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: This implies 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.

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, it is important to consider 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.