Dear reviewer,

We sincerely thank you for the constructive assessment of our work. We appreciate the recognition of the novelty in evaluating the LDNDC model for peatland. We also thank you for highlighting areas where the manuscript can be more informative. In response, we have now for, example, added a more detailed description of the nitrogen cycle processes in LDNDC, and provided additional details on the model calibration.

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.

Best regards,

Henri Kajasilta, on behalf of all authors

> Comment on "The effects of peat thickness and water table depth on CO2 and N2O emissions from agricultural peatlands - a process-based modelling approach" by Kajasilta et al., Biogeosciences
>
> General comments
>
> In this manuscript, authors applied LDNDC to simulate C and N dynamics in a single peatland field site following traditional grass-intensive crop rotation, and examined the change of net ecosystem carbon balance (NECB) and N2O emission under different prescribed peat thickness and water table depth. Although only one site is studied, this study also evaluated the capability of LDNDC to simulate C/N dynamics of peatland,

> which can be considered as a novel work. Though most works are solid, some key information is missing and shall be presented to fortify major findings. In addition, the description of the N cycle in LDNDC and its calibration is missing.

We appreciate the recognition of the novelty in evaluating the LDNDC model for peatland. We also thank you for highlighting areas where the manuscript can be more informative. In response, we have now added a more detailed description of the nitrogen cycle processes in LDNDC, and provided additional details on the model calibration as described below.

> Specific comments:
>
> Authors calculate soil water movement but enforce water table depth to the model at the same time. Based on my understanding, soil below the water table shall be saturated and only the soil moisture calculation above the water table is meaningful. Authors shall provide more information to justify your model treatment and explain how the boundary conditions and calculations have been changed in LDNDC after overlapping prescribed water tables.

We agree that soil below the water table should be saturated and that prescribing a water table depth can potentially interfere with the simulation of soil water movement if not handled carefully. We would like to clarify that the soil water movement module itself was not altered and no changes are made to boundary conditions. Here, we only forced the position of the water table by prescribing a saturated water content for the soil layers below the water table. The soil moisture dynamics above the water table are calculated normally by the model. We have added a clarification to make this explicit in the Model overview section as follows:

"The soil water movement is simulated using layer-specific field capacity and wilting point (governed by van Genuchten parameters) and saturated hydraulic conductivity. Each soil layer thickness can be manually defined and assigned with proper hydraulic properties according to the measured peat and mineral soil characteristics. The upper boundary condition is atmospheric, driven by infiltration and evapotranspiration, and the lower boundary is a free drainage condition. "

We furthermore added a sentence to section 2.4.4: "*A prescribed WTD was used in all simulations. In this mode, the model forces a saturated soil water content for layers below the WTD and simulates the soil water flow above the water table*".

> Since the EC tower is installed in the middle of the field, authors should provide more description to justify how they can obtain EC data only representing block 5 and 6. How authors conducted footprint analysis and what criteria used to filter EC observation contributed from block 1-4?

The eddy covariance technique measures total gas transport using 10 Hz measurements of wind speed and direction and gas concentration from the prevailing wind direction at the measurement height. The 30-min flux data originating from unsuitable wind directions (here: 0-135 and 300-360, where Blocks 3 and 4 were located) can be discarded based on the mean wind direction of the same period, which we did. In addition to simple wind-direction filtering, a footprint model is typically used to ensure that the flux signal originates from the target area (here: Blocks 5 and 6). Footprint models use different parameters (ex. wind speed, vertical heat flux, measurement height) to estimate the cross-wind-integrated footprint distribution from the eddy covariance tower to the edge of the target area. Generally, it is required that at least 70% of the flux signal has to originate from the target area in order not to discard the 30-min flux from further analysis. We used a well-established footprint model developed by Kljun et al. (2015) with the 70% threshold for the flux signal. We clarified the footprint analysis in Sect. 2.2.1.

> Authors mentioned air temperature measurement as an environmental variable, but there's no analysis related to air temperature in this article. Please explain why you need air temperature measurement.

