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Interactive comment on "Soil organic matter dynamics under different land-use in grasslands in Inner Mongolia (northern China)" *by* L. Zhao et al.

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REPLY TO REFEREE COMMENTS On behalf of my co-authors, I greatly appreciate that Dr. M.F. Dignac acknowledges the merit of our data, method and result. The reviewer also gave us valuable comments. Here, we try our best to address them.

1. Regarding the concern of "There is no reference for example to tri-OH alkanoic acids or diOH alkanoic acids ", Response: Tri-OH alkanoic acids and di-OH alkanoic acids such as 9,10,18-triOH C18 alkanoic acid, 10,16-diOH C16 alkanoic acid, 9,10-diOH C18 diacid were detected in some soil samples. Among them, 10,16-diOH C16 alkanoic acid are the most abundant, whereas others are only very minor. The concen-

trations of these compounds are much lower than those of α- or ω-OH alkanoic acids and α, ω-diacids. Since they are not specific for roots or shoots, we did not pay much attention to them. However, we marked the peak for 10,16-diOH C16 alkanoic acid in the figure S2 in the revised manuscript.

We have realized that the chemical compositions vary with plant species and tissues, as demonstrated by Mendez-Millan et al. (Mendez-Millan, M., Dignac, M. F., Rumpel, C., Rasse, D. P., and Derenne, S.: Molecular dynamics of shoot vs. root biomarkers in an agricultural soil estimated by natural abundance C-13 labelling, Soil Biology & Biochemistry, 42, 169-177, 2010). However, as mentioned in the introduction, our study sites have same dominant grass species, Leymus chinensis and Stipa capillata, but under different land use and management practices (see introduction section). So the effect of different vegetation on chemical compositions is minor in our case. We have already conducted preliminary analyses about chemical compositions for different plants from north China including grasses, trees and shrubs (shoot, leaves, barks and roots). The results showed that long chain ω-hydroxylalkanoic acids are much more abundant in roots than shoots of Leymus chinensis and Stipa capillata, whereas the α-hydroxylalkanoic acids are not such specific for grass tissues. These results will be wrapped up for another article.

2. Regarding the concern of "different chemical compositions in different vegetation species",

Response: It is true that chemical compositions vary with vegetation species. As a result, it is needed to consider species effect. However, in our case, as mentioned in the introduction (line 63-66), all grassland sites have developed in the same climate region and have same dominant grass species. So, the species effect on chemical composition of soil organic matter was minor in our study. This is one reason why we selected those sites for our study. Since vegetation species effect is minor, we can apply an enrichment factor to estimate different degrees of preservation among different source organic carbon (shoot vs. root) in Inner Mongolia grasslands. Nevertheless, we admit it is useful to examine chemical compositions of different grasses, particularly for those cases where mixed plants exist. In the revised manuscript, we add an comment on vegetation species effect in the conclusion as "Future studies will be: 1) investigation for other types of inert carbon such as charcoal to understand the response of different soil organic matter to land-use changes; and 2) estimation of biomass and chemical compositions for above and below-ground tissues for different vegetation."

Specific comments:

3. Page 6, line 180: "strong linear correlation". Is a r2 of 0.595 that strong? On Figure 4, it would be useful, first to precise that these are the values for each of the four soil samples taken at each site. And also to use different colors for the four different sites and indicate the means calculated for the different sites.

Response: Based on the statistical result ($r = -0.667$, $n = 16$, $p < 0.01$), we think it is better to say a significant correlation between δ13C and C/N ratio. In the revised manuscript, we changed "strong" into "significant". In our study, there are only four samples for each study site, so we did not perform statistical analysis for each site. Instead, we combined all samples for a statistical analysis.

4. Page 7, line 208-209: "These compounds were abundant in suberin of root and bark of higher plants". This is probably not the correct origin for the w-OH acids in the free lipid fraction, since suberin constituents are not recovered in the free lipid extract, but in the products of the base hydrolysis.

Response: We agree with the reviewer that intact suberin biopolymers can not be recovered in free lipid fraction. In our experiments, we indeed found small amounts of ω-OH acids in solvent extractable lipids. Similar results have been reported by Otto et al. (2005; Org. Geochem.) for Canadian grassland soils. Those ω-OH acids may exist as free monomers and thus can be extracted in free lipids. Since we are not sure if these free monomers are from suberin, we remove the sentence of "These compounds were abundant in suberin of roots and bark of higher plants (Otto and Simpson, 2006b;

Pisani et al., 2013)." in the revised manuscript.

5. Page 7, line 218: "and steroids". No steroids are indicated in Figures 5B and 6B. By contrast, a,w diOH acids are indicated in the figures and not mentioned in the text.

Response: There is no figure 5B and 6B in our manuscript. In the text, we mentioned steroids in lines 210-215, where we wrote as "Steroids including β-sitosterol, cholesterol, campesterol and stigmasterol were only minor components (< 0.21$\pm$0.14 mg g-1OC). Cholesterol is of a mixture origin from plants, fungi and animals, while other steroids are predominantly derived from higher plants (Otto et al., 2005 and references therein)." In figure S2, we marked two major steroids, namely C27 (cholesterol) and C29 (β-sitosterol) sterols. Other steroid compounds are not marked in Figure S2 due to too low abundance.

