RC1: ‘Overall, the paper is well written and complete. The introduction provides an adequate description of the context and introduces the aim of the research with useful references. The experimental design is well described. I would suggest including more information about the soils used in the experiment (e.g. soil classification). The statistical approach is well described, you report the methods and software used. The results are clearly presented and the conclusion seems to be strongly supported by the results. References are complete and up-to-date.’

Reply:

We would like to thank you for the time and effort you invested in reviewing our draft paper. We greatly appreciate your positive evaluation. According to the WRB soil classification map of Flanders (scale 1:40,000, converted from the Belgian Soil Map: DOI: 10.13140/2.1.4381.4089), the soil profiles originating from Kruisem and Bottelare are classified as Eutric Retisols, while the soil profiles from Oosterzele are Eutric Cambisols (Siltic). We added this information in L100-101. Obviously, soil profile descriptions would provide a more precise assessment. We do believe that already the major affecting physicochemical specifications for moisture dynamics are supplied, e.g. detailed texture values, OC, pH, bulk densities (Table 1) and pF curves (Fig. B1).

RC2:

27 replace 'can be' with 'is'

→ OK

37 remove 'content'

→ OK

50 replace 'moisture' with 'water'

→ OK

66 replace 'moisture' with 'water'

→ OK

114 with respect to the difficult-to-interpret results of this study, I wonder whether the lab room temperature may have had an effect. As stated, the two GWT tables were imposed for two consecutive time periods. The C turnover is highly sensitive to temperature. Let's assume a more or less standard Q10 of 1.8 for an Arrhenius-type equation. That means for a 10 degrees increase in soil temperature you would have 1.8 times the amount of C turnover. The lab temperature has a standard deviation of plusminus 0.5 degrees. For a 1 degree increase you end up with 18 % more respiration. Now, if the average temperature in the lab for the two consecutive incubation periods differs...

→ The room where the experiment took place was held under a ventilation system, and the system did not change between the GWT treatments. Therefore, the average room temperature between both treatments was 20.8°C. However, there was indeed some fluctuation in temperature over time during both treatments, hence the standard deviation of 0.5°C. We believe that this variation in room temperature had a smaller impact on C mineralization than the moisture regime itself. Although there was no difference in mean air temperature between the both incubation batches, we do agree that such fluctuation could have impacted our results. Therefore, we particularly agree to be more cautious with the explanatory statements regarding the Birch effect, as they are based on relatively small increments in ryegrass-C mineralization. We will phrase these statements more conditionally and carefully in our discussion and conclusion.

144 replace 'moisture' with 'water'

→ OK

145 replace 'moisture' with 'water'. Well, there is a little issue with terminology: there is either 'water content' or 'moisture'. Please correct for the entire manuscript...

→ OK

207 well, the more correct term would probably be 'potential' instead of 'height'

→The matric head is the height of a water column that corresponds to the matric potential, typically expressed in units of cm water height (cm WH). Thus, 'matric potential' is a general term describing the energy state of water in the soil, with 'cm water height' as its unit of measurement. Therefore, we consider our text correct and leave it as it is.

209 I suggest to replace 'moisture transport' with 'water flux'

209 and 210 replace 'transport' with flux

→ OK, we adapted the text the following way:

209: The latter were used to calculate hydraulic head differences (ΔH) between two adjacent sensor positions above the GWT, with hydraulic head the sum of matric head and gravitational head. They were used as an indicator for the water flux direction: positive ΔH values indicate a net upward (capillary) water flux, while negative ΔH values signify an overall downward water flux.

243 I would prefer to replace 'moisture transport' with 'variably-saturated water flux'

→ We doubt if that would be a correct phrasing, as 'variably-saturated water flux' does not appear to accurately describe the water state and flux in the unsaturated soil (above the groundwater table). However, we propose to adjust the original text:

3.1 Moisture transport as a function of GWT treatment

Into:

3.1 Soil moisture dynamics in response to GWT treatment

245 that first sentence can be deleted

→ We think this is an important first impression describing the results. Therefore we would like to keep it. It also includes some information regarding the infiltration depth of the wetting events (which is questioned in further remarks).

