*In the introduction the authors mention that alkanes are analysed in archives due to the 'easy lipid extraction procedure', which in fact is not true or only a minor part of the truth. First, e.g. cold water extracts would be much more easier, because one does not need a proper set-up. Second, alkanes were found to be of chemotaxonomic significance in plant leaves (e.g. several papers by Maffei and Maffei et al.) and in sedimentary environments they were described to be of diagnostic value (e.g. Schwark et al.), therefore highlighting their significance. Third, they were assessed due to their assumed high recalcitrance and probability to be preserved in sedimentary and terrestrial archives (e.g. Kuder & Kruge and Xie et al.) and therefore assumed to reflect paleoenvironmental changes.*

→ We acknowledge that there are several reasons why n-alkanes are used as biomarkers in loess research. Therefore we wrote on p.9878 ll.18,19 "This can be partly attributed to the relatively easy lipid extraction procedure, which allows obtaining results even from very organic-poor loess samples." In l.15 we state "… n-alkanes are used to infer vegetation changes…" and in ll.20,21 we continue "…the compound-specific deuterium/hydrogen (D/H) ratio of n-alkanes is used to infer paleoclimatic information.". Please let us know if you think this is not sufficient.

*The authors are simply wrong, if they describe their own publication (Zech et al. 2011b) as the first one performing biomarker research on loess-paleosol sequences. Numerous studies have been performed on the Chinese Loess Plateau, starting (to the best of my knowledge) with Jia et al. in 1987 – already 25 years b.p. and have been continued until the recent past (like Bai et al., Xie et al. and others).*

→ Zech et al. 2011b is a review paper highlighting the potential and limitations of some of the recent biomarker and stable isotope approaches in loess research. Nevertheless, we will rewrite this sentence during revision to state this more clearly. Please note, that in the subsequent sentences we wrote and cited "One famous application is the use of amino acid racemisation for establishing geochronologies (Novothny et al., 2009; Oches and McCoy, 2001; Zech et al., 2008)." and "During the last decade […] namely n-alkanes came into the focus of organic geochemists focussing on loess research (Bai et al., 2009; Xie et al., 2003; Zhang et al., 2006)." and "… the compound-specific deuterium/hydrogen (D/H) ratio of n-alkanes is used to infer paleoclimatic information (Liu and Huang, 2005; R. Zech et al., 2011).

*The argument that GDGTs in loess-paleosol sequences are not contaminated by postsedimentary microbial activity is also not completely valid as another publication than Zech et al., but from Huguet et al. showed exactly the opposite in the vicinity of former roots in loess.*

→ We agree with you that GDGTs may be significantly contaminated by post-sedimentary microbial activity in loess-paleosol sequences. That's exactly the point we are making when citing the "growth depth effect" (R. Zech et al., 2012) on p.9878 ll.24,25.

*The next problem I observed was the argumentation regarding n-alkanes in roots. First, alkane concentrations can be higher in roots than in aboveground plant tissues – there are several publications available on this. Also the already cited reference Huang et al describes for some species higher and for other lower amounts of alkanes in roots when compared to shoot tissues (e.g. Carex and Polygonum species). Second, in addition to the alkane content that is measured in roots, the authors also ignore rhizosphere processes at all (compare Jones or other reviews). Roots are continuously active during their growth, releasing exudates, producing fine roots (which commonly die rather quickly and might not be visible any more shortly after their death) and also release particulate organic matter into the soil or sediment. When only roots are analysed, one does not get an idea regarding the released substrates, which can also contain alkanes (like fine roots or*

*particulate matter), whereas it is clearly documented in several publications that especially young plant tissues are enriched in alkanes. As fine roots are not easy to see (and even their remains are harder to see, probably with- out leaving e.g. pores in the sediment), it is very hard to clearly state: our sediment we sample is definitely free of any postsedimentary root remains. Furthermore, the authors forget about the probability that alkanes are released if other components of roots like e.g. suberin, fatty acids, alcohols or others degrade. Some of the mentioned components can be also transferred in watery solutions (like exudates are) until a large distance from the former roots and release some alkanes there.*

→ We acknowledge that one *Juncus* and one *Carex* species investigated by Huang et al. (2011) show n-alkane concentrations higher than 10 micrograms/g. However, at the same time the n-alkane concentrations of the other investigated species is <10 micrograms/g. For comparison, leaves often have thousands of micrograms/g! Unfortunately, we are not aware of other studies reporting on n-alkane concentrations in roots (except for M. Zech et al., 2012b). Therefore, we kindly ask you to provide the respective references you mentioned.

