SOIL Discuss., author comment AC2
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Reply on RC2

Carrie L. Thomas et al.

Author comment on "Transformation of n-alkanes from plant to soil: a review" by Carrie L. Thomas et al., SOIL Discuss., https://doi.org/10.5194/soil-2020-107-AC2, 2021

We would first like to thank Anonymous Referee #2 for their critical review and helpful comments.

Anonymous Referee #2: *My main comment concerns the soil profiles. The authors described the trends, however for many results the data available are from surface to around ten centimeter. In many soils, these ten centimeters often only concern the organic layer. Accordingly, it should be clearly stated as it could be an important point. For example in figure 5a, we can observe important difference in n-alkane concentration in the first 20 cm and below. The distinction was better evidenced in figures 3 and 4. With the differences of scale, it is sometimes difficult to compare the evolution with depth (Figures 5 and 6). Perhaps it might be interesting to distinguish the trend between 0-20 cm and between 20 to deep soil in the discussion.*

Reply: We agree that for many of the longer soil profiles there is a difference in the trend between the upper and lower depths. For many of the profiles, there was only data available for the organic layer which as you mention is an important point to consider. We will revise our discussion in the future version to more fully address the effects of depth.

Anonymous Referee #2: *In the introduction, why do the authors specified that the study of lipids specifically could increase the overall understanding of SOM dynamics? Would it signify that the authors consider that all individual compounds have the same degradation and preservation pattern? However the authors mentioned potential preservation of n-alkanes. So the authors should improve how they would apply knowledge on alkane degradation on the whole SOM if they are better preserved.*

Reply:

Whether or not certain molecular component classes have a larger or smaller potential for preservation as compared to bulk SOM is currently a matter of scientific debate. While the currently prevailing paradigm has it that there is no intrinsic recalcitrance of certain molecular classes (e.g. Schmidt et al., Nature, 2011; Lehmann & Kleber, Nature, 2015), insights are emerging that SOM turnover rates are linked to functional complexity with an important role for variations in molecular diversity of SOM (Lehmann et al., Nature Geoscience,2020). Lipids constitute and important, and molecularly very diverse sub-class of SOM and as such studying lipid dynamics in soils under various pedogenic and environmental conditions will help further the debate on SOM dynamics.

We will include this clarification in the revised version of the introduction.

Specific Comments

Anonymous Referee #2: *L104: Replace where by when*

Reply: Thank you for the correction. We will fix in the revised version.

Anonymous Referee #2: *L 155 and L169 to 172: Are the trends noticed really significant statistically? In fig 3a perhaps it is significant for coniferous forest, mixed forest, and in fig 3b too, except deciduous but for fig 4 it is even more difficult to know if the differences are significant. Could it be possible to apply statistical analysis?*

Reply: In line 155, "significantly" is used in a colloquial rather than statistical sense. Although we contemplated completing a statistical analysis on the compiled data, we chose not to because we do not want to risk overinterpreting the data. Due to the limited size of the dataset that could be gathered, as well as differences across studies, it would be difficult to determine what populations are accurately represented in the data. We will remove the colloquial use of the word 'significant' in the revised version of the manuscript.

Anonymous Referee #2: *L226-230: I totally agree with the authors about the limitation. We need data to evaluate trends. Thus, the "trajectory plant-litter-soil" mentioned is perhaps too large because for coniferous forest and grassland the data are from very shallow layer (fig 5a), perhaps even only litter. It is difficult to write that it is a trend from plant to soil.*

Reply: We agree that describing our observed trends here as covering the entire trajectory from plant to soil is too optimistic considering the limitations of the data. Therefore, we will be more specific in the revised version.

Anonymous Referee #2: *L238: I do not remember that Jansen and Nierop (2009) discussed the production of alkane via alkene oxidation. The authors quote another citation for this possible source of alkanes.*

Reply: Thank you for pointing out this error. We will adjust the citations in the revised version.

Anonymous Referee #2: *L245-247: I do not understand how n-alkane degradation could result in an increase of n-alkane concentration.*

Reply: In these lines, we tried to briefly summarize how Wang et al. (2014) explained the unexpected results of their study. They did not say that n-alkane degradation would cause an increase in concentration, but that the process of degradation is complex due to multiple factors and that the unexpected increase in n-alkane concentration in the litterbags in a short-term experiment was evidence for this complexity. Thank you for pointing out that this was not well-phrased in the current version. We will clarify this in the revised version.

Anonymous Referee #2: *L290-291: How could earthworm influence the dis2009 should be Zech et al., 2010.*

Reply: Unfortunately, this comment is not entirely clear, but we would be very happy to address it if you are able to elaborate.