Response to Reviewers
Biogeosciences

March 2026

Dear Editor and Referees,

We thank the Referees for their thorough comments on our manuscript. We have carefully considered each point and provide our detailed responses, along with the corresponding revisions made to the manuscript below.

Yours sincerely,

On behalf of the authors
Reija Kronberg and Mari Pihlatie

Reviewer 1

Line 27: “Below field capacity” could range from quite wet to very dry; please provide a percentage of field capacity as the target moisture content at the first usage of this treatment in the text/figure captions (may not be necessary in the abstract if characters are limited).

> Percentage of field capacity is now provided instead of “below field capacity”.

Line 44: While I know this is a short summary, and cumulative is mentioned in the prior sentence, I suggest stating "total CO2" or similar here to avoid misinterpretation of the sentence as written.

> Revised as suggested. To remain within the 500-character limit, the mention about two specific soils was removed.

Line 48: Sentence is missing a comma or transitional word (e.g., "by"); suggest revising for grammar.

> Revised as suggested.

Lines 70-71: Some clarification is needed in this paragraph to explain whether you are strictly referring to OM mineralization during the saturated period, or both OM mineralization during the saturated period and upon re-drying, or upon re-drying only. The first sentence suggests that during saturation, C mineralization could be greater than in an aerobic soil, which is a different idea than "substantial" (but still probably lower than aerobic) sustained CO2 production due to anaerobic respiration, and also distinct from vulnerable OM mobilized during saturation which is released upon the return of aerobic conditions. Given the discussion text, where the drying phase it is emphasized (and related references cited, e.g., Huang et al., 2021), it seems that this text integrates references focusing on C dynamics during and after saturation, but it comes across as though the prior work is focused on “during” only.

> This section has been revised in response to the referee’s comment. It now more clearly refers to total CO_2 emissions, rather than only those occurring during waterlogging. In addition, we clarify that waterlogging may not have as strong an effect on CO_2 emissions as commonly assumed and may in some cases even enhance them.

"Contrary to conventional theory, recent studies suggest that temporary anaerobic conditions may maintain or even increase total C mineralization in mineral soils, relative to static aerobic conditions."

Line 72: Suggest specifying "temporary" anaerobic conditions here.

The term "temporary" has now been defined more clearly.

"Yet, uncertainties remain concerning the response of soil organic matter (OM) decomposition and carbon dioxide (CO_2) efflux to relatively short periods of very high soil moisture (weeks to a few months; i.e., temporary waterlogging) in cultivated mineral soils."

Lines 75-76: Rather than just amount of reducible Fe, technically this would be the quantity of reducible Fe-associated OM. I would also suggest stating directly "increased OM vulnerability due to Fe reduction" or similar rather than referring to the "mechanism."

Revised as suggested.

"Naturally, an adequate amount of reducible Fe that binds OM is a prerequisite for increased OM vulnerability due to Fe reduction."

Line 77: This is an important addition, but what is "substantial"? Please provide a percentage estimate here (as was discussed in the response to review).

Provided as requested.

"In the soils studied here, for example, short-range-ordered (SRO) Fe oxides represented ~50% of all pedogenic Fe (Kronberg et al. 2024)."

Lines 85-87: Grammar of sentence unclear; has an extra comma. Also, suggest making the following text a new paragraph focused on substrate.

The grammar of the sentence was revised and the paragraph on plant carbon inputs was separated into its own section.

"Because soil redox reactions are predominantly microbially catalyzed (Sigg, 2000; Stumm, 1995), the availability of labile C substrates stimulates microbial activity and O_2 consumption, thereby promoting the formation of reducing conditions and associated Fe redox transformations (Khan et al., 2019; Muhammad et al., 2021; Winkler et al., 2019)."

Line 110: Extra period/space.

Removed.

Line 112: "during and after..." Do the hypotheses apply to both periods?

