

Dear Editor and reviewers,

We are grateful for this reviewer's comments on our manuscript which have greatly improved the quality and readability of our paper. 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).

## REVIEWER 1 COMMENTS

**16230, 14: revised, or more simply, remove the comment saying that inherent bio-chemical recalcitrance is related to “poor nutrient” content. Biochemical recalcitrance is much more complex.**

We deleted the statement in question.

**You present here your objectives and the methods carried out to try to achieve them. While description of the methods for characterisation of SOC are well detailed, there is no information on the way you characterise the weathering status of soil material. Similar: you mentioned your expectations for SOC properties at the deposit site, but nothing is said about expected weathering status of minerals.**

We added the following text to the introduction:

*“We use the total reserve in base cations (TRB in  $\text{cmol}_c \text{ kg}^{-1}$ ) to characterize the weathering status of the soils along the transect and complement this with a qualitative and quantitative analysis of clay minerals. If changes in the mineralogical composition of the investigated soils due to soil redistribution act at a faster pace than weathering related changes, distinct mineralogical differences between profiles along the slope should be identifiable.”*

**SOC fractionation: It should be better explained, start by mentioning the fractions of interest for you that will be investigated in the rest of the study. It is not clear for me right now: On the one hand, according to Figure 1, I understand that microaggregates are different from the “s+cm” fraction: Microaggregates are more than 53 microns while s+cm are less. but I don’t understand the meaning of the arrow between the microaggregates and the “s+cm”. On the other hand, according to Table 1, I understand that microaggregate could be the s+cm fraction. Indeed, s+cm is nowhere, whereas the sum of % of bulk SOC in the macro, micro and non-aggregate = 100%.**

In our previous work along the investigated slope we extended the fractionation scheme to derive more detail in C fractions within the major aggregate and non-aggregated fractions. s+cm is hence a subfraction of the microaggregates. To clarify our motivation for the analysis of the respective fraction, we added the following text to the methods section on SOC fractionation:

*“For our analysis on the microbial composition and  $^{14}\text{C}$  age of the fractions we analyzed the environment in which C is stabilized, hence analyzing macro- and microaggregates as a whole. For the mineralogical analysis we are focusing on comparable fractions for the mineralogical analysis, hence comparing aggregated and non-aggregated silt and clay fractions.”*

**(please check sums: column 1 is 99%)**

Corrected

**Modify the title of 2.6. it should be “Age of C” instead of “turnover**

Corrected

**You discuss about C content in pyrophosphate extract p16242, l22. C content for each extract should be given in Results section if this is of interest for discussion.**

We added the SOC content as a second y axis to Panel I of Figure 2 and refer to it in the text accordingly.

**16240, l 12 to 20: these results should be in Supplementary Information.**

We argue that the information in line 12 to 20 should stay in the main text of the manuscript and not go into the Supplement. With this text we can give a valuable guideline to what in our opinion are the key findings presented in figure 4.

**The unit for the results on aminosugar is not clear. I assume you mean AS-C per g of C in the fraction, instead of AS-C per g of SOC (what refers to bulk total SOC)**

We corrected the units according to the reviewers suggestion. AS in fractions is now addressed as AS- per g of C and for the bulk soil as AS-C per g of SOC.

**Could you clarify the fate of organo mineral complexes when eroded from the stable position. Broken apart due to pH?**

We think it is too complex and uncertain to give a precise answer based on the gathered data in this study. Indeed the remobilization of organo-metallic complexes can be due to too many interconnected processes. Some could be the microbial decomposition of organic matter used in the formation of organo-metallic complexes (i.e. *Lundström et al. 1995. Eur. J. Soil Sci. 46, 489–496.*), or the impact of soil solution composition (i.e. *Berggren et al. 1998. Eur. J. Soil Sci. 49, 305-316*), or the re-complexation of new and fresh DOC by amorphous metals in organo-metallic complexes (i.e. *Schneider et al. 2010, Geochimica et Cosmochimica 74, 1606-1619*). But as it is not the focus of this paper to clarify the exact mechanism, we tried to avoid speculating on this.

**Discussion on old 14C age is bizarre. According to me, it is not necessary to mention charcoal, or this need to be supported by analysis, NMR for example. If you assume that high AS content in macroaggregate contribute to their stability, it can explain the age. High respiration rate is not contradictory, it just indicate there are different pools of C.**

We greatly revised the according section and removed the statements referring to charcoal and the postulated contradiction of high respiration and <sup>14</sup>C age.

**I would not insist too much on the fact that macroaggregates are proper habitat for microbes in conclusion and in the abstract, this is only a suggestion you make according to Deneff 2001) and (Kolb 2009). You have no evidence of living microorganisms and you do not know if aminosugar are young or old**

We feel that this statement can be justified, as our respiration data (presented in our former work [Doetterl et al. 2012, GCB] clearly shows that soils with large amounts of macroaggregate C also respire at a higher rate than soils low on macroaggregate C. We added a paragraph explaining this to the manuscript at the position in question.