246 Are measured (saturated) hydraulic conductivities available? In combination with the measured soil water retention functions this would allow to estimate the infiltration depth, or even a soil water content profile over depth. To get an idea of how much the upper 20 cm with the labeled C were actually affected by the irrigation for the different soils…

→ From the measured volumetric water contents (Fig. 2) we could in fact already clearly distinguish that wetting events lifted soil moisture at –10 cm depth, but not at –30 cm depth. So the infiltration depth must be located somewhere between both depths for all three textures. No hydraulic conductivity measurements were taken in these repacked topsoils, so we cannot further define the specific depth of infiltration. However, our main aim was not to assess the impact of these wetting events, but rather to compare the effect of the GWT on topsoil moisture within distinct soil textures, whilst also mimicking true field conditions, i.e. with minimal rainfall representing a dry summer. As the wetting events were identical between both GWT treatments, this approach seems valid to us.

Figure 2 The y-Axis of graphs should basically always start from zero.

→ We agree and have adjusted the Y-axis of Figure 2 to start from zero.

295 A difference in heads is usually referred to as a 'hydraulic gradient'.

→ We do think there is a difference between a hydraulic head gradient and the hydraulic head difference. The latter is obtained by subtraction of the two hydraulic heads at the two depths and is put in the units cm WH. The gradient, on the other hand, is obtained as the quotient between a hydraulic head difference and the distances between the measuring points, and has units cm cm^{-1} or alike. In addition, a 'gradient' would rather suggest a monotonous change in hydraulic head between at least two depths, but it is quite unlikely that we should encounter such trend in our heterogenous soil profiles. Therefore, we opted to just more directly refer to 'hydraulic head differences between adjacent sensors'.

315 well, now you end up with two factors affecting (or not affecting) the water content in the upper 20 cm with labelled C. The ground water table AND the irrigation. The irrigation will affect the respiration of the different soils differently. This might be difficult to interpret in the end.

→ We did not compare ryegrass-C mineralization between the different textures. Instead, we focused on the effect of GWT-induced moisture differences within identical soil textures. Additionally, statements about water fluxes were texture-specific, as irrigation might have had different effects on the moisture balance of different textured topsoils. Obviously, the wetting events influenced the topsoil moisture, but we deem them essential to mimic field conditions, where going 70 days without rainfall would be highly unlikely. The small amount of added water mimicking rainfall was based on a realistic scenario, representing a dry summer in the study area, and this regime was kept identical across different textured soils. In fact this scheme also mirrors the real field situation as the collected soils are in fact often at just short distances of each other in Flanders. This approach then allowed us to assess the net effect of GWT depth.

Fig. 3 Maybe a log10 scale applied to the y-axis helps this figure

→ Applying a log scale to negative values is not possible.

Fig. 4 I am quite sure a log scale applied to the y-axis benefits this figure

→ We tried a log-scale Y-axis. However, then the positive standard errors of the ryegrass mineralization rates for sandy loam soil resulted in both negative and positive log values due to the original values ranging from <1 to >1. This looks rather confusing, as the displayed errors would represent only the positive standard errors. Therefore, we decided to keep the current scale for the axes.

379/380 delete or move to results

→ OK, deleted

~~379/380: Overall, the established GWT contrast (–165 cm vs. –115 cm) significantly affected topsoil (–10 cm) moisture content in the sandy loam and silt loam columns, but not in the loamy sand columns (Table 2).~~

387 I assume '-30' and '-10 cm' are the coordinates in the column (i.e. the depth) and not the difference in pressure head?!

→ Indeed. If we mention pressure heads, the unit is mentioned as 'cm WH' instead of 'cm' solely. To make that more clear in this specific case, we would now add the word 'depth':

386: Second, hydraulic head differences between –30 cm and –10 cm depth were even negative, excluding upward moisture transport out of the directly underlying subsoil.

406 No, that is not unlikely. This could be a result of macropores.

→ We recognize that macropores can serve as preferential flow paths, facilitating a downward water flux through the soil pore network. However, in combination with the volumetric soil moisture data, we think it could not have been a dominant avenue for moisture transport. Higher measured water contents for the –115 cm GWT treatment compared to the –165 cm GWT treatment demonstrate that the net transport of moisture must have predominantly been upwards, as there were no differences in water regime and thus potential downward leaching through macropores between both GWT treatments.