- The first two hypotheses were revised to clarify the periods they refer to.

a. *Temporary waterlogging mobilizes Fe-associated C leading to increased soil DOC content during waterlogging.*
b. *Temporary waterlogging does not reduce the total cumulative CO_2 emissions from either soil.*

Line 144 (Figure 1 caption): Since FC is used throughout the figures, I wouldn't recommending changing this, but please also state "(50% of WFPS)" when describing this treatment in this figure caption, and the first place the treatment is mentioned in the methods.

Revised as stated in our response to the first comment.

Line 213: Superscript for R2.

Corrected.

Lines 251-252: See comment below about interpretation of "barely insignificant" p-values; a note here about your interpretation of p-values between 0.05 and 0.10 would be helpful.

Line 323: Below, p=0.08/p=0.09 are considered "barely insignificant" while this p-value (p=0.06) is considered not significant. Looking at the figures, I don't think that soil type looks to be a major factor, but suggest using consistent verbal interpretation of model p-values as a general rule.

The term “barely” was removed to improve simplicity and clarity, and the sentence was slightly modified.

“According to the post hoc tests, the difference between the CC and No CC monoliths in the second cycle was not significant in either water treatment in sandy loam soil (p=0.08/0.09) or in the 70% FC treatment in silty clay (p=0.09).”

Line 370: Small "n" for consistency with above figure captions.

Corrected.

Line 377: Suggest using "10 cm" (rather than “topsoil”) here for consistency with figures and how the other depths are presented.

Revised as suggested.

“At 10 cm depth the contents of all dissolved C species (TDC, DIC, DOC) were significantly higher in silty clay than in sandy loam soil as illustrated by the significant soil term (for TDC, DIC, DOC) as well as the significant interaction of soil and waterlogging days (for TDC, DIC) in the mixed effects models (Table S4-S6).”

Line 432: Suggest editing to "the subsequent drainage period."

Edited as suggested.

Line 470: Missing parentheses.

Corrected.

Line 496: No comma between flux term and could.

Corrected.

Lines 545-547: I agree with the authors that the apparent reducing potential does not align with actual contribution of anaerboic respiration, and that aerobic microsites might sustain aerobic respiration more than might be expected or accounted for. This work is in preprint, but speaks to the role of pore heterogeneity in mediating response to temporary saturation and may be of interest to consider related to this interpretation: Harman-Denhoed, Rachael and Leewis, Mary-Cathrine and Dutilleul, Pierre and Liebermann, Hannah P. and Kallenbach, Cynthia M., Soil pore heterogeneity buffers microbial communities against an extreme wetting event. Available at SSRN: https://ssrn.com/abstract=5579545 or http://dx.doi.org/10.2139/ssrn.5579545.

We thank the reviewer for pointing us to the interesting preprint by Harman-Denhoed et al., which highlights the importance of soil pore heterogeneity in mediating microbial responses to transient saturation. However, we have chosen not to cite it, as it has not yet undergone peer

review. We instead incorporated Keiluweit et al. 2017 as a reference, which supports the role of spatial and temporal variability in soil structure and redox conditions in shaping microbial metabolic processes. Overall, we do not want to broaden up the discussion in this regard as these were not directly measured in our study and additional elaboration would remain speculative.

"Finally, we want to bring up the possibility that O_2 entrapped in soil pores upon water saturation could have formed aerobic microenvironments (Williams and Oostrom, 2000) where aerobic respiration could go-on and contribute to an increased soil DIC and TDC content. Huang & Hall (2017) speculated that anoxic-oxic interfaces may play an important role in the observed increases in C mineralization during waterlogging periods. The porous and heterogenous soil matrix often leads to significant spatiotemporal variability in soil redox conditions as well as microbial metabolic rates and pathways (Fiedler, 2000; Keiluweit et al. 2017). Thus, despite the low redox potentials measured in our study (Kronberg et al., 2024), aerobic microsites may have facilitated aerobic instead of anaerobic respiration, thereby contributing to CO_2 production during waterlogging."

