

## General comments

First, I will note that I was not an initial reviewer for this manuscript. Therefore, I saw it after the first round of revisions and author responses. After my initial review, I did read through the previous reviewer comments and author responses. There is a chance I missed something, so I apologize if anything below was already commented on by reviewers and addressed/justified by the authors.

Overall, I found the topic of this manuscript very interesting and have a greater appreciation for the effects of how variable moisture regimes may influence soil organic carbon (SOC) stability by reading this work. In particular, I think the proposed question below is intriguing.

*“Will millennia-old SOC decompose rapidly once oxygen and moisture access is restored, or do the same properties that enabled its long-term preservation – fine texture, mineral associations, chemical recalcitrance – confer lasting resistance?”*

I think overall the paper is well written and addresses a complex and scientifically relevant topic and the authors revised much of the text in response to other reviewer comments and have improved the manuscript. However, I found some aspects confusing and believe that there are opportunities for improvement in clarity. I address those below in my general and line by line comments.

One particular aspect that I think needs clarity to improve the manuscript is for the authors to be very specific about what they mean by erosional, depositional, and burial zones/transects. For instance, clarifying that the burial transect is not equivalent to a modern depositional zone would be helpful. There is some discussion of this in the introduction, stating that it is historical geological events that caused the burial/deposition. However, when contrasting the geomorphic contexts as depositional vs erosional, that feels not quite accurate. Although the erosional transect is actively undergoing erosion, the depositional zone appears to be a “stable” rather than a “dynamic” landscape.

Thus, in a geomorphic framework, I think saying “depositional” transect may not be the best word choice. I think that “burial” or “buried” zone (used in many locations throughout the text) may be more appropriate since the zone is not actively undergoing deposition now - unless I have misinterpreted the background. In **lines 223 - 226**, the authors state the following - and I think this is the language they should be consistent with throughout the entirety of the paper to avoid confusion.

*“Sampling was conducted in 2016 and 2017 across two geomorphic settings: burial transects (where the Brady Soil is deeply buried beneath Holocene loess) and erosional transects (where the Brady Soil is exposed or shallowly buried due to hillslope erosion).”*

Additionally, I found the use of shallow, intermediate, and deep to describe “Depth” or “Soil Depth” in a few of the figures slightly confusing. If I am interpreting it right, this is the depth to the Brady Soil? In much of the text and captions this is referred to as “degree of exposure/burial” and relates to the Roman Numerals used in figure 1 and table A1. I think it

would be very helpful to call this variable “Depth to Brady Soil” or perhaps even say “Degree of exposure/burial” in the legends so that it matches the text and schematic for the field sampling.

I want to reiterate that I find this topic interesting and overall a well written manuscript. I understand the difficulties of sample collection and logistical limitations in the field. So with the samples available, I think the study design is appropriate. I do believe the manuscript could be improved from further clarity that might help it reach and be incorporated by a broader audience. I enjoyed reading this!

### **Specific line by line and figure comments**

**Lines 93 - 104:** This is a good addition and addresses some of the past reviewer concerns. I think it could be strengthened by additional references to recent work. One research paper that comes to mind is *Thaler et al (2021) - The extent of soil loss across the US Corn Belt*. Although it is not addressing paleosols, they are assessing the effects of soil loss/erosion on soil fertility and carbon that feels relevant to your motivation.

**Lines 112 - 114:** This is a very cool process! Just to clarify, is the rejuvenation of rock-derived nutrients here coming from new soil production via exposure of unweathered parent material (thus likely containing more rock-derived nutrients)? I think this sentence could benefit from a little more specificity. A suggestion may be “While erosion may remove C, the exposure of fresh relatively unweathered parent material can increase photosynthate additions due to the rejuvenation of rock-derived nutrients”. That change is up to the authors though.

**Lines 118-127:** I think this is a really interesting section. I think the discussion of soil properties (physical and chemical) and soil depth controls on SOC dynamics could be strengthened by recognition of some additional work. Maybe here in the introduction or later in the discussion. In particular the following papers may be worth considering. However, I understand if the authors believe they are out of the scope of the paper or are limited by the number of citations in the journal allows for.

- Lawrence et al (2015) - Long-term controls on soil organic carbon with depth and time: A case study from the Cowlitz River Chronosequence, WA USA
- Lawrence et al (2021) - The trajectory of soil development and its relationship to soil carbon dynamics feel like they could be relevant.
- Slessarev et al (2022) - Rock weathering controls the potential for soil carbon storage at a continental scale

**Line 122:** Is this reference to “Dolui et al. (in press)” different from the Dolui et al. (2026) paper? If not, the “Dolui et al. (in press)” needs to be corrected in many locations throughout the manuscript. If this is referring to an additional paper, it should be listed in the references list. Additionally, both initial reviewers pointed out that a typo of “(cite Dolui et al. i.e., paper 1)” existed. This is still present in section 3.1 and will need to be fixed (**lines 406-412**)

**Lines 145 - 168:** This section was easy to follow and a good review of these topics!

**Lines 170-172:** “This study investigates the sensitivity of modern and buried SOM to moisture inputs under erosional and depositional geomorphic conditions in the Brady Soil”. I am not sure that the term depositional here is totally accurate. I understand that different subfields may use this term variably, however is this an active depositional zone? I think the term burial (used in some spots) or maybe “past burial” or “buried” may be better to use. Mostly, I am not convinced (based on the background information provided) that this area is under a depositional geomorphic condition. It was a depositional zone in the past (just like the modern erosional setting). I am convinced that the erosional transect is undergoing erosional geomorphic conditions currently though!