409 well, there are very small lab tensiometers on the market.

→ In retrospect, installing compact tensiometers would have certainly been worthwhile compared to only using volumetric moisture sensors. However, we particularly opted for TEROS 10 and TEROS12 sensors (from Meter Group) as these consist of fine, sharp rods. This allowed to pierce our soil columns and the covering waterproof foil with minimal disturbance to the surrounding soil, and minimizing moisture and air losses. The type of small tensiometers with a sharp shaft diameter of less than 1 cm with a high measuring accuracy, for example the TEROS31 from Meter Group, was only released in 2021, the same year of the start of this experiment. 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 small lab tensiometers) would have been ideal to provide us with the most complete and reliable data, but it would also have resulted in twice the disturbance and more soil volume being occupied by the sensors in the columns.

421 Did you observe macropores during sampling?

→ When opening the soil columns after the experiment, we did not visually detect any preferential flow paths in the soil cores. However, since macropores can be as small as 0.08 mm, we cannot guarantee that absolutely none were present.

435 these 'bell-shaped' curves were usually determined for disturbed soil samples. And in the wet range the decrease of respiration is rather related to an oxygen deficit and is actually not related to water content. For undisturbed samples the moisture sensitivity function of respiration actually looks different.

→ The top 20 cm of the soil columns was amended with ryegrass and repacked on top of the undisturbed soil columns (180 cm length). Thus, the soil in which we followed the ryegrass C-mineralization was in fact disturbed, representing the ploughed topsoil in the field.

438 better state that the microbes show a more direct reaction to the water-filled pore-space than to the pressure head

→ We are not convinced that microbes generally react more directly to WFPS than to matric potential. To our knowledge, it depends on the specific moisture range. In very dry conditions, water potential is often a better predictor of C mineralization because small decreases in water content can translate in significant changes in pF. We would acknowledge this in our text and explain our rationale for still using WFPS as follows:

438: Particularly in the intermediate dry range the response of OM mineralization to volumetric soil moisture is strong, while at lower water content, mostly water potential is a better predictor. Given the occurring moisture range and to avoid conversion of measured volumetric moisture through pF curves sensitive to hysteresis, we further used % WFPS as a more directly obtained measure of soil moisture.

448 I suggest to delete 'unwanted asynchrony'

→ We propose to alter the following original text:

448: This unwanted asynchrony makes that the effect of GWT treatment on $C_{ryegrass}$-min should not be evaluated based on the 70-days cumulative $C_{ryegrass}$-min, but rather only after several weeks with GWT treatment imposed topsoil moisture differences effective by then.

into:

448: <u>This implies that</u> the effect of GWT treatment on $C_{ryegrass}$-min should not be evaluated based on the 70-days cumulative $C_{ryegrass}$-min, but rather only after several weeks, with GWT treatment imposed topsoil moisture differences effective by then.

453 which 'artefact'?

→ The artefact refers to the delayed emergence of topsoil moisture differences induced by the GWT treatment, which became apparent only after day 8 of the incubation. In contrast, the peak in mineralization of the easily degradable ryegrass-C pool occurred within the first few days. We can see that this term artefact was rather unclear and therefore we propose to alter the text from:

450: From the fitted kinetic model it emerges that the easily mineralizable $C_{ryegrass}$ pool had already been mineralized around day 14 in case of the loamy sand and sandy loam soil and day 20 for the silt loam soil. Thereafter, cumulative $C_{ryegrass}$-min followed a relatively constant course, i.e. it was determined by the mineralization of a more stable $C_{ryegrass}$ pool and the artefact in our experiment makes that evaluation of GWT on topsoil $C_{ryegrass}$ mineralization should further solely be based on effects on mineralization rate of the more stable $C_{ryegrass}$ pool (k_s).

into:

450: From the fitted kinetic model it emerges that the easily mineralizable $C_{ryegrass}$ pool had already been mineralized around day 14 in case of the loamy sand and sandy loam soil and day 20 for the silt loam soil. Thereafter, cumulative $C_{ryegrass}$-min followed a relatively constant course, i.e. it was determined by the mineralization of a more stable $C_{ryegrass}$ pool, and therefore the evaluation of GWT on topsoil $C_{ryegrass}$ mineralization should further solely be based on effects on mineralization rate of the more stable $C_{ryegrass}$ pool (k_s).

