Abstract:

L21 this is the first location where wetting events are mentioned. Please describe these events earlier in the abstract (L15)

→ We actually already mention them in line 17, but there we referred to 'water applications' instead of 'wetting events.' To avoid confusion, we propose to revise our phrasing:

L17: We examined (1) moisture supply by capillary rise along the soil profile and specifically into the top 20 cm soil, and (2) consequently the effect of GWT on decomposition of an added ^{13}C-enriched substrate (ryegrass) over a period of ten weeks, with limited wetting events representing a dry summer.

L24 this sentence is quite long and complicated, consider rewording for clarity or breaking up into 2 shorter sentences.

→ We agree and would adapt the sentence the following way:

L24: We postulate that the Birch effect might have been magnified following the rewetting of drier topsoils under deeper GWT levels. This then resulted in enhanced mineralization compared to conditions where the soil remained consistently wetter with shallower GWT level.

L30 the last two words in the abstract are the birch effect. For anyone who is not familiar with it this is a very confusing ending to an abstract. I suggest either explaining the birch effect at the top of the abstract or describing the process (i.e. enhanced mineralization after wetting of dry soil) instead of saying birch effect.

→ The experiment was not set up with the idea to specifically asses the Birch effect, therefore we would not want to start the abstract with a definition of this phenomenon. We do agree that some further clarification is needed before employing the term 'Birch effect' in the abstract.

We propose to alter the current abstract text from:

L24: We postulate that the Birch effect might have been magnified following the rewetting of drier topsoils under deeper GWT levels. This then resulted in enhanced mineralization compared to conditions where the soil remained consistently wetter with shallower GWT level.

To:

L24: We postulate that rewetting might have induced a stronger mineralization response, often referred to as the Birch effect, in drier topsoils compared to conditions where the soil remained consistently wetter with shallower GWT level.

And from:

L28: However, the net effect on topsoil C mineralization is complex and correct simulation of C mineralization may require further integration of specific processes connected to fluctuating soil moisture state, such as the Birch effect.

To:

L28: However, the net effect on topsoil C mineralization is complex. A correct simulation of C mineralization may require further integration of specific processes connected to fluctuating soil moisture state, such as the increased mineralization response after rewetting.

Intro:

In general, I think the phenomenon of capillary rise in drying soils is known and as the authors note has been thoroughly studied in the context of water availability etc. What is new here is the effects of this phenomenon on C mineralization, given the effects of water and changes in water content on it. The intro should therefore, I think, focus more on this part and here's a good place to describe the birch effect and fit it into your hypotheses, and less on what is known and why your method has merits over other methods.

→ We agree with the suggestion to replace a few sentences about previous research regarding capillary rise in terms of water availability with a hint already towards real fluctuating moisture regimes and the Birch effect. We would, however, rather not include the Birch effect in our hypothesis, as the experiment was not set up with the idea to asses this specifically. At the end of the document (in response to the last comment) a proposal for the adapted sentences of the introduction can be found.

L34 a word is missing before less. Becomes? Is?

→ OK, we rephrased it the following way:

L34: When soil desiccates, soil-water potential becomes strongly negative, making eco-physiological conditions for soil micro-organisms less favorable.

L34 In particular, (add comma)

→ OK

Methods:

In general, the methods are appropriate for the proposed research questions and are well described. A few comments:

L105 a layer of silt clay loam was added between the two 1m segments to ensure connectivity. I understand the reasoning for this given that two cores are artificially places on top of the other, but doesn't this bias the whole capillary rise measurement?

→ The 1 m segment cores were sampled separately due to the limiting length of the auger (L = 100 cm). The rationale for adding this silt clay loam soil was indeed to ensure a good hydraulic connection between the two segments. In retrospect, it would have been better to sample additional local soil from a depth of approximately 100 cm and use that instead. However, in both GWT treatments, the same setup was used, and observed decreases in θ_V with increasing height above the GWT were very logical. This suggests that overall any potential artefact effect of e.g. water absorption in the connecting soil layer must have been limited.

L114-L115 Because two GWT treatments were consecutively applied to each core (all in the same order of GWTs) it means that the second GWT treatment is not independent from the first because it carries over the effects of having been subjected to the first GWT treatment. Can you comment on this. If random cores were given the reverse order of treatments and shown that this does not affect the results that would have been compelling.

