Dear reviewer, Prof. Chris Evans,

We sincerely thank you for the thoughtful and scientifically insightful overall assessment of the manuscript. We appreciate the encouraging feedback. We acknowledge the important conceptual observations regarding the applicability of a traditional multi-pool turnover model to drained peat soils and the risk of equifinality. We have now expanded the Model overview section and Discussion to address these limitations more explicitly and to clarify how they relate to our modelling approach.

We also address all specific and technical comments in detail below. In addition, we have revised one of the scenarios and updated the measured N_2O dataset after identifying that the variance had been set too high in the previous version. Furthermore, we noticed a mistake in a conversion for one of our model drivers. The mistake resulted too low vapor pressure deficit inputs and has now been corrected by using relative humidity data directly as an input and letting the model handle the needed conversion. Consequently, the outputs showed an increase in simulated evapotranspiration values and N_2O emissions. In contrast, changes in CO_2 emissions were relatively minor.

As a result, the revisions have significantly improved the clarity and methodological transparency of the manuscript. You can find our detailed answers below.

Best regards,

Henri Kajasilta, on behalf of all authors

> This is a well-constructed and well-written paper which adapts an existing model (LDNDC) for agricultural peatlands based on detailed observation data from a Finnish experimental and monitoring study. The results showing clear CO2 and N2O emissions mitigation potential of both partial and full water table raising are broadly consistent with existing empirical data but provide new mechanistic insights. The study merits publication, subject to revisions based on the comments below.
>
> My main concern with the paper relates to the use of a 'traditional' multiple pool turnover model, developed for mineral soils, to represent carbon cycling in a peatland system where organic matter protection via waterlogging is likely to be more important (at least pre-drainage) than the properties of the organic matter itself (represented as 'young' and 'old' carbon'). Given that the peat now appears to have a high mineral content, organic matter protection by mineral surfaces may also be important. See e.g. Schmidt et al., Nature (2011) on why protection mechanisms are more important than intrinsic properties of organic matter, and why pool-turnover based models may therefore fail to represent processes correctly.

> The authors do seem to partly recognise these model limitations, and describe a process of parameter adjustment to try to reproduce observations. While the results look convincing, I would like to see a discussion of the potential limitations of the model conceptualisation (especially the application of a fixed pool turnover model applied to mineral-enriched drained peat) and the risk of equifinality resulting from adjustment of multiple parameters. In other words, can the authors provide a convincing case that the model is not getting the right answers for the wrong reasons?
>
> Regarding N2O emissions the model struggled to reproduce observations and this aspect of the study is consequently somewhat downplayed. This is unsurprising – N2O is difficult to predict or even measure, and the EC dataset the authors use for this study is one of very few available for agricultural peatlands, so there may be scope to comment a little more on this in the paper. With apologies for recommending a paper I was involved in, a recent EC N2O study of a cropland in England (Cowan et al., GCB 2025) may help with the interpretation of data from the Finnish study site (I guess this probably appeared too late for this submission). We found highest emissions under conditions of high soil temperatures and intermediate high soil moisture (associated with irrigation) and little or no immediate response to fertilisation events. Would the LDNDC model reproduce this?
>
> Whilst I am no expert on carbon modelling, my understanding is that models based on multiple pools with fixed turnover rates are not able to capture the key role of protection mechanisms (see e.g. Schmidt et al., Nature 2011). In the case of natural peatlands, the protection mechanism is waterlogging/anaerobicity. Is LDNDC able to represent this process adequately, and more generally is the pool-turnover approach justifiable for a peat soil? See also comment below about mineral association.

We appreciate the careful consideration given 1) to the conceptual limitations of applying a traditional multipool turnover model to drained peat soils, and 2) to the broader question of whether fixed pool turnover rates can adequately represent the key protection mechanisms governing peat carbon stability. We have extended the model description and the discussion of its limitations in the revised MS.pool turnover model to drained peat soils, and -pool turnover model to drained peat soils, and

LandscapeDNDC aims to reproduce the effects of waterlogging on the SOM decomposition by explicitly simulating the diffusion and consumption of oxygen in the soil profile. The oxygen partial pressure determines the anaerobic volume fraction in each soil layer, and the microbial metabolism and the enzymatic decomposition of

SOM are then evaluated separately for the aerobic and anaerobic volumes. The model has been used successfully to simulate the effect of waterlogging on the greenhouse gas exchange over rice paddies (Kraus et al., 2015), and we believe that its description of anaerobicity is reasonably close to the current state of art.

