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Comment on soil-2020-107

Anonymous Referee #2

Referee 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-RC2, 2021

This paper aims to review plant n-alkanes and their transformation from plant to soil. The authors focused on data concerning the quantification of n-alkane but also on traditional indices (ACL, CPI and OEP). The study is based on 37 studies in which environmental, soil and vegetation data were taken into account. The authors observed the quantity, ACL, CPI and EOP from litterbag incubation, in plant-litter-soil continuum and in soil profile. They highlighted some trends: a decrease of n-alkanes concentration, CPI and EOP with time or depth. There are general trends however this study also showed a different evolution depending on the considered plant or considered site. This review is interesting despite the small dataset selected due to the limited number of suitable papers. The authors underlined the difficulties encountered in performing a meta-analysis on biomarkers and the need for accessibility of data and pointed to knowledge gaps and suggested recommendations. The manuscript depicts most of the outcome explaining the evolution of *n*-alkane pattern with time or in soil either due to degradation pathway or source shift. Perhaps the soil factors and the potential transfer of *n*-alkanes in soil could have been more detailed. Thus, I recommend this study for publication.

Nevertheless, I have a few remarks that must be answered. 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.

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.

L104: Replace where by when

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?

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.

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.

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

L290-291: How could earthworm influence the dis2009 should be Zech et al., 2010.