→ We deliberately handled the deepest GWT treatment first. This approach ensured that any potential impact of moisture transport on soil structure was confined to a height above the GWT, which was then exceeded during the subsequent, shallower GWT treatment. Between the two GWT treatments, the barrels with the water representing the GWT were completely emptied, and the soil columns were left to dry out to a condition analogous to the initial state of the first GWT treatment. We will add a sentence to the Materials and Methods section to clarify this approach further. Alternatively, we could have sampled 24 cores and both GWT treatments conducted simultaneously. However, the collection of such long intact soil cores and preparing them in 2 m long setups was labor-intensive. In fact the 4 used cores per texture were already selected out of a larger collected set where the non-used cores displayed damage incurred during sampling and further handling. Hence these practicalities constrained using a fully randomized approach. Nevertheless, by conducting the treatments sequentially, we were able to precisely assess the effects of the two treatments on identical columns (pairwise comparison). This approach eliminated the potential influence of heterogeneity in soil, which could have been an affecting factor on the moisture dynamics. We do want to point out that we only assessed mineralization in the topsoil, which was in fact replaced going from the first deeper GWT batch to the second shallower GWT batch.

L121 I was disappointed that the authors did not report their results for the mineralization of native SOC and only show the 13CO2 results. This would have given a fuller picture of C dynamics in their systems. Also, because the added 13C litter is likely to 'exist' as particulate organic matter and a large proportion of SOC 'exists' as mineral associated organic matter, *quantifying their relative mineralization under different GWT treatments could have greatly improved your hypotheses and results, especially given the lack of difference in 13C mineralization between soils. Since that data surely exists and does not require repeating or doing new experiments, I highly encourage the authors to include this data. The authors` concerns regarding soil properties effects on SOC mineralization can be partially addressed by normalizing CO2 to C content, and besides the mineralization of the 13C litter is likely also impacted by various properties.*

→ CO_2 efflux from native SOC was higher during the first treatment (deeper GWT with lower topsoil θ_V) compared to the second treatment (shallower GWT with higher θ_V) for all textures. This counterintuitive outcome suggests that a substantial portion of the native CO_2 efflux resulted from SOC from the undisturbed and non-renewed part below 20 cm. Therefore, if we intended to compare native SOC mineralization, we should have used completely fresh soil columns for both treatments. As mentioned in our response to the above previous referee comment, we prioritized assessing the effect of GWT depth on identical columns to eliminate the potential impact of soil heterogeneity on moisture dynamics. This setup trade-off was initially decided, and is also the very reason why we worked with the ^{13}C-labeled ryegrass.

L132 Why did you use VWC sensors which have to be soil-calibrated and then have to be converted to matric potentials using a retention curve instead of using water potential sensors?

→ We preferred the VWC sensors because of their design. They consist of very fine, sharp rods, which allowed to pierce the intact soil columns and waterproof foil with minimal disturbance to the surrounding soil, while minimizing moisture and air losses. Additionally, in previous research we experienced that with tensiometers, there is a higher chance of malfunction compared to volumetric moisture sensors as they must always be filled with degassed and demineralized water and no signal is given when they are not filled properly. Installing both sensor types (volumetric moisture sensors and laboratory tensiometers) would have been ideal to provide us with the most complete and reliable data, but it would have also resulted in twice the disturbance and more soil volume being occupied by the sensors in the columns.

L180 delta13C of CO2 undergoes fractionation at different diffusivities (e.g. water contents). https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JG000766

Given that your labeled material wasn't very highly enriched, such differences can have an effect on your calculations. Were the parallel incubations of end members (L179-180) done at the same GWTs as the experimental incubations?

→ We assessed the isotopic signature of CO_2 emitted from native SOC and ryegrass using parallel 20 cm packed soil columns, so not in the 2 m soil columns, as already described in L180-186. Working with soil on top of undisturbed 180 cm soil columns in GWT barrels, would have been complicated as there would also be ‘contamination’ of CO_2 being emitted from the underlying soil, limiting to precisely assess the $\delta^{13}C$ of CO_2 derived from ryegrass mineralization. In these ancillary soil core incubations care was taken to as closely as possible simulate the moisture regime as it occurred in the main experiment: viz. every few days the 20 cm soil columns were weighed, and (a small amount of) water was added if their θ_V became lower than certain thresholds (0.1, 0.12 and 0.15 $m^3\ m^{-3}$ for respectively loamy sand, sandy loam and silt loam soils), while also the larger wetting events were applied identically as in the 2 m columns. We would not further comment on this in the revised version.

Results:

Consider renaming the treatments to something friendlier on the eye of the readers (e.g. instead of -165 cm and -115 cm GWT, GWT-deep and GWT-shallow)

→ We always explicitly mention the GWT depths because it is crucial for understanding the extent of the capillary moisture transport. Using terms like "deep" and "shallow" might not prompt readers to look up the specific depths to which we are referring. We also think these are terms rather open to interpretation. In the results and discussion section, we did regularly refer to the -115 cm GWT as the “shallower -115 cm GWT treatment” (L301, L313, L443, L446…). We propose to now also add this “shallower” and “deeper” term explicitly to the figures and tables (but then in addition to -165 cm and -115 cm).

