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Interactive comment on "On the stratigraphic integrity of leaf-wax biomarkers in loess-paleosols" *by* C. Häggi et al.

Anonymous Referee #2

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The authors present a brief study which contributes to the discussion about the potential of n-alkanes and fatty acids as biomarkers in paleosols. These compounds are well known as biomarkers in marine and limnic deposits. In contrast to soil, the biomarkers in these sediments are mostly deeply buried after deposition and, hence, are only little affected by "contaminations" with younger organic matter. In loess and paleosol relicts in loess, organic matter may stem from three sources: the loess source region, the phase of soil formation, and from a younger soil formed on top. The authors use two independent approaches to exclude that organic matter from a younger soil formation contributes to the n-alkane load in the underlying soil relicts or in loess. First, they studied to which depth n-alkanes are deposited in considerable amounts by soil formation using two soil profiles in alpine sediment which had no organic matter in its source region. They hypothesize, that the lower boundary of lipid input to the subsoil

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and substrate indicates the maximum vertical transport distance of lipids in soil and sediment and hence the maximum potential of contamination of old sediment layers by younger sediment layers. The authors should present this approach more clearly in the abstract by adding information about the soil type and that they also sampled the substrate / subsoil. If they only speak of soil, it is confusing to hypothesize that there was no input of organic matter. It should be clarified why the radiocarbon age of the lipids was not detected in the sample set. Second, the authors sampled modern and paleosol from a loess section in Serbia (lipid patterns already described in Zech et al., 2013). Here, they present radiocarbon (14C) analysis of selected samples which allow dating the input of organic matter and of the lipids. This method is well situated to detect input of younger organic matter into older sediments. They present a very interesting result that the total organic matter is younger than the lipids (n-alkanes and fatty acids) in the loess and paleosol relicts. This is convincingly interpreted as a input of younger organic matter which remains insignificant for the lipid fraction (n-alkanes and fatty acids). In terms of restricted solubility and mobility of the lipids compared to the majority of total organic matter this interpretation seems adequate. I support that this manuscript is published in Biogeosciences. However, to my opinion, it needs minor improvements in the line of argumentation. The authors need to sharpen the definition of "the soil" and the related hypothesis (i). They need to clarify in the abstract and especially at the end of the introduction that they investigated the gradient from the topsoil to the subsoil and the substrate of the two soil profiles. It needs to be said that they expect no significant contribution of lipids in the subsoil and substrate. The discussion should have much more focus on that topic. The authors should compare lipid loads with other concentrations observed in topsoils, subsoils and substrates and evaluate whether this is little or not. The Niederbuchen site shows interesting variations in the lipid loads which were not discussed (Fig. 2b). Unfortunately, a 14C analysis is lacking here, I wonder if this might be related to a transformation of fatty acids into n-alkanes which is still ongoing in this soil (see also the discussion along the 14C data for Cr1). It should also be discussed how deep the input of lipids might potentially reach (consid-

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ering other literature data and comparing the two profiles in this study). The discussion of the loess section (Cr) in Serbia is consistent, however, the first subsection of the paragraph deals with the input of fossil carbon which was not introduced before. As this is not the main topic of your study this can be shortened considerably. Especially the part about marine organisms can be deleted. The heading of subparagraph 4.2 is not ideal, since fossil lipid input would also and exclusively be post-sedimentary (as long as you do not assume contributions of marine rocks or coal in the loess substrate).

Details: Abstract lines 8, 15: be more specific about soil, subsoil and substrate. Line 20 ff.: the mention of “odd-and-even homologues” and of “reworking” at this place needs more explanation. Page 2 line 3: sort Zech citaions according to year rephrase (i) carefully. At the moment you say here that soils have no organic matter. Chapter 2.1.2, line 5: delete “and” Chapter 3.1, last sentence: the message of this sentence should be given in the abstract. Chapter 4.1: your point “four”: did you analyze d13C in this study or is the data from another study? Give either a reference or an argument why you analyzed the d13C. Conclusions: to my opinion you have not proven that the amounts of lipids are negligible below the topsoil. You should do this by comparison to other soil profiles and to the average of detectable lipids in paleosols. In Table 1 you can provide a definition of FAMEs or better use fatty acids (also in the manuscript you vary a bit too much with the names of the lipid compound classes). The figures are ok, however, in my version the symbols (especially nodules) are not good to see. In Figure 2 you could change A and B to have it in the same order as in Figure 1.

Interactive comment on Biogeosciences Discuss., 10, 16903, 2013.