

We are grateful for this reviewer's comments on our manuscript which have greatly improved the quality and readability of our paper and we appreciate the well based suggestions for wording and restructuring the manuscript. Based on these comments and suggestions, we have revised our manuscript in an effort to improve it and address the concerns.

Below is our response to the reviewer's comments (written in bold).

**p. 16231 Line 23-: The objective and hypotheses are best located at the end of the introduction part.**  
We restructured the introduction accordingly to implement this suggestion.

**p. 16233 Line 10-15: The authors analyzed only 3 soils – one per characteristic area of the erosion transect, separated into 3 depth profiles – in total 9 soil samples. If the author's haven't analyzed replicates per specific area, how does he authors take the spatial variability of soils into account aside from a proper statistic to focus on the relative differences between geomorphic positions?**

We see our work as a conceptual approach to analyze and illustrate the mechanisms of soil redistribution effects on C dynamics. (we added this sentence to the introduction). It is true that for a proper spatialization/mapping of the results, another sampling approach than the chosen transect design would have to be taken. However, this was not the aim of this work. The profiles presented in this paper have been carefully chosen among a range of profiles presented in our former work (Doetterl et al. 2012, GCB). Among these profiles, the selected ones are representing, again conceptually, the conditions at the three targeted slope positions in the clearest way as shown by the TRB and the CS137 data. Furthermore, Wang et al. (JGR, 2015) have recently published a modeling study based on the presented C fractions data, successfully predicting C turnover in this landscape affected by soil redistribution, further corroborating our study design and choice of profiles.

**p. 16232 – 16233: The authors should clearly state at the beginning witch fractions were analyzed and how this fractions were obtained (cPOM).**

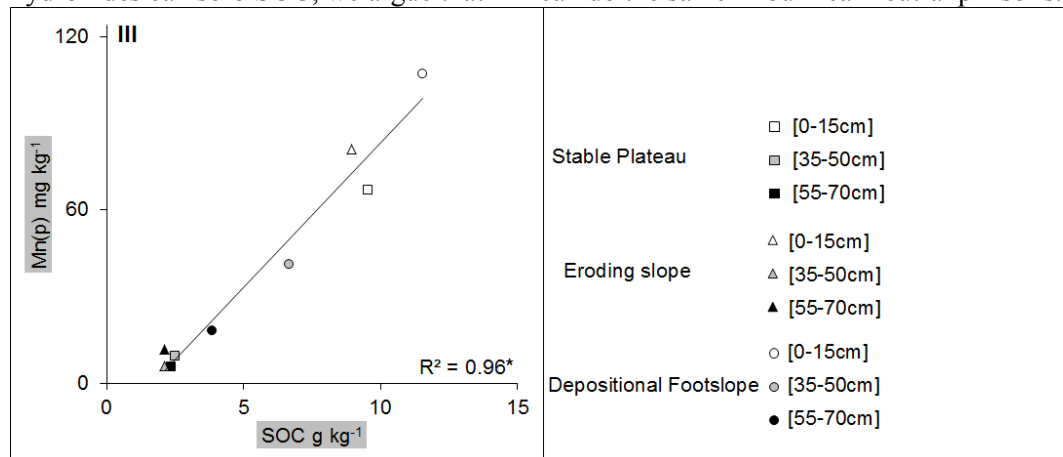
References to fractions not further detailed (CPOM) have been deleted from the manuscript. As we do not want to restate all methodological details of the former study, we added the following sentence: “*For further details on the gathered fractions see the original study Doetterl et al. (2012).*”

**p. 16237 Line 22: Why have the authors chosen this significance level?**

We would like to refer here to the principle of statistical hypothesis testing regarding **type I and type II errors** concerning the incorrect rejection of a true null hypothesis or failure to reject a false null hypothesis, respectively. A type I error is detecting an effect that is not present, while a type II error is failing to detect an effect that is present. Although the significance level of  $p < 0.05$  is surely more commonly used than  $p < 0.1$ , it is as arbitrary as 0.05 to indicate an effect is different from zero. Regression statistics, especially those applied on relatively small number of observations as in experimental studies are as susceptible to type 1 as to type 2 errors. This is why we have accepted a higher uncertainty in our significance level but restrain our interpretation of key findings in the discussion to those that are backed up by analysis of different origin (i.e. indicating high stability of C by amino sugar abundance AND high  $^{14}\text{C}$  ages).