Discussion:

L445-450 I agree that the asynchrony between 13C mineralization and water content differences are difficult to overcome. However, the differences in cumulative mineralization (fig5) and rate (fig4) become significant only as WFPS differences become significant. So I am not convinced that this surprising result is because the Birch effect was a more dominant process than water content. Could it be that litter 13C mineralization was favored at lower WFPS because it was occluded within pores that still retained water at lower WFPS especially in silt loam (e.g., https://doi.org/10.1016/j.soilbio.2022.108777), while other C sources were preferentially mineralized at higher WFPS because they were in larger pores? I again encourage the authors to look at the native C mineralization to provide a clearer picture of C mineralization in your experiments.

→ Although we did not show the SOC mineralization results, we found higher CO_2-SOC for the -165 cm GWT treatment, or when the topsoil was drier for sandy loam and silt loam. This is the opposite of your suggestion for silt loam soils (‘preferential mineralization of other C sources than the ryegrass at higher WFPS’). However, as mentioned earlier, we cannot distinguish clearly which part of this CO_2-

SOC efflux is coming from the renewed topsoil, and which part is from the undisturbed, non-renewed 180 cm soil column, which could already have been partly depleted for the second GWT treatment. We carefully considered the Birch effect because ryegrass-C mineralization rates were significantly higher mainly after the third wetting event for all three textures. We expect that the occlusion of the added ryegrass was limited within the relatively short incubation period of 70 days, but such could only be confirmed by soil physical fractionation and we would argue that such extra work is beyond the scope of the current study. Alternatively, it could perhaps also be argued that in silt loam soil, in particular under the shallow -115 cm groundwater, soil conditions for ryegrass mineralization already became a bit too wet (although still just around 40 % WFPS), i.e. with lesser availability of O_2 limiting mineralization. Even though we had considered such explanations, we did, however, not, bring them up in the discussion as without metrics of aeration, O_2 concentrations or redox potential, such an interpretation is speculative. Should the editor and referee see this nevertheless fit we could briefly complement the discussion with such alternative potential interpretations of the found results.

Regardless, if the Birch Effect turns out to be such an important aspect of the discussion of the C mineralization results, it should be explicitly termed, explained, and integrated in your hypotheses in the introduction.

→ We did not initially emphasize the Birch effect because we actually did not anticipate it as a potential explanatory factor for our results. To do so posteriorly would not be entirely appropriate. Our primary focus was on the impact of the GWT through capillary moisture transport, which is why we concentrated the introduction on this aspect. However, we agree that we could replace a few sentences (we propose to delete 6 lines) about previous research regarding capillary rise with information about C mineralization and moisture regimes under real, fluctuating conditions, while also hinting at the Birch effect already (adding 6 new lines). Therefore, we propose the following adaptation to the introduction:

When soil desiccates, soil-water potential becomes strongly negative, making eco-physiological conditions for soil micro-organisms less favorable. In particular, intracellular turgor pressure and cellular integrity are no longer guaranteed (Malik and Bouskill, 2022; Wang et al., 2015), while diffusion of substrates and extracellular enzymes becomes impeded (Manzoni et al., 2016). As a result, there is a strong moisture dependency of carbon (C) and nitrogen (N) mineralization in soils. Soil C models therefore simulate moisture through hydrological modules. As precipitation and irrigation are usually the primary suppliers of topsoil moisture, most models do not account for lateral or upward moisture influxes. However, during prolonged dry periods, drying out of topsoil may lead to establishment of counter-gravity soil suction gradients inducing significant upward redistribution of water from the groundwater table (GWT) to the vadose zone through capillary action, and as such, control topsoil moisture. With progressing climate change throughout Europe, weather patterns are becoming more erratic, with already increased occurrence of unusually lengthy dry periods and even agricultural drought in the Maritime climatic region over the past years (Aalbers et al., 2023; CEU JRC, 2022).