457 please replace 'refute' with 'reject'

→ OK

458 replace 'it needs .. in mind' with 'against the background'

→ We propose to alter the text from:

458: To interpret these seemingly illogical effects it needs to be borne in mind that the relation between soil moisture and C mineralization is complex when moisture fluctuates as in our experiment, compared to when it remains constant as is typically the case in experiments to infer the bell-shaped θv – C-min relationship.

To:

To interpret these seemingly illogical effects, <u>it is important to consider</u> the complexity of the relationship between soil moisture and C mineralization, especially when moisture fluctuates as it did in our experiment. This is in contrast to typical experiments to infer the bell-shaped θ_V–C-min relationship where moisture is held constant.

500 but not only the GWTs were modified, simultaneously precipitation was simulated

→ Yes, but with identical regimes of timed wetting events applied to both GWT treatments, the difference in both experimental batches is still solely the groundwater table depth. We would therefore keep the original phrasing.

516 now, if Richards equation is used there will always be water flux in all directions. It might be constructive to advocate Richards-based models in contrast to bucket models…

→ Although both mentioned model types use the Richards equation, free-drainage soil models only simulate capillary moisture transport towards adjacent unsaturated soil layers. They do not include upward moisture transport from the groundwater into the unsaturated zone. Therefore, we think this contrast in both approaches is of particular relevance for this paper as we observed variable influence of the groundwater on the soil water profile for the investigated texture and GWT depth combinations. As a compromise, we propose to now explicitly mention tipping-bucket models shortly in the discussion:

484: Ukkola et al. (2016) further reported that there is a systematic tendency among numerous land surface models to overestimate the consequences of drought. They attributed this to the assumption of a free-draining soil boundary in these models, i.e. no account is taken of a GWT. According to our results, simplified hydrological modules using the Richards equation but with free-draining lower boundaries applied in many soil C models, e.g. DAYCENT (Schimel et al., 2001), BIOME-BCG (Thornton, P.E and Law, B.E, 2010), CERES (Gabrielle et al., 1995), CANDY (Franko et al., 1995), would be less accurate for simulation of topsoil moisture during periods with limited rainfall, as these models do not incorporate capillary rise in simulating recharge and presuppose that water draining from the soil profile is lost. The discrepancy with real in-soil occurring physical processes becomes even larger for models that not use the Richards equation but instead employ a simplistic cascade bucket approach, such as DSSAT (Jones et al. 2003). Only a limited number of biogeochemical models based on the Richards equation also include bidirectional water flow between the saturated and unsaturated zones by defining or even calculating a dynamic GWT, e.g. LandscapeDNDC (Haas et al., 2013; Liebermann et al., 2018) and DAISY (Abrahamsen and Hansen, 2000). However, the question remains how accurate such models simulate upward moisture supply and relate C mineralization to moisture content, particularly under dry conditions.

516: For situations where the GWT is within these ranges our findings should motivate to include bidirectional water flow between the saturated and unsaturated zone, i.e. drainage and capillary rise from the groundwater, in soil models.

RC3:

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, <u>such as the increased mineralization response after rewetting.</u>

<u>Intro:</u>

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, <u>making</u> eco-physiological conditions for soil micro-organisms less favorable.

L34 In particular, (add comma)

→ OK

<u>Methods:</u>

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 added the following sentences to the Materials and Methods section to clarify this:

'L124: For both GWT treatments, the columns followed the identical preparation steps. We deliberately handled the deepest (–165 cm) GWT treatment first. This approach ensured that any potential impact of upward moisture transport on soil structure was confined to a height above the GWT, which was then exceeded during the subsequent, shallower (–115 cm) GWT treatment. '

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. We emphasized this by adding the following part to L118:

'L118: The GWT treatments were consecutively applied to the same columns, resulting in two incubation periods under identical laboratory conditions but with renewed 20 cm topsoil on top of undisturbed columns.'

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…).

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.