→ No, we do neither ignore rhizosphere processes and particulate organic matter transport, nor do we forget about possible n-alkane production by degradation of other components produced by roots. However, distinguishing between all these processes is rather challenging, and is it not the focus of our study. We agree with you, that roots cause an n-alkane contamination in subsoils/loess. The question is, however, "How high is this contamination in loess and does it prohibit using n-alkanes as biomarkers (both for vegetation and hydrological reconstructions) in loess research?" That's exactly why we use the $^{14}C$ dating approach in our study. We would highly appreciate if you could provide the references you mentioned showing that young plant (root) tissues are enriched in n-alkanes. We are not aware of respective studies.

*Regarding the comment by Chikaraishi and the anonymous reviewer, I agree that compound-specific 14C determinations are mandatory and probably also 14Corg measurements would be also useful, especially if short chain alkanes of another source than long chain alkanes are present. Otherwise, one gets only a mixed age, which tells not a lot. Investigation of other aliphatic compounds like hopanes, steranes, isoprenoidal alkanes and so on could contribute to a rough source assessment in the alkane fraction via GC/MS, which has not been performed. E.g. I see also pristane and phytane in the gas chromatogram, which can give some first insights into the degradation of the organic matter (see textbooks by Killops and Killops and Peters et al.). Furthermore, the authors state that investigations of other fractions than the alkane fraction should yield more results like fatty acids. As the lipid extraction procedure is 'easy' as they told in the introduction and they extract not only alkanes – why didnot they analyse other lipids and PAHs, too? E.g. Rethemeyer et al. have also analysed 14C fraction-specific for different lipid fractions of the same sample and interpreted the results in terms of contamination and sources of organic matter within the fractions. Thus, it is surprising, why only part of the work has been done here.*

→ As acknowledged in our replies to Dr. Chikaraishi and reviewer#2, we agree that compound-specific $^{14}C$-dating of individual n-alkane homologues would be highly desirable. But are they mandatory? Does the bulk n-alkane fraction $^{14}C$-dating limit the innovation of our $^{14}C$ mass balance approach as we suggest it for loess research? Does it not deserve to be published in your eyes? We would like to recall that Y. Huang et al. (1996, Org. Geochem.) and Rethemeyer et al. (2004, Radiocarbon) have already demonstrated that dating bulk n-alkane fractions is a powerful tool in biogeosciences. In our opinion, it is reasonable to apply this method also to loess research; even more when it is combined with the suggested $^{14}C$ mass balance calculation in order to estimate the post-sedimentary contamination semi-quantitatively.

Nevertheless, we fully agree with you that "only part of the work has been done". Therefore, we would be delighted to see your and/or other working groups feeling encouraged by the suggested $^{14}C$ mass balance approach and to see it being applied to other loess-paleosol sequences as well as to other biomarker classes (see conclusions p.9887 ll.21,22). As replied to Dr. Chikaraishi, we suggest including a statement in our revision emphasizing the need for compound-specific $^{14}C$-dating of individual n-alkane homologues in future studies.

*In terms of the analysed sample set, it remains questionable, why samples for 14C and OSL were not taken for the same depth (and partially for the same stratigraphic unit), but from different ones. This weakens the meaning of the results as the OSL ages show a large scatter and not a continuous increase between 2.5 and 8 m and e.g. for sample 21 it is not clear, which OSL age would be correct. Anyhow, for this sample, the interpolated line (how was it calculated?) does not get a reliable result. Probably, also in that depth identical OSL ages would be observed like for the OSL sample taken half a meter above, if one analyses them from the same depth.*

→ For $^{14}C$ dating, we chose samples with high n-alkane concentrations (see Fig. 1C) in order to yield high C-contents in the collected n-alkane fractions (→ more reliable $^{14}C$ results). However, we do not see why different sampling depths for OSL samples should weaken the meaning of our results? Within errors, the OSL-ages are stratigraphically consistent and allow establishing a reasonable and reliable chronostratigraphy. In addition, it should be noted that the OSL-results are in agreement with previous findings from the Saxonian Loess Region (Kreutzer et al., 2012; Meszner et al., 2011, 2012) (see p.9885, ll.6-9). Nevertheless, we acknowledged and highlighted in ll.14-17 and ll.27,28 that an age overestimation for the three lowermost OSL-ages cannot be fully excluded; although we have no evidence for this assumption. We suggest including in the revision that due to this unlikely but possible age-overestimation the calculated post-sedimentary n-alkane contaminations for the respective two lowermost n-alkane samples have to be considered as maximum values.
The interpolated OSL age estimation for sample 21 (32 ka) was obtained graphically, but it is reasonably in agreement with the calculated age of 31.2 ka based on a linear trend line through BT839 and BT840.