Whether or not moisture supply via capillary rise is a relevant process to be accounted for by soil C models, will not only depend on climate, but also on factors such as the depth of the GWT and soil physical properties. But to date, the effect of the GWT depth and capillary moisture supply has nearly exclusively been studied in relation to crop yields (Awan et al., 2014; Feddes et al., 1988; Kroes et al., 2018; Zipper et al., 2015) and irrigation needs (Babajimopoulos et al., 2007; Jorenush and Sepaskhah, 2003; Prathapar et al., 1992; Yang et al., 2011). For example, Zipper et al. (2015) found optimal maize crop yield at GWT levels of 0.6, 0.8, and 0.9 m depth for sandy loam, loam, and silt loam soils, respectively and attributed this to optimal water supply resulting from capillary action. ~~Awan et al. (2014) found that when GWT levels lowered from 100 cm to 150 cm to > 200 cm in silt (clay) loam soils, water supplied by capillary rise to the rootzone of cotton and wheat reduced from 28 % to 23 % to 16 % and 9 % to % 6 to 0 %, respectively.~~ When considering bare soils, simulations of the so-called extinction depth for GWT evaporation resulted in depths of 70, 130 and 420 cm for respectively loamy sand, sandy loam and silt loam soils (Shah et al., 2007). This diverse range of modeling outcomes highlights the site-specific nature of capillary rise~~, as it not only depends on obvious factors such as soil texture, GWT depth and soil water potential gradients, but also on soil structure and soil profile development. As a result, to~~. To the best of our knowledge, there exists no robust proof on whether or not, and when, GWT depth might significantly control topsoil heterotrophic activity, which may inform us on the pertinence of accounting for its depth and capillary rise in updated soil C models. To validate simulation results, a few studies have been carried out with parallel small-scale field lysimeter experiments monitoring the soil water balance (Kelleners et al., 2005; Prathapar et al., 1992; Yang et al., 2011). Alternatively, Grünberger et al. (2011) injected a deuterium enriched solution to the GWT to follow capillary rise in arid areas. Both approaches, however, are labor intensive and/or require high investments and technical expertise. Li et al. (2022) instead simply excluded upward capillary moisture transport in a field trial on crop residue decomposition by placing a 5 cm gravel layer at a depth of 50 cm, and found that for sandy soils a GWT depth at just 60 cm was not shallow enough to notably provide the top 25 cm soil with capillary moisture. However, this approach required disturbance of the topsoil and moreover the artificial break of capillary rise also unintentionally cancelled out unsaturated downward water redistribution. Most importantly perhaps, the main impediment of observational field approaches, such as the ones listed above, is their inability to control ambient factors such as GWT depth, precipitation, temperature and relative humidity. This limitation restricts our ability to study the effect of individual components of the soil water balance like capillary rise.

As an alternative, a handful of laboratory-scale experiments have sought to infer the capillary moisture impact on soil biogeochemical processes. Rezanezhad et al. (2014) and Fiola et al. (2020) found that highest C mineralization was found at transient redox conditions above the capillary fringe, where moisture and oxygen are in balance. However, due to the small scale of the used setups (packed soil columns of 45 cm and 30 cm length, respectively) an appreciation of capillary rise was not possible. Malik et al. (1989), Shaw and Smith (1927) and Lane and Washburn (1947) assembled larger packed soil columns to determine maximum capillary rise height as a function of soil texture. They found capillary moisture supply up to 149 cm (loamy sand soil), 183 cm (sandy loam soil) and 359.2 cm (silt soil), respectively. But as those columns were repacked from sieved soil, soil structure was disrupted and in-field occurring heterogeneity and macropores were not well represented, while neither the impact on C mineralization was assessed (Lewis and Sjöstrom, 2010). ~~In sum, there is no clear empirical evidence of the control of moisture supply by capillary rise on topsoil organic matter (OM) mineralization.~~ In sum, there is little empirical evidence of the control of moisture dynamics by capillary rise on topsoil organic matter (OM) mineralization. Not only the impact of GWT onto mean topsoil moisture content seems a blind spot but, but possibly also the amplitude of soil moisture fluctuation in topsoil may depend on the magnitude of moisture supply by capillary rise. After a rainfall event, rewetting of dry soil leads to strongly increased C mineralization, often referred to as the Birch effect (Birch, 1958). This effect depends on the magnitude of the soil moisture increment and/or drier pre-wetting condition (Liang et al., 2023). With a stronger continuous supply of moisture via capillary rise we may expect smaller fluctuations in topsoil moisture and then also smaller Birch-effect induced soil C mineralization peaks.

Our main aim was to study if, during a (simulated) period ~~of drought~~with limited rainfall, there would be a significant effect of capillary rise from the GWT on topsoil moisture and OM mineralization for loess deposited arable lands in North-West Europe. We designed a setup wherein excavated 200 cm long undisturbed soil columns were incubated in the laboratory with ambient factors being regulated and soil moisture monitored. Columns of three soil textures were subjected to minimal watering events representing a dry summer and two GWT depths to study the interaction between both factors and to provide a representative depiction for our study region, i.e. North-West Europe. The decomposition of an introduced substrate, i.e. 13C-enriched ryegrass, was monitored through CO2 headspace measurements. We hypothesized that a deeper GWT would result in reduced topsoil moisture content and as a result, C mineralization in the topsoil would be relatively inhibited compared to the treatment with shallower GWT. We expected an increasing susceptibility to reduced moisture of the C mineralization with coarser soil texture as water losses by evaporation would be less compensated by capillary moisture input. Although physicochemical protection of OM is stronger in finer-textured soils, we expected that such direct effect of soil texture on mineralization of the added OM would be of less importance in the short term (10 weeks) as opposed to regulation of soil moisture by the soil texture and GWT depth combinations.