However, we fully agree with the reviewer that even with these details, the model does not offer a mechanistic description of processes such as organo-mineral associations that affect SOM persistence in organic (and mineral) soils. Our simulation is therefore, to some extent, an empirical approximation based on the existing data. We performed the sensitivity experiments described in Section 3.3 to check whether the predictions are robust against perturbations in model parameters, but the effects of structural uncertainties are difficult to assess in a study based on a single model. Our predictions regarding water table management need to be interpreted with care and if possible, eventually tested against experimental data. In response to the reviewer's concerns, we have substantially improved the Discussion regarding the applicability of the model for this kind of systems. We describe the conceptual limitations of the fixed turnover multipool approach when applied to (mineral enriched) drained peat and discuss the importance of WT and anaerobic protection mechanisms in peat decomposition, and to what extent LDNDC captures these processes.

Regarding N_2O, we agree that this is a challenging flux to model, especially given the limited number of long-term EC datasets available for agricultural peatlands. In the revised manuscript, we now discuss this limitation more explicitly and have used the reference to Cowan et al. (GCB 2025), which indeed provides additional interesting insights.

In addition, we have adjusted the description of the MeTrx sub-model in Section 2.3.1 as follows:

> *MeTrx describes the turnover of of litter and soil organic matter using six carbon and nitrogen pools, which are replicated for each soil layer. The model simulates the biogeochemical effects of waterlogging by evaluating C and N cycling processes separately for the aerobic and anaerobic volume fractions in each soil layer. The anaerobic volume fraction is diagnosed from the oxygen partial pressure, which in turn is solved explicitly from layer-wise profiles oxygen diffusivity and consumption. N2O-forming processes (nitrification and denitrification) are simulated based nitrifier and denitrifier growth and are dependent on substrate and oxygen availability, microbial activity and soil pH.*

We believe these additions provide the conceptual depth requested by the reviewer and clarify both the strengths and limitations of applying LDNDC to drained peat soils.

> **Detailed comments**
>
> L28 – Statement that Finland accounts for 1/3rd of European peatland area may depend on the definition of Europe (EU? Everything west of the Urals? Something else?). Please clarify.

This is clarified in the revised MS: we meant just EU and current candidate countries as 2006 definition.

> L39 – It is possible to either raise the water table, or to reduce the water table depth, but I don't think that "Raising the water table depth" makes sense. Depth is positive downwards (a 2m deep swimming pool is deeper than a 1m deep swimming pool) so either refer to something like groundwater level (positive upwards) or amend the terminology around WTD.

Thank you for pointing this out. This is corrected throughout the MS.

> L50 – See also Cowan et al (2025) which suggested that these intermittent emission pulses coincided with disturbance events during periods of low N demand, but not with fertiliser events when demand was high.

Thanks again for sharing this interesting study which we had not seen before the submission of our manuscript. Although it was unclear to us how it could support the sentence of L.50, we did think it would be relevant to add this reference in the previous sentence to further support our statement on current knowledge.

> L68 – What is the justification for the statement that LDNDC provides a suitable basis for modelling peat soils, given that the preceding text states that it has been shown to work on mineral soils? Perhaps better to say 'may provide a suitable basis'?

We agree. Corrected as suggested.

> L225-240 – It is not entirely clear whether some of the parameter values (e.g. Ksat) were derived from actual measurements, literature, or expert judgement. If the latter, can the authors confirm that parameter values were not iteratively fitted in order to reproduce the observations (potential equifinality/over-parameterisation issue noted above).

The values for Ksat used in the model were not fitted iteratively to reproduce the observations. Instead, they were based on measured values determined from soil samples. The values were unpublished but similar to those recently reported by Pham et al. (2026), where the mean Ksat of the peat samples was 0.00169 cm min^{-1} at the study field (minimum and maximum 0.000192 and 0.0054 cm min^{-1}).

We did adjust the van Genuchten parameters to ensure a physically realistic simulation of soil moisture, since we considered it a key control on SOM decomposition and N cycling. However, the values we used were within the ranges now reported (1.14 - 2.55 for n and 1 - 8 m^{-1} for alpha) in Pham et al. (2026; supplementary information).

> L239 – The presence of underdrains greatly enhances the movement of water between the peat and the ditch network (in both directions, depending on relative water levels). This effectively leads to a much higher but more heterogeneous hydraulic conductivity within the field. How was this handled in the model? A 1d model that treats the soil simple as a vertical two-layer system with uniform Ksat may not be able to reproduce field-scale variations in water table (or therefore emissions) if the drains are not somehow represented.

