We would like to thank the editor for the valuable suggestions, which we have thoughtfully incorporated. Below, we present the editor's comments in *italics*, followed by our responses and proposed revisions to the text:

I think that you replied to the comments of the reviewers but I propose that you revise your response to the question of reviewer 3 with respect to the monitoring of native C mineralization. I think that also the drastic change in temperature from field conditions to a uniform temperature of 20 °C over the entire profile length is important. This change in temperature will especially have an impact on the carbon mineralization in the deeper soil horizons.

Ln 145: 'The native soil OM-derived CO2 efflux in part originated from mineralization of native soil OM in the undisturbed (–20 to – 200 cm) soil. As this soil column was not renewed between both GWT treatment batches, the quality of subsoil OM differed between both GWT treatments. For these reasons it is not meaningful to present native SOC mineralization data and results are kept restricted to ryegrass-C mineralization.' I am not so convinced by this reply to the reviewers' question why you did not consider the native CO2 efflux. Isn't the C-pool large enough so that no big effect in C pool size and composition should be expected after a few weeks? I would however expect that the change in temperature of the subsoil from roughly 10 °C to 20°C would impact the mineralization of subsoil carbon a lot and would rather consider this as an argument why not to look at the native carbon mineralization.

We agree that a uniform temperature of 20°C throughout the entire soil profile is unrealistic and can see that this forms a valid argument to disregard SOC emissions. We do still believe that even at lower realistic subsoil temperature then still our serial experimental design would not have allowed comparing SOC-derived mineralization between both GWT-batches. Although indeed the background SOC stock would not be different between the batches, there will be differences between the labile-C pool that is likely to have been in part depleted after the first batch. We therefore also still included our other motivation in the M&M text.

➔ To clarify, we propose the following revisions to L130-142:

'We used a model OM substrate (*in casu* ^{13}C-labeled clipped ryegrass) and focused solely on the mineralization of this added substrate in the topsoil, without including data on native soil OM mineralization. This approach was chosen primarily because the inherently different quality and quantity between the three soils would no longer allow studying the effect of GWT depth, soil texture and their interaction on soil OM mineralization. Additionally, CO_2 effluxes from native soil OM included contributions from mineralization in the undisturbed subsoil column (from –20 to – 200 cm), which was left unchanged between GWT treatment batches to reduce potential soil heterogeneity influences on moisture dynamics. As native soil OM in this subsoil had thus already undergone partial mineralization during the first GWT treatment, starting conditions would differ for the second GWT treatment. Particularly so as subsoil was moreover kept uniform at 20 °C, which deviates strongly from expected field conditions, where temperature is lower and typically decreases with depth. This discrepancy further limits the applicability of our setup for assessing subsoil OM mineralization.

I propose also to reconsider the name of the model that you use: parallel first-zero-order kinetic model. In fact, since you express the mineralized carbon as a percentage of the applied carbon, you assume that the mineralization scales linearly with the initial carbon amount. This would correspond with a first order kinetic. In a zero-order kinetic, the reaction rate is not dependent on the concentration of the substrate. In your model it is proportional to the substrate concentration. But, what you consider as a 'zero-order' is in fact a linearization of a first-order reaction for short times compared to the reaction time scale. Therefore, I propose calling the model a parallel two rate first-order kinetic model.

- ➔ We agree and adapted the phrasing in L214, L246, L248, L374, L384 and L386.

Finally, I think you need to define better what you mean by 'capillary rise'. It could be interpreted as an upward water flow or it could be the hydrostatic water content profile above a groundwater table. When you assume hydrostatic conditions, the water content at the soil surface can be read for a given groundwater table depth from the water retention curve. But, when there is this upward flow and evaporation, the water content at the soil surface will always be smaller than the water content for the same groundwater table depth under hydrostatic conditions. In order to assess the water content under upward flow conditions near the soil surface, it is important to know the unsaturated hydraulic conductivity and the evaporation flux. I propose to include information about the upward evaporation fluxes that were measured in the different treatments. It should be possible to derive that from the amount of water that was added at the bottom of the columns to keep the water table constant over time.

- ➔ In this study, we consider capillary rise as the active upward movement of water driven by capillary forces. This process must indeed be considered alongside evaporation, as some of the moisture supplied by capillary rise is subsequently lost to evaporation. This is also described at the end of the introduction in one of our hypothesis in L 90-91:

  'We expected an increasing susceptibility to reduced moisture of the C mineralization with coarser soil texture <u>as water losses by evaporation would be less compensated by capillary moisture input</u>.'

  In the discussion, specifically in section 4.1, lines 407-409, we also distinguish between the potential capillary rise driven by hydraulic head differences, which depend on the hydraulic characteristics of the soil, and the actual net effect on topsoil moisture due to to evaporative losses:

  'First, although positive hydraulic head differences (ΔH) between –60 cm and –30 cm enabled capillary action, they displayed an increasing trend throughout the experiment (Fig. 3). Hence, the soil was observed to be drying out at 105 cm and 135 cm above the GWT, indicating clearly that <u>evaporative losses were insufficiently compensated by a capillary water flux</u>. '

However, to make this clear this from the beginning of the manuscript, we propose **modifying** the term 'capillary rise' in the following lines of the **abstract and introduction**:

L15: We examined (1) ~~moisture supply~~ upward moisture flow by capillary ~~rise~~ action 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.

L23: These findings suggest that the ~~capillary rise~~ upward capillary moisture flow, along with the resulting increase in topsoil moisture and the anticipated enhancement of biological activity and ryegrass mineralization, might have been counteracted by other processes.

L 45: Whether or not moisture supply via upward capillary ~~rise~~ flow 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.

L53: 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. Such insights are essential to determine whether incorporating GWT depth and upward capillary ~~rise~~ moisture flow in updated soil C models is warranted.