Air temperature is included because it is one of the key environmental drivers for LDNDC. Although we do not present a standalone analysis of temperature, it directly influences several core processes in the model. For example, air temperature is used to calculate growing degree days (GDD), which control plant development, growth, and senescence, and therefore directly affect simulated biomass production and carbon inputs to the soil. Air temperature is also used by the model to calculate the soil temperature, which regulates many microbial processes.

> Model description shall be informative. Authors at least shall provide enough information for readers to understand model structure and parameters. I would suggest authors to explain: 1) Major model processes, which authors may have already mentioned a bit, but need to consolidate and rephrase. 2) Model structure: How plant type is represented? How many organic C, organic N and mineral N pools are defined in different modules? How many layers of soil and the node depth of each soil layer? 3) Add description in table S1 to explain key parameters used in these key processes and 4) explain the reason why choosing parameters in Table S1 for calibration. I also have some minor suggestions/questions related to model description, so feel free to rephrase this part if necessary.

In line with the reviewer's comments, we have updated the model description to provide clearer explanations of the model structure and parameters. In particular,

1. Description on how the model handles soil water dynamics has been added to the model overview section.

2. For the study years we have used two different plant types: perennial grass and barley depending on what has been growing on the field. In principle, different plant types have distinct parametrisations, which can be altered by explicitly changing the parametrisation for the whole plant type (affecting all corresponding instances) or targeting individual planting events. The changes in parametrisation are described in the corresponding section 2.3.2 in the manuscript and therefore the following concise description has been added to section 2.3.1:

   Each plant type is characterized by a distinct parameterization that governs its responses to key environmental drivers such as radiation, precipitation, and temperature.

3. We now describe the model's soil C and N pool structure as follows:

> *MeTrx describes the turnover of litter and soil organic matter using six carbon and nitrogen pools, which are replicated for each soil layer along with the pools of inorganic nitrogen (ammonium and nitrate) and dissolved organic carbon and nitrogen.*

The setup of the soil layers can be seen in supplement Table S5. We also clarified the soil layers and node depths in the manuscript (see also responses to other comments).

4. Parameters were selected to correct undesired model behavior and to enhance dynamics seen in the empirical observations. The modifications to decomposition rates (METRX_KR_DC_HUM) were necessary to achieve representative emission levels, while modifications to GDD and senescence parameters were crucial to replicate plants' seasonal dynamics in northern latitudes. Description of the parameters have been added to Table S1.

> Though evaluation is performed, no calibration on the N cycle, especially mineral N, is done in this study. I understand the mineral N observation is hard to obtain, but authors shall explicitly explain why the calibration cannot be done?

We agree that ideally the N cycle would also be calibrated based on local data. However, we believe that the N2O flux data used in this study would not be sufficient for a robust calibration of the N cycle, because the N2O emission represents only a small fraction of the entire N turnover. The N2O flux, even if continuously measured, therefore provides only a weak constraint on the N cycle. Furthermore, using N2O flux as a calibration target would likely suffer from confounding factors, since N2O is emitted by several microbial processes with complex responses environmental drivers and soil C and N availability. We therefore decided to not calibrate against the N2O fluxes but instead to publish the results using default parameterization, as a basis for more N-focused studies in the future. We added this explanation in the end of revised Section 4.2.

> Conclusions from this study are based on results from one model. Though the parameter uncertainty related to respiration and evapotranspiration is explored, uncertainties from other parameters and from model structure are not considered so it shall be discussed at least, e.g., certain simplification of model representation can cause under-/overestimation of NECB/N2O emission?

Thank you for this important comment. We agree that parameter and structural uncertainties beyond those analysed in our study should be acknowledged. In this study, we focused on a selected subset of parameters because these processes are most directly linked to peat decomposition and N_2O emissions, and because a fully exhaustive uncertainty analysis is not feasible given that the LDNDC model contains hundreds of parameters. Therefore, we chose this targeted approach to retain interpretability.

At the same time, we recognise that peat is a highly heterogeneous and complex soil material, and many physical and chemical properties not explicitly varied here (e.g., peat degree of decomposition, hydrological conductivity, N dynamics) can influence C and N turnover. Indeed, our results showed a strong correlation between NECB and exposed organic matter, but in reality, multiple peat properties interact to regulate decomposition rates. Therefore, other parameters not covered in our analysis may contribute additional uncertainty to NECB and N_2O estimates.