Regarding a,w diOH acids, we identified α,ω-alkanedioic acids but no a,w diOH acids in grassland soils. From line 233 to 235, we wrote as "The α,ω-alkanedioic acids including C8, C9, C10, C11 and long-chain even numbered acids from C16 to C28 varied from 1.68$\pm$0.51 to 2.38$\pm$0.38 mg g-1OC." In Figure S2 B, we marked several major long chain n-diacids with carbon number of 20 to 24.

6. Page 7, lines 225-227: "Branched alkanoic acids [: : :] reflect inputs from soil microbes". Microbial lipids are generally recovered in the free-lipid fraction. Why would they be present in the saponified extract here?

Response: From line 225 to 227, we wrote as: "Branched alkanoic acids [: : :] reflect inputs from soil microbes". Branched alkanoic acids are biomarkers for soil microbes, which are detected in both free lipids and bound lipids. Similar results have been reported by Otto et al. (2005) and Feng et al. (2007). The detection of branched fatty acids in saponified extracts of grassland soils reflects that part of these compounds are bound to soil minerals or other SOM polymers.

7. Page 8, lines 241-242: "The ratios C:V and S:V were calculated to estimate the

source of lignins". These ratios can also vary with the degradation degree of lignins.

Response: This is a good suggestion. In the revised manuscript, we rewrote those sentences. From line 241, we changed the sentences into "Because V-type phenols are more resistant to biodegradation than S and C, the ratio of C:V and S:V changes with degradation stage of lignins. Here, we calculated the ratios of acids over corresponding aldehydes (Ad/Al) of lignin phenols. Since aldehydes are oxidized into corresponding acids with lignin degradation by white-rot and brown-rot fungi fungi, the Ad/Al values are indicative of degradation stage of SOM (e.g. Hedges et al., 1988)."

8. Page 10, lines 304-305: "aliphatic and lignin biomarkers are preferentially degraded during the grassland degradation process. This explanation is consistent: : :". This is more an hypothesis than an explanation. An explanation would require to explain how and why these compounds can be preferentially degraded, or more rapidly degraded, and necessitate some some information on the plant inputs and on the dynamics of the microbial communities in these soils.

Response: In the revised manuscript, we changed "explanation" into "hypothesis" in line 316 and 329.

9. Page 10, line 315: "long-term decomposition in soils". More probably "long-term stabilization", since the decomposition of plant residues in soil is generally not considered as a long-term process.

Response: we accepted this suggestion and changed "long term decomposition" into "long term stabilization".

10. Page 10, lines 335-338: The meaning of this enrichment factor is not clear, since the inputs of the different markers might strongly vary in the different sites, depending on the vegetation type, plant species, primary productivity, environmental conditions.

Response: As we mentioned above, all studied grassland has same dominant grass species and similar climate conditions. So the effects from the vegetation type, plant

species and environmental conditions are similar among four grassland sites. This is the reason why we conduct study in these sites.

11. Page 11, lines 341: "changing allocation pattern of above and below-ground biomass of vegetation". Since no estimate of plant biomass input is proposed in this manuscript, the validity of this hypothesis on plant C allocation is difficult to evaluate. This last paragraph drastically indicates that plant input measurement would strengthen this study.

Response: We accepted this points. The measurement of chemical compositions in different tissues are needed, which is currently undergoing in our lab. In the revised manuscript, we emphasize the necessaries of estimating allocation of above and below-ground biomass. We added the sentence in the end of this paragraph as "In order to better understand this SOM change, the next step will be the quantification of biomass inputs in Inner Mongolian grasslands with different degree of human disturbance.".

12. Page 11, line 349: "dynamics of responses of soil organic matter". The composition was studied here, but not the dynamics, which would require the use of an isotopic marker, or a mass balance taking into account the plant inputs to soil.

Response: We could not find this phrase "dynamics of responses of soil organic matter" in our manuscript.

Minor comments: 13. Page 9, line 279-283: this sentence is not clear

Response: we rewrote the sentences as "Soil organic carbon and nitrogen contents in RG is about one-fold higher than in NG, which is likely attributed to that: (1) continuous potato cultivation and fertilization for two decades (1990-2010) resulted in the accumulation of more organic carbon and nitrogen in soils; and/or (2) banning grazing for 2 years (2010-2012) is benefit for organic carbon preservation in grassland soils."

14. Page 9, line 286: "became more positive". If this is a general trend described in

the literature, a more explicit expression such as “generally becomes” should be used.

Response: we accepted this comment and made correction as well.

15. Page 10, line 307: replace “newly soil fungi: : :” by “newly formed soil fungi: : Response: we accepted this comment and made correction.

16. Page 10, line 324: replace “thereby reduced” with “thereby reduce”

Response: we accepted this comment and made correction.

17. Page 11, line 343 and line 364: replace “resident time” with “residence time”

Response: we accepted this comment and made correction.

Interactive comment on Biogeosciences Discuss., 11, 5613, 2014.