The positive correlation between SOC and Mn seems to me a spurious correlation. Both parameters are strongly depth dependent. The SOC decrease for all fractions with depth and the Mn (p) decreases with depth, except s+c at the depositional site and for s+cm at the eroding and depositional site. Both exceptions show no correlation. The authors should be careful to draw a meaningful conclusion from it.

The reviewer's comments was of big concern to us, too. Please have a look at the figure below: When we plot samples from the same depth along the slope, we still get the same relationship of high Mn = high SOC (but with less samples and hence more shaky statistics). An exception is the 55-70cm eroding slope position, where very low SOC content shows no close association with Mn, most likely due to the very low (0.2% C) SOC content there and higher levels of Ca. No such consistent relationship could be found for the other tested metals (see revised figure 3). Hence, we argue that there is a valid connection between Mn and SOC described here that we try to explain via the mobility of these metals at certain pH levels (see discussion). Given the fact that other studies have illustrated frequently that Fe and Al oxy-hydroxides can sorb SOC, we argue that Mn can do the same in our near neutral pH soils.



p. 16242 Line 19-21: Why should especially Mn be important for the dynamic of SOC by promoting the formation of organo-mineral associations? What is the conception of the authors?

We think this is related to the mobility of Mn vs. Fe or Al. On p. 16243 l.29ff.(original submission) we wrote: *Third, the pH values in our soils are near neutral pH (Table 1), which is a pH buffer zone where Mn is highly mobile as Mn<sup>2+</sup>, potentially forming organo-mineral complexes, while the mobility of Fe and Al is strongly limited at pH>6 (Lindsay, 1979).*

p. 16245 Line 4-5: Where can the audience find the amount of aggregates in regard to the erosion transect and the depth?

This information is given in table 1. We clarified the table to make clear where to find this information.

p. 16229 Line 2-3: This first sentence of the abstract “It has been suggested that eroding landscapes can form C sinks or sources,” – seems no substantial statement for the beginning. The authors should better point to the specifics of a dynamic landscape in regard to organic carbon stabilization. Or mention that there is an ongoing discussion about the role of eroding landscapes in organic carbon stabilization. Suggestion: “The role of eroding landscapes in organic carbon stabilization operating as C sinks or sources have been frequently discussed, but the underlying mechanisms are not fully understood.”

We followed the reviewer's advice and replaced it with the suggestion above for the abstract.

**p. 16230 Line 12-18: Is this section about recalcitrance necessary? If I haven't missed anything, it is not of major importance for the discussion and the conclusions of the paper. So, why do the authors open a debate about biochemical recalcitrance?**

As one of the aims of this paper is to shed light on the complex interplay of factors controlling C turnover and stabilization, we feel that the actual discussion on recalcitrance vs. environmental factors should be part of the introduction to the manuscript.

**p. 16230 Line 26: largely undone? better: remains neglected (until now).**

We replaced “undone” with “neglected”.