**Lines 223 - 226:** Relates to comment above - I think this is a better way to say it and the authors should try to stay consistent throughout the paper.

*“Sampling was conducted in 2016 and 2017 across two geomorphic settings: burial transects (where the Brady Soil is deeply buried beneath Holocene loess) and erosional transects (where the Brady Soil is exposed or shallowly buried due to hillslope erosion).”*

**Line 235:** 0-30cm for both modern and Brady Soils?

**Line 254:** Again, is “(Dolui et al., in press).” a different paper from the 2026 one referenced in line 263?

**Lines 286-287:** Perhaps this is described in references to the model, but it could be helpful to state generally what the timescales of “slow” and “fast” cycling are for this study.

**Line 292:** biological replicates? Since there are multiple types of replicates it may be helpful to be overly explicit when referring to them.

**Lines 307 -309:** How quickly did you dry the soils/how fast did this process take place?

**Lines 347-349:** Did you measure respiration during the drying phase?

**Line 371:** Small note - Paleostatus is capitalized in some locations and not others. Is there a format the journal suggests?

**Line 371-374:** “Depth” here refers to the degree of burial or exposure right? I think the authors should try to be consistent with terms. Or be explicit and call this “depth to Brady Soil”.

**Line 401-402:** Would it be useful to explicitly state the pH ranges for the modern vs Brady Soil if the authors say “especially the Brady Soil”?

**Line 406 and 411:** “(cite Dolui et al i.e. paper 1)” seems out of place...

**Lines 407-408:** Units for the CEC should likely be included.

**Line 426:** Just to clarify, I am interpreting “greatest depth” here to mean largest burial (roman numeral III in that transect) and least erosion (roman numeral I in that transect). If that is not correct, I am misinterpreting something somewhere and more clarity may be useful.

**Line 485:** Not to hyperfixate on this, but the “deepest layer” here is slightly confusing. Again, it might be more clear to say “greatest depth to Brady Soil” if that is what the authors mean. I could see there being confusion with the measured soil depth/layer. However, the authors explicitly state that all are from 0-30cm (of the modern and the buried Brady soil). So it *shouldn't* get misinterpreted. However, I had to remind myself of that throughout this section.

**Lines 574-597:** I maybe missed something (reference to a specific pool or control) between these two statements or got a little lost in the results here. Both of the statements address the decay rate of fast cycling modern soil in the continuously wet vs wet-dry cycling, correct? But have different statements.

- Line 574: The decay rate of the fast-cycling SOM pool in modern soil was significantly higher in the wet-dry cycling experiment (8.11 day<sup>-1</sup> i.e., 0.123 days TOT) compared to the continuously wet experiment (0.127 day<sup>-1</sup> i.e., 7.89 days TOT,  $p < 0.001$ ), indicating a much faster turnover response to rewetting.
- Line 594: In modern soils, however, there was no significant difference between decay rates of fast-cycling SOM in continuously wet versus wet-dry cycling experiments.

**Lines 672-673:** May consider providing a citation for this “*Reduced decay could arise from physical protection within aggregates, reduced microbial access to substrates, or changes in solute diffusivity.*”

**Lines 693-694:** Could the authors reference a table where the data shows the 2000 times slower decay rates for slow vs fast cycling pools?

**Lines 694-695:** Are these turnover times (TOT) for the total bulk soil? Or a specific pool? I assumed at first one of the pools because of the sentence before, but I think it is for the bulk soil.

**Lines 702-708:** I think the discussion section is much improved from the initial submission. I wonder if at the end of this paragraph it would be useful to cite *Rasmussen et al (2018) - Beyond clay: towards an improved set of variables for predicting soil organic matter content* or another research article. The authors mention their inability to resolve chemistry to it may be useful to cite something referring to the complexity of detangling texture and mineralogical controls? Just a suggestion!

**Lines 743-745:** I think this is a great idea for future work!

**Lines 777-791:** The topic of this paragraph is a great addition to the discussion section. I think it could be strengthened by referencing past work addressing geomorphic and topographic

controls on soil properties and organic carbon dynamics. These are a few suggestions that may be relevant. Perhaps they are addressing a different timescale than you are interested in, but I believe the discussion could be improved by including a wider perspective on these controls.

- Yoo et al (2006) - Spatial patterns of soil organic carbon on hillslopes: Integrating geomorphic processes and the biological C cycle
- Patton et al (2019) - Topographic controls of soil organic carbon on soil-mantled landscapes
- Hunter et al (2024) - Geomorphic controls on the abundance and persistence of soil organic carbon pools in erosional landscapes

**Figure 1a:** I think this schematic helps a lot with visualizing the field sampling scheme and is very clean! I have some suggestions for clarity. The lidar in 1a is very intriguing and really demonstrates what the landscape looks like.

- Because the distance along the x axis in 1b appears to be relative to a reference point, is it possible to flip it so the direction of the burial and erosional surfaces matches the photograph below? I understand if that doesn't make sense though.

**Figures 2 and 3:** If these terms are referencing the “degree of burial/erosion” it may be useful to say that and reference the Roman Numeral scheme the authors included. Or call it “Depth to Brady Soil” explicitly.

**Figure 4 and 5:** Are these field depths? 6 cm is quite shallow compared to the depth at which samples were collected. I think maybe it is supposed to be meters?

**Figure 5:** Small note, I think the formatting got messed up somewhere, the p value is off of the figure in the PDF I am looking at.

Again, I thought this was overall very interesting work! I found it interesting to read and address a societally and scientifically relevant question. I think some clarification will greatly improve the manuscript's reach.