Thank you for raising this issue. We agree that the effects of the underdrains are difficult to capture in a 1-dimensional model. We therefore did not attempt to simulate the water table but instead used the measured WTD time series to force the model. This approach assumes that the effect of the drainage is captured by the temporal variation of the water table, which in turn affects the water content of the unsaturated soil layers by setting the lower boundary condition for the water movement within the soil profile. We acknowledge that a more sophisticated model of water dynamics would be a significant improvement. Nevertheless, based on the comparison against measured soil moisture, we believe that the current approach is sufficient to support the conclusions in this paper.

We added a note in the text as follows:

The soil moisture was simulated using the WatercycleDNDC (Kiese et al., 2011; Petersen et al., 2021) module in combination with a prescribed water table depth as described in Section 2.4.4. The prescribed water table was assumed to capture the effect of tile drainage, which cannot be explicitly simulated by the LDNDC.

> L243 and onwards – conventionally CO_2 emissions from land are reported (e.g. in IPCC guidance) in t CO_2-C/ha/yr, or t CO_2/ha/yr. I would suggest using one of these, and avoiding any ambiguity in the units used (kg/ha could be C or CO_2).

Thank you for pointing this out. This has been updated throughout the manuscript.

> L276 – How meaningful is the constant 15 cm WTD scenario? This is not environmentally realistic and appears an obvious outlier compared to the other scenarios in Fig 3. At the least, it would be helpful to also include a 15 cm variable scenario so that it is possible to make comparisons – at the moment it is hard to know how to interpret the difference in emissions between 15 cm fixed versus 30 or 50 cm variable scenarios.

Thank you for identifying this illogical scenario. We agree that a constant 15 cm WTD does not represent an environmentally realistic hydrological regime throughout the year. Our intention in including the fixed 15 cm scenario was to explore the theoretical maximum mitigation potential under very shallow groundwater conditions. Now, we acknowledge that the lack of a corresponding dynamic scenario limited the interpretability of the comparison with the variable 30 cm and 50 cm scenarios.

In response to the suggestion, we have now added a dynamic 15 cm WTD scenario, in which the water table follows a more realistic seasonal pattern centred around an average depth of 15 cm below the surface.

> L278 – 'Squeezing' is an odd term here – 'rescaling'?

Thank you, we agree. Corrected as suggested in the revised MS.

> L331 – Strictly I would consider that NECB should also include aquatic C loss and the CH4 flux (hence why we have referred to NEE + harvest as 'NEP', although I appreciate that this term is not used consistently either). If NECB is retained then please acknowledge which terms are omitted.

Thank you for your suggestions. We have kept the term NECB but we added the following clarification:

Other components that affect the NECB include organic fertilization, leaching of dissolved carbon, and the atmospheric exchange of methane. We did not include these fluxes, because organic fertilizers were not simulated or used in the years covered by the EC measurements and because the contributions of methane and leaching were found to be small compared to CO2 based on earlier data on the same site (Yli-Halla et al., 2022).

> L343 – Regarding drier measured vs simulation conditions in early summer, see my comment above about the role of subsurface drains.

We refer to the response for comment L239.

> L373 – The units here (kg C/m2/yr) are different to the ones used earlier. I recommend using either t CO2-C/ha/yr or g CO2-C/m2/yr throughout the paper. Similarly for N2O, you could use kg N2O-N/ha/yr for consistency with IPCC emissions reporting.

Fixed.

> L378 – Delete 'get'

Deleted.

> Figures all look nice but some fonts (especially numbers along axes) are small, and will become very small if they are reduced in size for final publication - please enlarge.

Thank you for noting this. Fixed the smallest fonts in figures.

> Fig 7 – I think it is more conventional to plot observations on the x axis and simulated values on the y axis.

We appreciate the referee's point of view. We acknowledge that a case can be made for plotting the simulated values on the y axis (Pauwels et al.2019) as well as for the opposite (Piñeiro et al, 2008). For the current manuscript, we prefer not to change the convention, since this would also affect the regression results.

> Fig 8 – Grey dots in lower plot (EC N2O) fade into the background a bit, but are really rare and important data so perhaps make them more prominent, e.g. black? The years in these plots should also be labelled with the crop (grass/barley) that was present in that year, to help with interpretation (also for LAI and ET)

Fixed.

> L424 – See general comments about the validity of using a fixed-turnover C model for peatlands.

