

Interactive
Comment

Interactive comment on “Soil redistribution and weathering controlling the fate of geochemical and physical carbon stabilization mechanisms in soils of an eroding landscape” by S. Doetterl et al.

Anonymous Referee #3

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General comments

The manuscript by Doetterl et al. entitled “Soil redistribution and weathering controlling the fate of geochemical and physical carbon stabilization mechanisms in soils of an eroding landscape” (bg-2014-472), aims to clarify the effects of soil redistribution and weathering on physical and biogeochemical soil organic carbon stabilization. The paper addresses important issues for SOC sequestration in a highly dynamic landscape. The authors present several really interesting results.

Specific comments

Introduction

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p. 16231 Line 23-... : The objective and hypotheses are best located at the end of the introduction part.

Methodology

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?

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

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

Results

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, expect 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.

Discussion

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 author's?

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

Technical corrections

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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.”

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?

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

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 ... ”

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... “

p. 16232 Line 10: dot too much!

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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.

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

p. 16238 Line 16: significant difference or trend?

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

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?

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

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

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

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

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

p. 16246 Line 23: (AS) ?

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.