Line 580: The implication for total CO2 emissions is important ("good news for climate change"), but suggest revising to less informal language or softening language slightly, given the additional considerations brought up after and the general limitations of it being a mesocosm study, etc.

The first paragraph was slightly modified based on the comment.

"A recent study showed that many agricultural fields in Finland already experience periodic waterlogging during late autumn when transpiration is low and rainfall is high (Mattila and Vihanto, 2024). This highlights the urgent need to apply mechanistic insights on the effects of temporary waterlogging on C dynamics from laboratory incubations to field scale, with our mesocosm study acting as a crucial bridge between the two. Our results suggest that waterlogging outside growing seasons might not significantly affect net C mineralization in cultivated mineral soils in a cool, humid climate. Accordingly, the associated risk of soil C loss and resulting climate feedbacks may be smaller than anticipated from laboratory incubation studies, which have largely focused on tropical and volcanic soils (e.g. Bhattacharyya et al., 2018; Huang et al., 2020; Winkler et al., 2019)..However, our study focused merely on the effects of altered soil moisture on CO_2 fluxes and dissolved C dynamics, without

accounting for potential impacts on C inputs, soil N dynamics or on production of another potent greenhouse gas, nitrous oxide (N_2O). It is also important to recognize that besides soil moisture, climate change-induced shifts in e.g. off-season soil temperatures and freeze-thaw cycles are likely to affect greenhouse gas emissions and soil C sequestration capacity (Heikkinen et al., 2022; Liu et al., 2024). Thus, studies that evaluate the net impacts from these factors in an integrated manner are warranted in the future. "

Line 589: Suggest editing to "in the future."

Added "the" as requested.

Line 593: Suggest editing "weather" to "climate."

"Weather" was replaced by "climate" as suggested.

Lines 601-603: It might be worth emphasizing that the lack of Fe dissolution as a primary driver is suggested to be due to sustained aerobic respiration in this system, rather than a contradiction of the underlying mechanism of OM mobilization after dissolution of Fe-OM associations per se.

As discussed in the manuscript, sustained aerobic respiration may have contributed to the observed patterns. However, the primary processes underlying continued CO_2 production in our system remain uncertain based on the available data. Our intention in the final summarizing paragraph is to focus on the main hypotheses of the study. We think that expanding this section to emphasize specific alternative pathways, such as sustained aerobic respiration, would introduce unnecessary complexity and potentially dilute the main message.

Referee 2

L24: "plant growth"

We respectfully disagree and have chosen to retain the original wording for the sake of simplicity. Our intention is not to emphasize plants, since they were not the primary focus of this study, but rather to describe an agricultural growth cycle. We believe that the context makes the meaning of 'growth' sufficiently clear."

Treatments should be clearly explained in the abstract. I suggest:
L26-27: “while in the other half soil moisture was maintained below field capacity”
L30: “within these water treatment groups (waterlogged and control)

Revised as suggested

“In turn, during all three off-seasons, half of the monoliths were subjected to waterlogging lasting seven weeks, while in the other half soil moisture was maintained at 70% field capacity. Within these water treatment groups (waterlogged and control), the monoliths were further divided into two plant treatment groups: in half of the monoliths, an overwintering cover crop (Festuca arundinacea) was grown, while in the other half soil was left bare for the off-seasons.”

L38: “After the onset of drainage of the waterlogged samples”

Modified as suggested.

“After the onset of drainage of the waterlogged monoliths, CO_2 fluxes from both soils increased more than predicted based on changes in soil moisture and temperature, likely due to the release of previously accumulated CO_2.”

L51: “usually”? “commonly?”

“Traditionally” was exchanged to “commonly”.

L69: I would remove this sentence “which provides protection against biodegradation (Cotrufo et al., 2019; Lavallee et al., 2020), at least in static oxic conditions.”