**Please correct p16246 “microbial biomass can be found driven by the abundance of C as nutrient source.” C is not a nutrient itself.**

Corrected, we replaced “C” with “organic matter”

**Table 1. Give the unit in mg/ g fraction, not in %fraction**

We corrected the unit (now:  $\text{g kg}^{-1}$ )

**Table 5 is not very useful.**

We removed the table from the manuscript and the according references and discussion in the text.

**Figure 2. To which depth corresponds this Figure? Could you make subpanels for each depth? It would be very useful.**

We clarified in the legend of figure 2 that it includes samples from all depths and profiles. The main purpose of this figure is to show the distinct differences between pyrophosphate, oxalate and DCB concentrations between s+c and s+cm. depth or slope related changes are addressed in the correlations of table 3, and often not significant. We believe, therefore, that a three panel figure is the better choice than extending this to a 9 or 18 panel figure including all depths.

**Figure 3: what is really the unit? g per kg of SOC, what refers to TOTAL soil C, or g per kg of C in each fraction. Could you use distinct dots for each location and depth**

We clarified in the figure caption that the unit is g SOC per kg soil and revised the figure according to the reviewers suggestion.

**Figure 4: does it correspond to the clay size fraction?**

Yes, and we added: “*of the clay sized fraction*” to the caption.

**Figure 5: what is the unit? see similar question above. It looks like you do not recover all AS after fractionation. Could provide details about that? Also for parameters relative to minerals**

We clarified the unit according to the reviewer’s earlier suggestion for figure 3. Losses of AS after the fractionation have not been further investigated. We argue that these losses are related to free, floating POM and free AS that has been removed during the fractionation procedure. As we do not provide further data on this, and the focus of our work are the relative differences and trends along the hillslope for samples treated the same way, we did not add any (speculative) assumptions on this into the manuscript.

**I really like your new Figure 6. It would still benefit from some improvements. Some suggestions.**

**- Clarify if the information on the level of weathering indicated above or below the soil line should be taken respectively for top and subsoil.**

We clarified in the figure caption that the: “*Statement on the level of soil weathering along the transect refers to both top and subsoil*”.

**- The distribution of AS in the various fractions is impacted by the size of each fraction at each deposit site and depth. I would suggest another graphical representation if your intention is to show that macroaggregate have a high content in AS compared to other fractions.**

We modified the pie charts to show the relative contribution of AS in each fraction to total AS by multiplying AS per g C in a fraction with C stock estimates of the respective fraction given in table 1. The size of the pie chart now indicates the absolute AS content of these fractions combined in g per m<sup>2</sup> and per cm depth of a profile.

**- The figure on the top right: What’s about having a vertical line, with the age of subsoil at its right, and the age of topsoil at its left. Or use plot graph instead of cumulative bars.**

We followed the reviewers advice and replaced the figure with a plot graph instead of cumulative bars.

**Try to always provide for each location-depth information on (i) amount of secondary minerals, (ii) amount of aggregates, (iii) amount of organo-mineral complexes (you could indicate “isolated by PyroP, to make sure there is no confusion with aggregates).**

We added a scatter plot to the figure showing the amount of secondary minerals in top and subsoils along the slope. The amount of C<sub>pyr</sub> in the bulk soil is now given as a column next to each AS pie char. The Amount of aggregates has not been stated in this figure. We feel that the figure is already loaded with information but we state this information in Table 1. Furthermore, information on aggregates is derived from the former GCB data while we think this figure should focus on the new insights of this paper.

### REVIEWER 3 COMMENTS

**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).*”

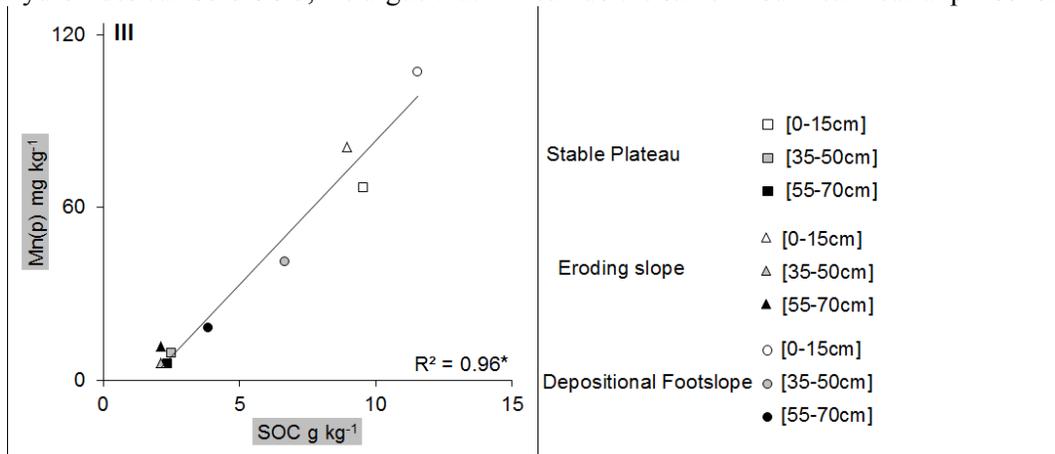
**p. 16237 Line 22: Why have the authors chosen this significance level?**

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 oxyhydroxides 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 reviewer’s 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.