. I therefore wish that the authors may extend the discussions of future research strategies in the last part to

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provide some “practical” suggestions on potential research directions or tools to overcome the current analytical weaknesses. For instance, a key issue with the lignin CuO oxidation method is its unknown extract efficiency, which may vary for samples with different mineral matrix or SOM interactions. But this is rarely tested or stated in the papers because we are short-handed dealing with complex macromolecules such as lignin. There is hardly any pure natural macromolecular lignin standard for us to test the method. Can we circumvent this problem using pyrolysis GC/MS or isotopic labeling? Or a combination of different methods may provide further insights on the “hidden” lignin? Before any major analytical breakthrough is made, we must make full use of the current tools for soil lignin studies rather than abandon them, right?

Authors: Dear Dr. Feng, we thank you for the effort that has gone into evaluating our article. Your suggestions and comments led to a greatly improved manuscript. We agree that the part on future research strategies was too short and vague. The problems of quantifying total amounts of aromatics in soil may not be fully solvable with currently available analytical methods. However, strategies of combining different methods could help to gain improved estimates on how much aromatics might be ‘hidden’ in soil. We added a more comprehensive discussion on the problem that not all of the aromatics can be extracted from soil (page 7, lines 27 ff.). In addition, the last part of the manuscript has been entirely revised and extended. It now presents a list of possible future research strategies that might help to step forward on solving the problems outlined in our article. The list includes open research questions as well as suggestions on methodological approaches.

Dr. Feng: I also agree with the authors that there is a big gap between the long-term fate of lignin and short-term laboratory experiment including sorption studies, in which “hot moments” of lignin transformation may not be captured. For instance, sorption experiments that observed selective binding of aromatics to minerals are typically void of microbial interactions due to the use of HgCl₂ (and alike). In natural soils, aromatics that are presumed to sorb selectively to minerals may be preferentially degraded

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by microbial communities living at the mineral surfaces as well, hence leading to the paradox of “dissolved” and “solid phase line of evidence”. Inclusion of both natural geochemical and microbial processes in laboratory or field experiments may be key to finding the missing piece of the soil lignin jigsaw puzzle”. I believe this goes beyond mineral protection and analytical limitations.

Authors: We extended the discussion on current limits of process understanding (see chapter 4.2). One major aspect of the discussion is that the ‘fate’ of sorbed material is hardly understood. We agree, the view that DOM leached from decomposing litter sorbs onto mineral surfaces and then contributes to stable SOM might be too simplistic. Further transformations, desorption, and transport of the sorbed OM might occur in soil. Possibly this depletes SOM in aromatics.

Dr. Feng: Last but not least, as indicated by the title, the discussion of this paper is focused on plant derived aromatics (lignin and tannin). But how important is black carbon in the overall distribution of aromatic signals in the soil and DOM? Can we distinguish the two? I think it may be useful to briefly differentiate and clarify the fate of lignin/tannin versus black carbon in the soil for the readers’ benefit.

Authors: We decided to keep focused on plant litter-derived aromatics, but added a discussion on possible contribution of pyrogenic OM (page 8, lines 30 ff.). Current available literature on DOM mainly considers plant litter-derived aromatic compounds. Numerous studies addressed in the production of aromatics in forest floors during litter decomposition. Few studies are available on role of pyrogenic OM for DOM fluxes. Many studies on DOM use indicators for bulk aromatics (e.g., UV absorbance). Hence, the unknown contribution of pyrogenic OM to DOM is an uncertainty in the interpretation of DOM flux data.

Dr. Feng: Other minor comments: Pg 3, L13: I’m not sure if this sentence is totally true forest floor is the result of greater litter inputs versus losses through mineralization and translocation via leaching and bioturbation, etc. For tropical forests with strong litter

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mineralization, forest floor can be thin or almost absent as well. Actually, I don’t think it is necessary to mention the cause for forest floor formation here so this first sentence may be deleted.

Authors: We shortened the paragraph and deleted the discussion on bioturbation. We agree, it is a side aspect and not necessary to mention.

Dr. Feng: Pg 3, L15: decreases with depth.

Authors: Sentence has been removed.

Dr. Feng: Pg 3, L20: 25-89% is a very high estimate does this only apply to soils with limited bioturbation and to subsoils? What about root input? Does this DOC flux include root exudates, which should be differentiated from that leaching from aboveground litter? As root biomass decreases with depths, it is natural that root-derived DOC decreases in mineral soils.

Authors: We present more details on the work cited in this paragraph. The estimates are for acidic forest soils with permanent forest floor. Hence, there is indeed limited bioturbation. The model calculations base on measured DOC fluxes, DOM sorption and mineralization. The role of roots as source of DOM is not clear. In general, there is little information available on that issue. We discuss it in the revised manuscript (page 5, lines 25 ff.).

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