*In the results and discussion section the authors cite their own paper, when they relate C31 alkane as grass derived. First, Maffei and many others published numerous papers on that before, and also Maffei et al. observed that C31 is largely abundant in coniferous trees. Additionally, there are hundreds of other papers that describe the variability of alkanes in plant tissues and that there are numerous other plants than grasses, showing an enrichment of C31 alkanes.*

→ We cite Zech et al. (2012a), because in that very study a detailed discussion of the n-alkane patterns and the respective vegetation reconstruction for the here investigated loess-paleosol sequence is presented. Anyway, following your request we will add additional references here. Please also note that in Zech et al. (2012a) we highlighted that most coniferous trees have by two to three orders of magnitude lower n-alkane concentrations compared to most grasses and deciduous trees and hence coniferous trees presumably did not contribute significantly to n-alkanes in loess-paleosol sequences.

*Another comment regarding the source of organic matter in soil: The authors state that petroleum derived C cannot enter the soil, but exactly this was described by Lichtfouse in one of his publications and also others report such contaminations.*

→ Thank you very much for this comment. We agree that in principle a petroleum contamination in soils is possible and add "those" on p.9883, l.21 ("In <u>those</u> soils and paleosols where a petroleum contamination can be excluded, increased UCM humps may

indicate…"). In the here investigated Weichselian loess-paleosol sequence, we consider the risk of a modern petroleum contamination by human activity to be minimal.

*The approach using 14C ages or root remains from other areas for the calculation of post-sedimentary overprint in the Saxonian loess sequences is problematic. The best would have been to analyse a root from the same area and also its alkanes for 14C. Otherwise, the results on % overprint are somehow randomized. E.g. what would be the case, if rooting has occurred 15 kyears before present or 100 years before present? A kind of calibration curve (starting immediately after sedimentation and finishing today) would be the best approach to assess this, rather than using three randomly chosen values for the calculation. Also the probability of rooting during former periods of paleosol formation is possible and was completely ignored. One example on that: What would have happened, if during the period, where the gelic Gleysol at 4.2 m depth has developed, one period of rooting occurred and another one 3 kyears before present (or no rooting after 20 kyears at all)?*

→ We do not agree. Our 5 contamination scenarios are not chosen randomly, but represent the most likely contamination scenarios according to often seen modern root contamination in loess-paleosol sequences and according to the three so far published (Holocene) ages for rhizoliths in loess (Gocke et al., 2010 and Pustovoytov and Terhorst, 2004).

→ We did not ignore a possible contamination during former periods of paleosols formation and we are very grateful for this contribution to the discussion. In fact, if you have a closer look on Fig. 1 you can see that a post-sedimentary root-contamination during formation of the gelic Gleysol at 4.2 m depth (ca. 25 ka) cannot explain the underlying n-alkane age of 20.3 cal. ka BP.

*In general, root contamination is discussed to modify the alkane patterns in the mentioned profile, if 14C ages do not correlate with OSL ages. As in general I agree for profiles, where root contamination is proven (was not done for the profile), also other potential overprints are possible. One could be particulate transport through the profile, where also soot particles can be translocated, which would explain also the short chain alkanes in the distribution pattern. Alternatively also penetrating soil solutions containing DOC (including fatty acids) can be translocated and then might have released short chain alkanes at a large depth. It is also possible that short chain alkanes can be dissolved and transported in aqueous solutions as documented by Ferguson et al.*

→ We agree that different processes can potentially contribute to n-alkane contaminations in loess-paleosol sequences. And we are grateful for your comment as a valuable contribution to the discussion of our manuscript in BGD. However, given that distinguishing between these processes is beyond the scope of our manuscript and given that root-contamination is probably the most relevant process, we prefer not to include a more detailed discussion in the revised manuscript.

*Due to the abovementioned comments, I guess that not all of the conclusions can be confirmed by the data.*

→ We greatly acknowledge your contribution to the discussion of our manuscript in BGD. Nevertheless, we may kindly ask you to provide a more detailed statement, which conclusions exactly are not confirmed by the data.

*Unfortunately, I was named in the acknowledgements of the manuscript, although a proper discussion of the data and the drawn conclusions did not occur. Otherwise, some of the abovementioned comments should have been acknowledged by the authors by mentioning the critical points before submission.*

→ We apologize for not having asked your agreement to be mentioned in the acknowledgements of our discussion paper. We included you due to the abundant discussions we had during the last years and due to the discussion we had on Fig. 1 and the $^{14}C$ mass balance approach recently. However, following your request we will not include you by name in the acknowledgements during revision.