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

Anonymous Referee #2

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Review of Reyerson et al, submitted to Biogeosciences: Evidence of old soil carbon in grass biosilica particles

Introduction Many vascular plants produce siliceous bodies known as phytoliths, and these can contain carbon occluded into their structure – termed phytC. The δ13C of phytC and their 14C activities have been used for either paleoenvironmental reconstructions or dating, and recent work has repeatedly argued that phytC may represent a sizable atmospheric CO2 sink. But as Reyerson et al. note, there are several conceptual inconsistencies and knowledge gaps regarding our understanding of phytC, including a) poor knowledge of any relationship between δ13C of phytC and climate, and b) systematic offsets between phytC 14C ages and the plant tissue 14C ages. These problems may be compounded by the poor inter-laboratory repeatability of phytC

quantification. These issues led workers to posit a non-photosynthetic origin for some fraction of phytC, i.e. from some pool of 'older' soil carbon. This is the central issue that this manuscript seeks to address.

Reyerson et al. use a variety of experimental approaches, including above- and below-ground C isotope manipulations. They also incorporate aspects of an inter-laboratory comparison/protocol test, CO2 flux measurements and a phytC thermal stability experiment. As a result, and because the experimental protocols are not fully described, it is often hard to identify exactly what has been done. Therefore, although the conclusions seem sensible, it is often hard to assess the extent to which they are supported by the data. At the level of individual sentences, this is a well-written manuscript. But because of these issues, at level of paragraphs to whole sections, I find the structure to be confusing and difficult to follow.

In general, the manuscript presents useful data and sensible conclusions that will be of interest to many workers and fall within the scope of Biogeosciences. I recommend the authors spend some time re-evaluating the structure of the manuscript – particularly the results - and considering carefully the degree to which the extra analyses they performed actually contribute to the results and conclusions. These extraneous details might even include the FACE experimental setup. I think the manuscript would be much stronger if it was reduced to the key messages – which I understand to be that phytC δ13C and 14C can be explained as a mixing between soil carbon and photosynthetic (ambient) carbon, and that true phytC concentrations are lower than reported elsewhere.

Major comments The four different extraction protocols for phytoliths from modern plant material are referred to many times throughout the manuscript, but besides a brief indication in a figure legend it is not mentioned what they are or why some are more aggressive than others. At some points in the results/discussion, more confusion is introduced: e.g. what does it mean when phytoliths are 'extracted simultaneously using just one protocol'? Relatedly, the different extraction protocols clearly have a important

effect on the quantification of phytC (cf Pg. 15380-1) and on phytC δ13C (cf. Fig 4). Therefore it would be nice if there was some recommendations for future work. Also, are there inter-laboratory differences when using the same protocols?

The results are not presented in a clear and logical manner. To my mind, it would make more sense to separate first into the above- and below-ground experiments, and then have subheadings for the different analyses (phytC, δ13C, 14C...). At the moment, section 3.1 (for example) includes (i) phytC concentrations (ii) 14C results from all substrates (including some not mentioned before...) (iii) extraction protocol differences and (iv) CO2 fluxes. Overall, this makes for confusing reading.

I can not follow how 21 phytolith concentrates was arrived at (Pg 15376, ln. 8). If I understand correctly, there are six biomass samples from the above-ground experiment and 6 from the below-ground experiment. Multiplied by four labs and four protocols – this should be more than 21! And later (Pg 15380, ln 8) – why do they become 52 14C measurements?

The rationale for including the CO2 chamber flux measurements (section 2.2.3) and the thermal analysis (2.3.4) in this manuscript is unclear. When these are returned to in the results (briefly in section 3.2, and section 3.3 respectively), or the discussion, they seem to add little, and instead just distract from the main message.

Minor comments The figures could be larger, with more detailed captions. Pg 15379, line 9: what is 14C-free? Pg 15384, line 3: measuring, not measured. Fig 2b – is phytC 'acessibility' really what is meant here? Should it not be the opposite? Throughout – I can't follow the logic of calling the SOM pool the 'oldest' if only one measurement was made.

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