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Interactive comment on "Land use change affects biogenic silica pool distribution in a subtropical soil toposequence" *by* Dácil Unzué-Belmonte et al.

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We would like to strongly thank all referees for their thoughtful comments and their appreciation of the paper. The paper has strongly benefited from the suggestions. All minor revisions and rephrasing were accepted as suggested by the reviewers. The text was also checked by a native English speaker. In this response, we present a detailed overview of our responses to all comments.

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In this article, the authors question the impact of land use change in tropical environ-

ment, from forested to cultivated land, on the biogenic silica content of soil. They find that in addition to the known impact of harvest (which prevent plants Si to be returned to the soil), erosion also plays a role by moving the superficial, biogenic silica-rich, soil layer downslope where it can be buried. The authors find that increased erosion leads to higher biogenic silica mobilization, eventually leading to stronger biogenic silica depletion in soil. Deforestation and steeper slopes are found to be aggravating factors for this biogenic silica depletion. The study is globally sound and of good quality and is worth publishing. I do however have some minor concerns detailed below. 1 Erosion vs land-use: The land use change aspect could be discussed more thoroughly. The authors says several times (e.g., L281 and 296) that the impact of deforestation is clear and that the "study clearly shows how sensitive subtropical soil silica cycling is to deforestation". I don't think the impact of land use change is that clear though, and the authors somehow acknowledge it later in the text: "The absence of a larger decrease in the gently sloped cropland may indicate that deforestation occurred too recently to see such a decrease, [. . .]." The authors should be more moderated and discuss more in detail the limited difference of average biogenic AlkExSi content between forest and cultivated sites (gentle slope). They should also discuss more the change of the biogenic AlkExSi distribution within the profiles between the two land uses.

> We moderated the tone along the manuscript in relation to the differences between the two gently slopes due to the land use change, especially in Lines suggested.

Regarding the erosion aspect, it would be useful to give somewhere in the introduction some data on the erosion fluxes after deforestation. If such data are available in the literature, that would help the reader to get an idea of the importance of the phenomenon at a global or regional scale. Is it just an epiphenomenon or potentially a major Si sink?

> We included in Lines 54-57 some information about erosion in agricultural land: "In cultivated catchments, preferential BSi mobilization is associated with erosion

during strong rainfall events (Clymans et al., 2015). During such events, biogenic Si can represent up to 40

2 The authors used an innovative technique to estimate the biogenic silica content in soils. This technique allows differentiating between Si originating from biogenic silica dissolution and from soil minerals during a leaching. The authors discuss the biogenic alkaline extractible Si (AlkExSi) content in the soil profiles, but also spend a lot of time presenting and discussing the non-biogenic AlkExSi content in soil profiles. Nonbiogenic AlkExSi can somehow be seen as a proxy for geochemical and mineralogical change in the soil under anthropic pressure, which is interesting, but here the authors spend nearly as much time presenting and discussing the non-biogenic AlkExSi content in soil profiles as they do for biogenic AlkExSi, the object of the study, . . . to in fine say that there is no clear trend to observe. I do understand that a negative result can also be interesting, but in this case the part dedicated to non-biogenic AlkExSi could, to my opinion, be shortened. The potential interest of looking at non-biogenic AlkExSi data should also be clearly explained earlier in the text.

We reduced the section about the non-biogenic AlkExSi pool and made several changes along the whole manuscript in order to clearly focus the work and results on the biogenic AlkExSi pool. We still discuss partially the results about the non-biogenic AlkExSi pool in order to show that the description of this pool couldn't have been possible if other methods were used.

3 Regarding the writing, although the structure is globally good, the phrasing is sometime confusing and the manuscript would greatly benefit from some additional careful readings and reworking to improve the clarity. The Results section for example could be expurgated from long data descriptions that just repeat the content of the tables.

Several changes were made along the whole manuscript to improve the writing and the long data description in the Results section has been removed.

Other examples are given in the specific comments below. Specific comments: L 22: "that deforestation will rapidly deplete" should be "that deforestation can rapidly deplete" as one cannot generalize that easily the observations made here.

Changed in Line 22.

L 22 and 283: 10-53

The percentages are the differences between the Averaged pools. After adapting the Result section percentages are clearer in Lines 195-203.

L48: change "most relevant" into "most"

Changed in Line 48.

L55: ". . .can represent up to 40

Changed in Line 55.

L 99-100: This list of pits is indigestible. Please just mark the pits on figure 2.