We refer to our response to the reviewer's general comments above.

> Fig 12 – Use ‘decrease’ and ‘increase’, not ‘decreas.’ and ‘increas.’ As with other figures, fonts will be too small if this figure is smaller in a paper.

Fixed

> L460 – It is a significant overstatement to claim that the match between simulated and obsvered values (for a limited number of observations, following what seems to be a considerable amount of parameter adjustment) ‘confirmed’ that the model is suitable for agricultural peatlands. At best, I think the results maybe ‘supported’ the use of the model, subject to caveats.

Thank you for pointing this out. We agree and have revised the text accordingly.

> L465-471 – See Cowan et al (2025) for some more recent EC measured N2O flux data from an agricultural peatlands.

The study of Cowan et al. (2025) was added in section 4.2 to support our discussion. Although Cowan et al. (2025) does highlight that the raise in N2O emissions during the potato crop couldn’t entirely be attributed to higher WT and soil moisture, they did raise an interesting point on how WT could potentially increase N2O emission and negate CO2 mitigation strategies. Due to the uncertainties in the simulated N2O emissions, it is difficult to evaluate whether the model accurately captures the response of N2O emissions to changes in the soil moisture and consequently, its impact on the overall mitigation potential. However, it is an important point to keep in mind in further modelling studies. Additional measurements on drained agricultural peatlands with raised water table would also be valuable to enable more robust evaluations of the simulation results. These points were raised in section 4.3, where the Cowan et al. (2025) study was also used as a reference.

> L475 – For obvious reasons I would be interested to know how the results (observations and model outputs) compared to the Evans et al (2021) relationship between CO2 and WTDe (i.e. drained peat depth). In one scenario (WTD 50 cm, shallow peat ~ 38 cm) the water table was not raised into the peat layer (at least on average) so our simple function would predict no reduction in emissions. This is over-simplified of course, because with variable WTD will rise into the peat layer during wet periods, but still I would expect the emissions reduction to be small (also if a more sophisticated aerated C stock value is used). If the authors are able to offer any further insights about this that would be a useful addition to the discussion.

> [I see that this question is briefly discussed on L518, but without really commenting on why some mitigation is predicted in this situation. I guess it may be for the reasons above?]

Thank you for bringing this to our attention. Due to seasonal variation, we indeed observe water table levels reaching the peat layer. Additionally, given the way in which our scenarios were constructed, the water table never drops below the measured water table level, and the response to drawdowns is slower. We elaborate this in the manuscript and explain that peat layers being submerged more frequently and for longer periods in the scenario runs is possible most likely reason for mitigations.

> L491 – The %C data in Table 1 indicate a high degree of mineral mixing with the peat in most or all of the fields. Have the authors considered whether organic matter stabilisation onto mineral surfaces might influence carbon turnover and emissions? It does not seem like LDNDC considers this possible mechanism. It seems potentially more important than (unspecified changes in) 'peat quality' as a possible reason why CO2 fluxes and C stocks might become partly decoupled.

Thank you for bringing this up. LDNDC does not simulate the OM protection mechanisms explicitly. The model includes a response function that adjusts OM turnover rates based on the clay content, but it is unclear whether this approach would be enough to meaningfully simulate the mineral mixing. We agree that explaining the apparent decoupling between CO2 fluxes and C stocks is likely to require a mechanistic understanding that goes beyond the concept of "peat quality". We now mention this possibility in the Discussion section 4.3.

> L505 – See earlier comment about the realism (or otherwise) of a flat 15 cm WTD scenario. Even a very well managed paludiculture field would struggle to maintain this uniformity, so I am not convinced that this scenario is meaningful in practical terms. A variable 15 cm scenario would be more enlightening (as the subsequent discussion here suggests) so why not include this? If a variable 15 cm scenario (with biomass harvesting) predicted that the system would be close to C-balanced that would be an interesting and important result.

The scenario is replaced by more realistic, dynamic 15 cm scenario as described earlier.

References

Pauwels, V.R.N., Guyot, A., Walker, J.P., 2019. Evaluating model results in scatter plots: A critique. Ecological Modelling 411, 108802. https://doi.org/10.1016/j.ecolmodel.2019.108802

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Piñeiro, G., Perelman, S., Guerschman, J.P., Paruelo, J.M., 2008. How to evaluate models: Observed vs. predicted or predicted vs. observed? Ecological Modelling 216, 316–322. https://doi.org/10.1016/j.ecolmodel.2008.05.006