We appreciate the reviewer’s suggestion, but we have chosen to retain this sentence. There is no space limitation in this section, and we consider the sentence necessary for maintaining clarity and completeness.

L76: Probably remove “more”

Removed as suggested.

L78: “is however not”, instead of “has, however, not been”

We think that present perfect is an appropriate tense here because it describes a pattern observed across studies up to the present. If we would only refer to different soils, present would be preferrable.

L115: Probably remove “plant”

Removed as suggested.

L130-137. I would simplify this part. For example: “Mesocosm allows assessing the role of soil structure and plant cover on c flows and transformations, while maintaining the control over environmental conditions, but they have been rarely used. In our 1.5-year greenhouse experiment, we studied C dynamics in intact soil profiles under controlled temperature and water conditions to capture processes more representative of field conditions”

Revised as suggested.

“Mesocosms allow assessing the role of soil structure and plant cover on C flows and transformations, while maintaining the control over environmental conditions, but so far they have been rarely used. In our 1.5-year greenhouse experiment, we studied C dynamics in intact soil profiles under controlled temperature and moisture to capture processes more representative of field conditions.”

L132: remove “scale”. To my understanding, there is not a laboratory scale.

Removed.

L163: Not clear why you use a 2012 reference for a 2020-2021 sampling. Does it refer to the auger used? Maybe you should place the reference just after “soil auger”.

The reference refers to the sampling methodology used, and it is now placed after “soil auger”.

L169-170: I would put this before explaining the differences, because this statement justifies the representativity of the selected soils and thus their relevance. On the contrary, the differences in texture are relevant to hypothesis 3, only.

Modified as suggested.

“Cylindrical soil monoliths (d, 15.2 cm; h, 63 cm) were collected with a tractor-mounted soil auger (Uusitalo et al., 2012) in November 2020 and May 2021 from two agricultural fields in Southern Finland classified as Eutric Stagnosol (60°16'23.4"N 24°56'40.6"E) and Eutric Cambisol/Mollic Umbrisol (60°49'07.3"N 23°45'54.8"E) with silty clay and sandy loam USDA texture, respectively. These soils represented typical OM

(Fernandez-Ugalde et al., 2022; Heikkinen et al., 2013; Lemola et al., 2018) and SRO Fe oxide contents for the region. The two soils differed in their hydrological characteristics. The dense structure and fine texture in the silty clay soil limited the movement of water through the profile causing water stagnation and repeated alteration of oxidized and reduced conditions resulting in the formation of stagnic properties (see Kronberg et al. 2024). In contrast, the coarse texture in the sandy loam soil facilitated an efficient water movement and transport of solutes into deeper soil layers. In the profile brown colors indicated a release of Fe from primary minerals.--"

L228, "...sensor hole sealings, compromising complete waterlogging in the topsoil"

Modified as suggested.

"In the second study cycle, some unfortunate leakages occurred in some sandy loam monoliths through sensor hole sealings, compromising complete waterlogging in the topsoil (further discussed in Kronberg et al. 2024)."

L234: Probably remove the reference to the CH4 analyser

Removed. Mention of methane fluxes was also removed from the beginning of the paragraph and from headings.

L271: "the fraction of CO2 flux"

Modified as suggested.

L396-399: you can simplify the formulation: "once normalized per root biomass, cumulative CO2 fluxes without cover crop were 3.7-6 times higher than with the cover crop Fig S2)"

Simplified as suggested.

"Once normalized per root biomass, cumulative CO_2 fluxes without cover crop were 3.7 to 6 times higher than with the cover crop (Fig S2)."

L418: Remove "In the empirical temperature and moisture dependency model fit to our data,"

Removed as suggested.

L448: I think the abbreviations have been introduced already.

Explanations of the abbreviations were removed.

L451, and others: please, carefully revise the reference to tables that can´t be found. I spotted several starting here until the end of the results section.

All false references have been now corrected.