We marked the selected pits in Figure 2 and included the reference to it in the text (Line 104).

L 129: To check the quality of what analysis?

Removed.

L 184 - 188: Again, this list is painful to read and the data are already in table 2 anyway. Please remove. Also, the number of digits after the comma varies for a same average in the text and in table 2 (E.g., 14$\pm$5.0; 14$\pm$5; 14.2$\pm$5). Please homogenize throughout the manuscript at the correct precision level.

Data list was removed and text from Section 3.3 was adapted.

L 189: I don't think these recalculated Si pools are "more accurate"; it's not a question of accuracy but rather a question of making it comparable to the cropland dataset.

Changed in Line 191 for "another average".

L 195 - 206: Here again, the data are not easy to read and to understand. Instead of repeating again the average data, maybe refer to the table 2 for the average values and just give the difference between the sites with different slopes.

Already commented above.

L 233: "adsorbed onto oxides" and "adsorption" not "absorbed onto oxides " and "absorption"

Changed in Lines 222 and 223.

L 240-246: I don't understand the point of this paragraph.

Unfortunately there are not too much data to compare with at this respect. Only few studies have measured biogenic silica pools in soils (mainly in temperate ecosystems) and with comparable methods. The aim of this paragraph is only to relate our results with other existent results and to explain the differences that emerge from that.

L 277: What does this "37perc" mean?

It is the accumulation calculated with Eq. 2 and included in Table 3. We changed in Line 264 the percentage for "(AC of 37perc)".

L 282: ILS > ILG > ARG > ARS, what are these acronyms? I could not find where in the text they were explained. S and G stand for steep and gentle, but the other letters?

Lines 267 and 307 acronyms changed.

L 290-294: I don't get the point here. Do the authors mean that the slope is a more important parameter than the land-use regarding the erosion intensity? Also, what are these 67

The percentage 67perc is the accumulation calculated (Table 2). We detailed: "(AC of 67perc)" in Line 283.

L 339-340: the higher abundance of clay mineral and oxides in more weathered soils is not really a specific feature of Cameroonian basalts, it is nearly a definition of soil weathering. The point the authors are trying to make here is not clear.

We rephrased Lines 320-323 in order to clarify that we do not mean that a higher presence of clay mineral and Si oxides is a particular feature of basalts from Cameroon, but that the cited study might be comparable to the rhyodacite bedrock of the sites from this study: "Weathering degree has previously been correlated to the amount of pedogenic silica accumulation in sedimentary soils (Kendrick and Graham, 2004). Further, clay minerals and Si absorbed onto oxides were reported by Delvaux et al. (1989) and Opfergelt et al. (2009) respectively, to be largest at most weathered sites in a study carried out in volcanic soils from Cameroon".

L 355: It is worth mentioning that some authors also vividly contest this hypothesis.

We included a statement about the existence of different opinions at this respect in Lines 345-348: "Although there are different opinions regarding this topic (Santos and Alexandre, 2017) some have suggested that atmospheric carbon sequestration could be enhanced through phytolith production and subsequent burial (Li et al., 2013; Parr et al., 2010; Song et al., 2016)." , and included the reference:

Santos, G. M. and Alexandre, A.: Earth-Science Reviews The phytolith carbon sequestration conceptâĂŕ: Fact or fi ctionâĂŕ? A comment on " Occurrence ,

turnover and carbon sequestration potential of phytoliths in terrestrial ecosystems by Song et al . doiâĂŕ: 10 . 1016 /, Earth Sci. Rev., 164, 251–255.

Figure 3 and 4: unless I missed something, these two figures tell exactly the same thing. The layout is slightly different and one is in mg.g-1 while the other is in kg.m2. . . and that's it. Is there any reason to keep both? I would also suggest to indicate the acronyms of the pits directly on the figure, to make the comparison with the table easier.

We consider essential the inclusion of the concentrations. AlkExSi concentrations were really measured in the samples. Calculated pools, although needed for comparison, include some interpolation, which make the pools an estimation and not real data. Moreover, we consider that the jump from the raw AlkExSi data directly to pools will be difficult to follow for the readers. We also included the pit acronyms in both Figures.

We would again like to thank you for providing the opportunity to substantially improve our manuscript, and we hope that our paper, which is the first to combine land use change and erosion in the study of terrestrial biogenic Si in subtropical soils, will be accepted for publication in Solid Earth.

Yours sincerely,

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Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2017-21/se-2017-21-AC3-supplement.pdf

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