**p. 16231 Line 5-7: rewrite sentence “, decomposition has predominantly degraded the more easily decomposable SOC fractions” suggestion – “During the transport of sediment and the accumulation at the deposition site, decomposition of easily available SOC fractions has predominantly occurred**

We replaced the sentence in question with the following: *“During the transport of sediment to and accumulation and burial at the site of deposition, easily available SOC fractions have been decomposed.”*

**p. 16231 Line 10-12: rewrite sentence – here it is hard to grasp the information the authors would like to point out. In the sentence before the authors mention that SOC at the depositional site is more stable, then the authors highlight that sometimes the depositional sites can store labile SOC. It is not clear which message the audience should take out of these sentences. Suggestion –“However, areas (or landscapes) with a fast burial can lead to the accumulation (storage) of labile SOC which is still vulnerable to decomposition if the conditions at the site of burial change. Thus, there is an ongoing discussion about depositional sites of highly dynamic landscapes as C sink or source. Soils at eroding sites are usually C depleted... “**

We followed the reviewers suggestion and restructured the sentence accordingly.

**p. 16232 Line 10: dot too much**

Corrected

**p. 16232 Line 1-18: This longer episode about amino sugars is interesting and important, but please incorporate it in the earlier introduction or moved it partly to the discussion.**

As part of the earlier suggestions of the reviewer, we restructured the introduction accordingly.

**p. 16232 Line 15-18: Sentence is really long and therefore it is hard to grasp the point.**

We split this sentence into two separate sentences to ease the understanding.

**p. 16238 Line 16: significant difference or trend?**

We replaced “trend” with “difference”

**p. 16241 Line 17 & 20: If abbreviations (AS - amino sugars) are used, please use it constantly throughout the whole manuscript.**

We corrected this throughout the manuscript where necessary. We only spell out AS in captions, headers and at the beginning of sentences.

**p. 16240 Line 10 & 16: If kaolinite is expected as partly inherited from the parent material, why are the kaolinite concentrations decreasing with depth at the eroding profile?**

If kaolinite is one of the end products in the weathering sequence in these soils it should be higher in more weathered soils (stable and depositional profile) than in less weathered soils (eroding profile). The deepest layer of the eroding profile is the least weathered part of soil along the sequence. Hence, Kaolinite is

ONLY derived from the parent material there, hence lowest in comparison to other samples. We added a sentence to the manuscript in section 4.2 to implement this line of thought.

**p. 16245 Line 6-7: delete one “first” ... suggestion “the depositional site is firstly induced by decomposition of C or by mineral weathering.”**

Corrected

**p. 16245 Line 6-7: “breakdown of aggregates at the depositional site is induced by decomposition of C first:” is it not a contradiction to the citation of p. 16246 Line 11-12**

Aggregate breakdown and aggregate formation can both appear at the same time. Aggregates composed of fresh litter and minerals might break down once the litter is further decomposed, while new aggregates form using remaining litter fragments after burial or transportation as observed by X. Wang et al (2014). Please also note that aggregate size classes play an important role. Newly formed Macroaggregates with old litter might be a lot smaller than aggregates containing higher amounts of fresh, hence larger, litter particles.

**p. 16245 Line 14-18: Rewrite this sentence.**

We replaced the sentence in question with: *“This is consistent with observations of Duemig et al. (2012) where higher SOC loadings of clay minerals were observed in consequence of a shortage of reactive surface area in clay depleted soils compared to more clay-rich soils.”*

**p. 16246 Line 3-4: C:N ratio or CN ratio**

Obsolete (section deleted)

**p. 16246 Line 10: “Von Lutzow” - uniform notation, please check your References!**

Obsolete (section deleted, but we checked it for the rest of the MS)

**p. 16246 Line 23: (AS) ?**

deleted

**p. 16247 Line 18-20: “.allow assessing information on the effectiveness of protection through a specific set of stabilization mechanisms.” – What? Please rewrite and set up the argumentation more carefully, so that the audience to follow your thoughts behind this statement.**

We replaced the sentence in question with the following: *“Hence, decreasing amounts of only certain fractions must be related to the decomposition of C within these fractions. Furthermore, comparing this decrease to C contents associated with other fractions allows assessing information on the effectiveness of protection through a specific set of stabilization mechanisms after burial.”*

Once again thank you very much for your time with this review,

The authors.