L538: I suggest removing "while CO_2 production was lower in the sandy loam topsoil compared to silty clay," specially because you discuss earlier on the tlack of clear differences.

This section aims to explain why the difference in cumulative CO_2 emissions between the two soils was smaller than anticipated. We acknowledge that the original sentence was complex and difficult to follow, and it has now been revised for clarity.

"In sandy loam the increase in DIC was more modest in the topsoil than in silty clay but the content increased more evenly at all soil depths. This suggests that while CO_2 production was lower in the sandy loam topsoil compared to silty clay, higher microbial activity at deeper depths has increased the total respiration per area, thereby narrowing the difference in cumulative CO_2 emissions between the two soils."

L571-576: Does this add to the overall work? Honestly, I don´t think so and I recommend full removal

The whole section considering CH_4 was removed.

L606/section 4.2.1.: you mentioned that the trapping of CO2 in the water-filled soil pores decouples CO2 efflux from CO2 production. This is true, but the pattern you observed in rather due to the subsequent release of the trapped CO2 upon drainage. I recommend to structure the argumentation from this perspective, as this will is indeed help explaining the pattern you describe in the L629-635 paragraph. In addition, be careful with the statement "Thus, the observed decrease in CO2 efflux likely stems from a decreased gas transport rather than production only (L631-632), it is not clear what you mean".

This section has been restructured and now presents the key message more clearly.

> *"Increasing soil DIC concentrations—reflecting the accumulation of CO_2 within soil pores—indicated that during waterlogging, C mineralization continued at a higher rate than would have been inferred from CO_2 flux measurements alone. In our experiment, accumulated CO2 (ΔDIC) was at most ~30% of the total CO2 production during waterlogging (sum of cumulative CO2 fluxes and ΔDIC) which is in a good agreement with previously reported values (Maier et al., 2011; Sánchez-Cañete et al., 2018). The accumulation of CO2 results from impeded diffusive gas transport under waterlogged conditions (Greenway et al., 2006), causing CO_2 to be retained in the soil and released upon re-drying, when a peak in CO_2 efflux was observed. Consequently, the discrepancy between CO_2 production and measured efflux demonstrates that momentary soil CO2 efflux should not be considered equivalent to soil respiration as the two are decoupled in transiently high soil moisture events (Maier et al., 2011; Ryan and Law, 2005; Sánchez-Cañete et al., 2018). Thus, the immediate response of CO_2 efflux to soil waterlogging reflects not only the moisture sensitivity of heterotrophic respiration but also the concurrent constraints on gas transport."*

L741: This is a crucial argument for sustaining your results, and I would be careful with the formulation. Surface CO2 fluxes reflect ONLY soil-atmosphere release (by definition, this is what is measured), and the release depends on CO2 production and transport processes.

L742: In the same line, you don´t measure CO2 production, but CO2 release (that´s why you define CO2 production as CO2 efflux + deltaDIC).

> We thank the referee for highlighting the imprecise use of terminology. We have slightly revised the sentence, and it should now be technically accurate.
>
> *"We conclude that surface CO_2 fluxes reflect the net soil–atmosphere release of CO_2, which depends not only on production but also on transport and temporary storage within the soil profile. Consequently, simple empirical models are unable to capture momentary soil respiration."*

L748: I would welcome a somewhat stronger statement here, rather than "suggest that .. may not significantly affect". I think you can say that enhanced waterlogging will not mitigate soil CO2 losses in this ecosystem.

> We appreciate the reviewer's suggestion to strengthen this statement. Because our conclusions are based on a single mesocosm experiment

conducted in a greenhouse, we consider it more appropriate to maintain slightly more cautious phrasing. We therefore maintain the wording as ‘suggest that …,’. To provide a clearer and more decisive formulation, we have removed the word ‘may’ and simplified the following phrase.

“Overall, our results suggest that off-season waterlogging in a cool, humid climate has no effect on total CO_2 emissions from cultivated mineral soils.”