L67: This limitation restricts our ability to study the effect of individual components of the soil water balance like capillary ~~rise~~ moisture supply.

L76: In sum, there is little empirical evidence of the control of moisture dynamics by capillary ~~rise~~ water flow on topsoil organic matter (OM) mineralization. Not only the impact of GWT onto mean topsoil water content seems a blind spot, but possibly also the amplitude of soil moisture fluctuation in topsoil may depend on the magnitude of moisture supply by capillary ~~rise~~ action.

L84: Our main aim was to study if, during a (simulated) period with limited rainfall, there would be a significant effect of capillary ~~rise~~ moisture flow from the GWT on topsoil moisture and OM mineralization for loess deposited arable lands in North-West Europe.

And propose the following adaptations in the **discussion and conclusion**:

L418: When the GWT was raised to –115 cm, moisture at –30 cm was higher than at the –165 cm GWT, implying that upward capillary moisture flow ~~rise~~ markedly impacted soil moisture up to at least 85 cm above the GWT, less so beyond 105 cm and no more beyond 135 cm.

L422: Hence, it seems likely that upward moisture ~~supply~~ flow in the sandy loam and silt loam columns by capillary ~~rise~~ action reached at least up to a height of 135 cm.

L458: Consequently, as we worked with 2 m undisturbed soil columns and realistic GWT depths, we do expect findings of GWT-dependent capillary moisture supply ~~rise heights~~ and topsoil moisture to be representative for the field situation.

L517: According to our results, hydrological modules which calculate water fluxes between adjacent soil layers, but with free-draining lower boundaries, applied in some soil C models, e.g. DAYCENT (Schimel et al., 2001), would be less accurate for simulation of topsoil moisture during periods with limited rainfall, as these models do not incorporate capillary moisture flow ~~rise~~ in simulating recharge and presuppose that water draining from the soil profile is lost.

L548: For situations where the GWT is within these ranges our findings should motivate to include bidirectional water flow, i.e. drainage and capillary transport ~~rise~~, in soil models.

L555: During prolonged periodic droughts, expected to become more frequent under future climate, correct simulation of the mostly neglected capillary ~~rise~~ moisture ~~supply~~ transport may become imperative for reliable simulation of C cycling in agricultural land in North-West Europe.

To get back to your proposal to indirectly quantify evaporation: it would indeed have been very informative to establish a water balance over time, including an indirect **estimation of soil evaporation** by measuring the amount of water added to the barrels to maintain a constant level. However, because the barrels had a much larger diameter than the four soil columns per texture positioned within each barrel (Fig. 1), a considerable amount of water evaporated directly from the open water surface inside the barrels, rather than solely from evaporation out of the soil columns. As a consequence, our setup does not allow deriving evaporation based on water level changes in the barrels.

Detailed comments

Ln 22: 'In contrast, C mineralization pulses after the wetting events were even higher for the drier –165 cm GWT soils. For the silt loam soil, where capillary rise supply had the largest contribution to topsoil moisture, a lower mineralization rate of the stable Cryegrass pool was also found with shallower GWT.' These sentences are not clear. First you write that the mineralization pulses after wetting are higher for the -165 cm soils. Then you write that in the silt loam soil, the mineralization is lower for the shallower GWT. Do you mean that the C mineralization pulses of the labile pools after wetting events are higher for lower groundwater tables in all soils and that for the silty-loam soil also the mineralization of the stable pool is higher for the deeper GWT than for the shallower GWT? Or do you mean that for the silty loam soil the mineralization of the stable pool is lower than in the other soils for the shallower GWT? Make clear what you are comparing with each other when you write larger than or smaller than … .

- ➔ We propose to adapt our text the following way:

L21-L24: 'In contrast, CO_2 efflux pulses after some of the wetting events were even higher for the drier –165 cm GWT than for the –115 cm GWT across all three soil textures. Additionally, a model fitted to cumulative ryegrass mineralization showed a lower mineralization rate for the stable $C_{ryegrass}$ pool in the silt loam soil with the shallowest GWT, where capillary rise contributed most significantly to topsoil moisture, compared to other combinations of soil texture and GWT depth.'

Ln 23: 'These findings suggest that a potential capillary rise effect of increased topsoil moisture on ryegrass mineralization might have been counteracted by other processes.' You need to mention which effect has been counteracted because this is not clear. I propose: 'These findings suggest that a potential capillary rise effect of increased topsoil moisture that leads to higher biological activity and ryegrass mineralization have been counteracted by other processes.'

- ➔ We propose to adapt the text the following way:
  L24: 'These findings suggest that the upward capillary moisture flow, along with the resulting increase in topsoil moisture and the anticipated enhancement of biological activity and ryegrass mineralization, might have been counteracted by other processes.'

Ln 144: shouldn't you keep 'mineralization'?

- ➔ We agree and adapted as suggested.

Ln 157: I suppose the dose was 1.5 g C kg-1 of dry soil (and not wet soil).

- ➔ Correct, we have clarified this in the text.

Ln 155: 'texture-specific soil-grass mixtures' could also be interpreted as the soil-grass mixtures being different for the different textures, which was not the case. I propose: the soil-grass mixtures for the three soil textures.

- ➔ We agree and adapted as suggested.

Ln 242: 'hydraulic head differences (ΔH) between two adjacent sensor positions above the GWT' This is not clear because you can calculate differences between two adjacent sensors in two opposite ways. Since you are interested in the sign of the difference, it is important to mention how you calculate the difference. A way to avoid this is to mention that you calculated averaged hydraulic gradients between two measurement heights, where height was defined to increase in the upward direction (as opposed to depth which increases in the downward direction).

- ➔ We agree and adapted as suggested.