We also acknowledge model-structural limitations. LDNDC was originally designed for mineral soils, and although it has been adapted to peatlands, some simplifications such as representation of anaerobic microsites may lead to under- or overestimation of peat mineralisation and associated GHG fluxes. In the revised MS, we explicitly discuss that model structure may affect simulated NECB and N_2O emissions, and that future work should include broader parameter uncertainty analysis and possibly multi-model comparison. We have now expanded the Section 4.2 to clarify these limitations.

> Technical correction:
>
> Line 79: "chemical soil properties" shall be "soil chemical properties"

Thank you. This is corrected.

> Line 95: If both blocks 5up and 6up have similar peat layer thickness to block 5 and 6, I suggest changing the color of block 5up and 6up in Figure 1.

Thank you for this suggestion. We do not have direct soil measurements from blocks 5up and 6up, but based on the available information of the organic horizon thickness (Yli-Halla et al. 2022), it is reasonable to assume that their peat layers are more similar to blocks 5 and 6 than to the blocks on the northern part of the site. However, because measurements are unavailable and these two blocks were intentionally excluded from the simulations, we consider it clearer to indicate this distinction in Figure 1 by using different colours. This visual separation helps avoid implying that blocks 5up and 6up were treated identically to blocks 5 and 6 in the modelling setup.

> Line 125: "following (Vekuri et al., 2025)" shall be "following Vekuri et al. (2025), "?

Thank you. This part is removed entirely during the revisions.

> Line 128: "gaps of two hours" shall be "gaps within two hours"?

It is actually gaps of 2 hours or less. We added this information to the revised manuscript to make it clearer.

> Line 130: What is the size of the window for your moving average?

The initial window size was 3 days, and if needed, it was increased until two adjacent observations were available as described in Gerin et al. 2023. We have now included this additional detail in the manuscript.

> Line 131: The cited paper Vira et al. (2025) does not describe how you process measured ET?

We apologise for the missing data-processing details for the ET; they are indeed not included in the referenced paper. ET data were processed simultaneously with CO_2 fluxes, and the data filtering steps were the same as for CO_2 fluxes, but accounting for H_2O flux-specific processing options. We have clarified H_2O processing in the revised Sect. 2.1.1:

> *Half-hourly evapotranspiration (ET) was calculated and processed with the Eddypro software (v. 7.0.9, LI-COR Biosciences, USA) by applying similar filtering steps as described in Vira et al. (2025) for CO_2 but using steady-state flags and spectral corrections specific to water fluxes.*

> Line 148: "LAI was evaluated using the methods described in" Do you have in-situ measurements of LAI you did evaluate against S2 products?

Thank you for the question. We have compared satellite-based LAI estimates with field measurements on grassland with good performance in southern Finland in Heimsch et al. (2024). In addition, several studies provide performance analysis of Sentinel-2 LAI retrievals in varying environments (e.g. Brown et al., 2024; Kganyago et al., 2024). Nevertheless, we acknowledge that the performance of satellite-derived LAI can vary by location, and therefore we cannot fully guarantee that the Sentinel-2 LAI is equally accurate at our study site. We have improved the description of the Sentinel-2 originated LAI estimates and added this clarification to the revised Methods. In addition, we acknowledge this in the revised Discussion.

> Line 154: "layer-wise representation" How did you define the soil layer depth, node depth and top/bottom boundary conditions for your soil water movement/dynamics module to simulate WTD dynamics?

The layerwise representation of the soil profile is described in Supplementary Table S5. We simulated the topmost 2 meters of soil, and depending on the block, 20-60 layers were used in the numerical discretization. The layer thickness increased from 2 cm near the surface to a maximum of 30 cm at the bottom. In each soil profile, the hydraulic and chemical soil properties were assigned to the layers by grouping them into 6 distinct strata based on site-specific measurements of the organic and mineral horizons (Yli-Halla et al. 2022). For each stratum, the bulk density was set to the measured values, but due to the C and N loss during the spinup years we compensate the C and N contents in the topsoils to match the stocks with the measurements in 2020.

