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Biogeosciences Discussions

Interactive comment on "Evidence of old soil carbon in grass biosilica particles" *by* P. E. Reyerson et al.

Anonymous Referee #3

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General comment

This study aims to support the assumption that organic carbon occluded in phytoliths (PhytOC) has a non-photosynthetic origin. This has important implications for the global C and Si biogeochemical cycle and for the understanding of mechanisms controlling C cycling in soil-plant systems. The work presented here can help to move the field forward by enhancing our understanding of the coupled cycle of Si and C in terrestrial environments. This study is challenging the budget of OC stored in soil-plant systems as the origin of PhytOC is partially explained by uptake of organic molecules from the soil organic matter pool. The pathways of organic molecules uptake from a mix of old and young SOM pool and occlusion in amorphous silica in plants is however not yet completely understood. This manuscript has the merit to put into question the

use of PhytOC as a paleoenvironmental tool (proxy of plant C) and definitely demonstrates that a part of PhytOC has a non-photosynthetic origin and as such provides valuable perspectives in the field of OM translocations in soil-plant systems, and very promising research avenues in soil-plant interactions and SOM recycling. The results presented here could also highlight that a part of OM considered as stable in soil (old OC using 14C dating) is highly accessible for plant uptake and, as a consequence, can be rapidly recycled in soil-plant systems.

The assumptions stated in this study are supported by a very good dataset which is based on robust methodologies: use of reference materials/internal standard, interlaboratory measurements, replicates, experiments isolating atmospheric CO2 and SOM pool, 4 protocols of OC extactions from phytoliths. The procedures are very well explained and presented. However the result part is quite confusing and very tricky to read. As suggested by the other anonymous reviewer, I strongly recommend to completely re-organize this part of the ms and to focus on absolutely necessary data for the purpose of the paper. The discussion is very clear but sometimes, it is hard for the reader to understand on which data the authors based their assumptions.

Specific comments

- p15370, l2: seems quite confusing the use of PhytC instead of PhytOC, commonly used in literature I think so - p15370, l 1-15: it would interesting to read somewhere in the ms a bit more about the processes behind the mobilization of old SOM and the implication of this mobilization. - P15372, lines 23-24: what do you mean by "optimized"? - P15376, lines 14-23: this part is not clear. Could you please precise/clarify which extraction procedure is used for which SOM fraction. - P15380, line 8: you have to clarify which sample is analyzed. This is not clear which experimentation is carried out for which sample: 21 phytolith concentrates and 52 14C targets ?? - P15380, lines 13-14: what do you mean? Could you please clarify? - P15380, lines 15-16: when you discuss the extractions by the 4 protocols, the presentation of results are very hard to follow. Could you please find a better way to present your results? And what do you

mean by “phytC yield”? - P15381, lines 15-16: how do you explain the link between the extraction aggressiveness and the increase of discrepancies? - P15382, lines 3-8: this part is not clear/tricky to follow. Could you please rephrase/clarify? - P15382, line 12: the use of the terminology “protocol 2b, 1b,. . .” is not the very useful to make the ms easier to read, and to follow the results. Could you use another terminology in which we can directly understand the degree of aggressiveness of the extract? - P15382, lines10-15: this part is hard enough to follow - P15382, line 21: “showing that the inorganic fraction of the C-soil was. . .” is already a discussion of the results. - P15383, lines 10-25: the results are not clearly presented. It seems that we are already in a discussion part. - P15384, line 16: it could be good to find a better way to present this figure (Figure 2 a and b), which is not clear enough in the present state. - P15385 lines 8-16: quite interesting to see that different fractions of SOM are occluded in phytoliths. How can you explain that a part of OC is taken up by plants form a pool of SOM considered as old and stable in soil? - P15385, lines 17-20: the mixing equation should be further explained. - P15386, lines 1-5: not clear what is new compared to Alexandre et al. 2015? - P15386, line 8-11: Could you present a synthesis of the accuracy and reproducibility of extraction of OC and 14C measurement on your extracts? This would support your study and evidences. - P15387, 6-7: “Third, . . .” I don’t see how this part is supported by your data? - P15389: I don’t really get the reason why some authors measure 1.5-3% of PhytOC while Santos et al. measured <0.1%. Did you isolate environmental factors (type of vegetation, climate, geology, soil type) that can influence the concentration of OC occluded in phytoliths? Could you please clarify? - P15390: I fully agree as it is quite paradoxal to talk about PhytOC stabilization in soil-plant systems while phytoliths are known to be amorphous and highly soluble. - P15390, line 7: “10% phytolith stability” I would disagree to use this 10% factor as it will largely vary depending on environmental conditions such as, activity of elements (Si, Al, Fe, H+) in soil solution, morphology of phytoliths and thus type of vegetation, elemental concentration of phytoliths and thus soil type, . . . - P15390, lines 12-14: good to point this. . . - P15390, line 20-28: good to clearly highlight the implications of the study. But it would

be a good idea for the clarity of the ms to have a more detailed and clear presentation/discussion of your results (see general comment and the anonymous review#2). - P15391, line 27: why “in association with”? The transportation process would differ to the precipitation process. - P15392, lines 1-8: the role of oxalic acid exudates on the mobilization of OC and the likely uptake of the mobilized organic molecules is very interesting. But I don’t see the link with the dissolution of Si bearing phases during active Si uptake? Do you have scientific references to mentioning this? How can you state that the mobilized SOM is ready to be chelated with Si, as no scientific evidences support up to now a direct chelation process between Si and organic molecules? Could you please clarify?

Interactive comment on Biogeosciences Discuss., 12, 15369, 2015.