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Interactive comment on "From leaf to soil: *n*-alkane signal preservation, despite degradation along an environmental gradient in the tropical Andes" *by* Milan L. Teunissen van Manen et al.

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We thank Reviewer 2 for their recognition of our novel dataset and constructive comments that have helped us to develop our manuscript further. We respond to the general points raised, and then specific issues, below:

General comments

1. Reviewer 2 expresses concerns regarding our ability to disentangle environmental factors controlling the n-alkane pattern shifts given the extent of the environmental gradients studied; particularly with regard to: (a) the limited gradient in relative air

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humidity, and (b) a lack of consideration of pCO2.

a) We accept the reviewer comment on the gradient range. However, we do not feel that this compromises the integrity of our manuscript, because the focus on the manuscript is on the degradation process, not the environmental gradient. We address the entanglement of the environmental variables specifically in lines 232-235, and agree that disentangling them is not possible in this study. We will address the implications of the environmental entanglement more explicitly in the discussion section. See also our response under reviewer comment “Lines 83-89”.

b) We did not consider the gradient in pCO2, as there is evidence for a link between pCO2 and n-alkanes isotope signatures (via C3/C4 plant distributions)(e.g. Boom et al., PALAEO3, 2002), but there is no evidence for a link between pCO2 and n-alkane patterns in literature. However, there is strong evidence for a link between temperature, humidity and precipitation and n-alkane patterns (e.g. Bush and McInerney, OG, 2015; Hoffmann et al., OG, 2013; Tipple and Pagani, GCA, 2013), which is why we chose to include those variables. Additionally, our transect spans across a montane forest, where C3 plants dominate along the whole gradient. We do not see it fitting to include a discussion on pCO2 in this manuscript, but will address our reasoning for choosing the particular environmental gradient more explicitly.

2. Reviewer 2 expresses concerns regarding the overstatement of the novelty of the findings and that our findings are not sufficiently discussed and/or embedded in relevant literature. We thank the reviewer for providing suggestions of additional literature. We will include the missing literature reference provided by the reviewer (Schäfer et al, 2016 SOIL) and review our wording in the discussion and conclusion sections to not suggest novelty where it is not applicable. Specifically, Reviewer 2 suggests to include a discussion of why our results “do not show a shift in ACL during degradation in necromass and soils”, in contrast to what has been found in other studies (Wu et al., OG, 2019, Zech et al., GCA, 2011). We are unsure what the reviewer means by a “shift in ACL during degradation”, as our study does not track degradation over time as is

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done by Zech et al. (GCA, 2011). In the scope of our study, we interpret the comment to mean “discuss why the ACL of necromass and soil n-alkanes do not shift along the gradient”. We do not see a necromass ACL shift along the gradient due to the limited sampling along the gradient (lines 243-244). We do see a change in soil ACL along the gradient, which we have discussed in light of Wu et al (2019) (lines 302-303).

Specific comments

Lines 13-14 52-54, 257-260: We accept the reviewer comment. The aim of our statements was to indicate that compared to the large number of leaf wax n-alkane distribution studies, necromass and soil n-alkane patterns studied are limited. Especially in the context of the n-alkane patterns palaeoecological proxy literature, we find there is a gap of knowledge in the taphonomic processes that influence the interpretation of the proxy. Additionally, we find a limited number of studies that focus on n-alkane patterns (rather than quantities and isotopes) (such as Wu et al., OG, 2019) and are set in the ‘natural’ tropical settings (rather than temperate agricultural settings)(such as Wiesenberg et al., 2004; Zech et al., GCA, 2011). We appreciate the missing literature reference of Schafer et al. (2016, SOIL), but we do think it is fair to state there is a knowledge gap in our understanding of how taphonomic processes complicate/alter the interpretation of the n-alkane patterns proxy (in the tropics in particular). We will alter the wording to avoid any possible suggestion that we claim there is no available literature.

Line 54: the sentence lists studies that study and compare both plant material *and* soils, not all studies that have studied necromass *or* soils. We will reword the sentence to avoid confusion.

Lines 32, 33, 38: We are of course aware of the fact that extractable lipids in general, and n-alkanes in particular have been studied for decades (see e.g. a review article on this by one of the co-authors: Jansen & Wiesenberg, SOIL, 2017). However, the application of n-alkanes as palaeoecological proxy with a more detailed interpretation of the

signal than simply as indicator of input of terrestrial higher plant material is something that has only gained ground in the last 15 years. Still, even such application until now has been mainly focused on reconstructing past vegetation distributions, or via isotope signatures, past climatic reconstructions. The use of the chain length distribution patterns of n-alkanes as proxy for climatic conditions is indeed novel and, when further tested and developed, would add a valuable tool to the previously mentioned existing palaeoecological application of n-alkanes and/or their isotopic signatures. It is clear we failed to properly specify the novelty of the application we discussed in our manuscript. We will alter the statement so it is clear we mean to say that the application of n-alkane patterns as a palaeoecological proxy (not isotopes or other applications) are relatively new and under development.

Lines 38-40: We accept the reviewer comment. The sentence aimed to lists previous findings that suggest n-alkane patterns are a promising development in palaeoecological proxies. We will reword the sentence so we are clear about what aspect of the n-alkanes proxy we consider novel and promising (also see previous comment).

Lines 83-89: We accept the reviewer comment. Although the humidity gradient is short and close to 100% along much of the gradient, we chose to include the variable because we want to include all available environmental variables that have been found to relate to n-alkane distribution changes in previous studies (also see Reviewer 1 comment 2). Additionally, although the humidity gradient range of variation is only narrow, excluding it would introduce noise in the interpretation of the other correlations. We agree the limitations of the humidity gradient need to be discussed explicitly. We will include a statement acknowledging the limitations of the humidity gradient and the implications this has for the results on line 234.

Lines 203-205 and Figure 3: We disagree with the reviewer comment. We think that the nMDS of the sample types combined (Fig 3d) shows unambiguous overlap between the sample types, to the extent that we do not see added value in performing additional statistical tests to show that the sample types have similar n-alkane patterns

(as nMDS is a widely accepted statistical analysis to show (dis)similarity between samples). However, we acknowledge that the wording suggests statistical testing, and will change the wording to better reflect the analysis performed.

Lines 369-370: We do not think that the sentence “Taken together, our results and previous findings [. . .] suggest that ancient n-alkane signals likely carry environmental information similar to that observed in modern leaves, necromass and soils” claims (or suggests) (a) that sedimentary n-alkanes are constant or (b) that no other processes affect the interpretation of sedimentary n-alkanes. Additionally, the next sentence acknowledges the existence of other factors that complicate the interpretation of the sedimentary n-alkane record. We agree with the reviewer that it would be a stretch to claim that modern n-alkanes directly translate to sedimentary n-alkanes. We do not see it necessary to change the original sentence, but we will elaborate and include the factors suggested by the reviewer (transport and thermal overprinting) to the existing caveat sentence as additional examples of complicating factors.

Lines 375-378: We accept the reviewer comment. There is indeed large scatter in the correlations in Figure 5, in particular in the leaf sample data. On the particular line we will change “reflecting” to “correlate with” so it is clear we do not suggest the correlation is perfect. We will also include a statement that acknowledges the variance in these correlations, and what this implies for the palaeoecological proxy (section 4.3).

Technical corrections All technical corrections are accepted and will be incorporated in the manuscript.

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