

Interactive comment on “Timescales of C turnover in soils with mixed crystalline mineralogies, Kruger National Park, South Africa” by L. Khomo et al.

Anonymous Referee #2

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GENERAL COMMENTS

Summary: This manuscript investigates the controls on carbon turnover and C inventory across soils ranging in mineralogy in Kruger National Park, South Africa. The authors sampled soil across a variety of parent materials resulting in differing mineralogical characteristics. Older soils with low contents of short-range order mineral phases were chosen to specifically test the impact of phyllosilicate clays on C turnover time and stocks. To elucidate mineral protection mechanisms, particle size and density fractionation was used in combination with ^{13}C and ^{14}C measurements. The main finding is that the content of high surface area, 2:1 phyllosilicates (i.e., smectite) is a better predictor for C turnover times in these soils than clay content (i.e., $< 2 \mu\text{m}$ size

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fraction). They conclude that analysis of phyllosilicate clay composition reveals clearer insights into C stabilization mechanisms than clay content alone.

Strengths: This manuscript provides a unique dataset in that it is one of very few that measured radiocarbon on different soil fractions (particle size and density) of the same soils. This approach puts the authors in a position to examine the usefulness of individual fractionation approaches for different soil system, and provides interesting insights into the mineral protection mechanisms responsible for C storage in low-SRO, phyllosilicate clay dominated soil systems. This manuscript will be of broader interest to the SOIL readership and I support its publication.

Weaknesses: 1) The authors don't draw a clear line between two definitions for 'clay', i.e. clay as a particle size category and clay as phyllosilicates. It would be helpful to be consistent with this terminology throughout the manuscript

2) The manuscript could use some editing. The introduction is not very concise and could state the research question more clearly. There are typos, somewhat confusing sentence structures and word choices, and mislabeled figures throughout (see specific comments below).

SPECIFIC COMMENTS

Abstract

P1L18: What does 'stabilized' mean in this context? Adsorbed?

P1L19-20: This is a speculative argument informed by the data presented here and should be framed as such. It is a valid point to make, but it should not be framed as a result.

P1L21-24: This argument is confusing to me. The authors state that the fraction that is not clay ($> 2 \mu\text{m}$) has much shorter turnover time than the clay fraction. They then attribute the shorter TT in the $> 2 \mu\text{m}$ fraction to weaker bonding of C to Fe and Al oxyhydroxides and kaolinite as well as the presence of more modern plant inputs (light

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fraction). The part that's confusing to me here and throughout the manuscript is the fact that I would consider Fe/Al oxyhydroxides and kaolinite as clays. They could be clays either because of their small size (oxyhydroxides) or because they are phyllosilicates (kaolinite). I understand the authors' point in the discussion that some of these minerals may have stuck to larger grains and so were removed with the $> 2 \mu\text{m}$ fraction. But I strongly suggest making sure you clearly separate when you talk about clays as size separates and when 'clay' means phyllosilicate.

L24: HF and LF are not defined

Introduction

P2L8: What is "dehydration bonding"? Do you mean ligand exchange?

P2L13-15: Poch et al. seems an odd reference here. It's work on clays on Mars.

P2L15-16: Masiello et al found correlations between pyrophosphate extractable Fe and Al and turnover time. I thought that pyrophosphate extracts poorly or amorphous Fe and Al phases, not crystalline sesquioxides as the authors state here.

P2L17-24: This argument seems very convoluted to me. Isn't the argument that organics on different mineral phases may exchange at different rates and therefore have different turnover times. Fractionation techniques average across a number of these interactions, and so there is a need to look into mineral composition of these fractions more closely to gain insights into what mineral phases provides the most protection (and thus the longest turnover time)? I see where the authors are headed with this argument, but this paragraph could be more concise.

P2L28: What is 'older C storage'. Choice of words (c.o.w.) is odd.

P2L32: Lawrence et al provide a large dataset, but it's not true they 'virtually' measured 'all' reactive components in the system.

P3L2-4: These two sentences makes little sense to me. I think it would help to frame

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the argument in terms of C stocks and turnover times, rather than ‘storage’ and ‘stability’. That applies to the remainder of the paragraph.

P3L8: ‘Trade-offs’ of what?

P3L9-10: I agree with this statement, but that is not a research question.

P3L10: Having this very broad goal stated here seems a bit misplaced. I think the authors should state the specific research question here.

Methods

P3L28: 105 yrs? I might be wrong, but shouldn’t it be longer?

P4L31: Typo, should be Plasma

Results

P7L19-20: where does it go up to 26%. The values in Table 2 range from 0-15%?

Table 2 was also published previously, better in the SI?

Table 3: I would include total C in HF (mass of C). There are also a few typos in the table header.

P9L7: I think the authors are referring to Fig. 5. The authors should check what figures are referred to in the following. It didn’t always seem to match up.

P9L21-29: Why does every sentence refer to Fig. 4? I’m not sure what is said here refers to anything I can see in Fig. 4.

P9L23ff. On the issue of whether or not H₂O₂ oxidizes mineral bound OM. I think it does, and it’s a mute point to argue it doesn’t. But the authors could at least find some refs to support the claim that it has minor impacts.

Discussion

P10L27: c.o.w. What are ‘C properties’?

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P12L6: c.o.w. 'sorbbers' is not a word. It's 'sorbents'.

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