The soil water movement was simulated using layer-specific van Genuchten parameters and saturated hydraulic conductivity. These hydraulic properties were defined separately for each soil layer to represent water movement in peat and mineral soils. The water table depth is not simulated but prescribed based on the measured time series or by the counterfactual scenario (Section 2.4.4). The upper boundary condition is defined by infiltration and evapotranspiration, and the lower boundary is a free drainage condition. Due to the forced water table, the lower boundary condition effectively applies at the level of the water table.

We have clarified these details in the revised sections 2.3.1, 2.4.1 and 2.4.4 to improve transparency regarding how WTD dynamics are represented.

> Line 160: It seems like "Plamox" is an abbreviation. Can you tell readers the full name?

Plamox is a "Plant growth model" used in the LDNDC (Liebermann et al. 2019). We added a clarification about it in the revised manuscript.

> Line 162: What does "CanopyECM" mean?

CanopyECM stands for "Empirical Canopy Model". We added this in the revised manuscript.

> Line 163: It seems like "MeTrx" is a module representing soil biogeochemical processes? Please provide the full name.

MeTrx is an abbreviation of "Metabolism and Transport of x" in the LDNDC model. The main focus for this submodel is to simulate production and consumption of CO2, N2O and CH4. We added a clarification about it in the revised MS.

> Line 169: "as soils may exhibit substantial annual losses" shall be "as soil organic carbon may exhibit substantial annual losses"

Thank you. This is corrected as suggested.

> Line 172: "The module handles the dynamics of water within the soil profile" can be simplified "The module handles the soil water movement, and accounts for the amount of precipitation intercepted by foliage, infiltration, ..."

Thank you. This is corrected as suggested.

> Line 173: "possible changes in snow cover and ice content in the soil" This requires air temperature and soil temperature profile. How does your model obtain this information? By calculation or use prescribed temperature profile?

The soil temperature and its vertical variation are simulated by solving the heat transfer equation within the soil column, with the upper boundary condition given by the air temperature. Precipitation is assumed to be snow when air temperature is less or equal to a given temperature limit, which is kept as default in the model (0 °C). Ice forms in the soil, when the soil temperature drops below the given ice temperature parameter (kept as default, 3 °C). More information on the calculations for the formation of ice is included in the revised LDNDC model descriptions.

> Line 174: "Evapotranspiration follows the potential evapotranspiration and is limited either by the amount of surface water or remaining potential evapotranspiration, whichever is reached first." Actual evapotranspiration (AET) is different from potential evapotranspiration (PET), which can only be equivalent if you are calculating ET from a submerged surface. In your case however, you have drained peatland blocks during a certain time period, so they are different. Please clarify.

We agree that this wording was misleading. We have revised this as follows:

> *"Actual evapotranspiration is limited by water availability and is therefore calculated as the minimum of potential evapotranspiration and the available water at the surface or in the upper soil layer."*

> Line 180: "The default value of spinupdeltac is 0, corresponding to the original equilibrium assumption, but it can now be set to reflect user-defined annual changes." What is the value of this "user-defined annual changes" in your study?

In our study, the user-defined annual change in the humus carbon pool during the spin-up (spinupdeltac) was derived from site-specific measurements of NEE and harvested biomass from the shallow-peat blocks 5 and 6. Based on these measurements, we prescribed an annual carbon loss of 4500 kg C ha^{-1} yr^{-1} during the spin-up years. This value represents the estimated long-term net C balance of the site under conventional management.

However, we note that due to adjustments made to the carbon decomposition parameters in our simulations, the modelled C loss during the spin-up period did not exactly match this prescribed value and was somewhat larger.

We added a cross-reference from Model overview section to Site initialization where the use of the parameter is explained with values.

> Line 186: "we increased the decomposition rates" What method did you use to determine the increment of the decomposition rate?

The decomposition rates were adjusted manually based on a set of ensemble simulations, in which the parameters were sampled using a Monte Carlo technique. We have added clarification in the section 2.3.2.

> Line 187: "model initialises most of the carbon and nitrogen in two pools that represent young and old organic matter," Should this be labile vs. recalcitrant? Also, how did you determine their relative proportion without observation?

We adopted the young/old characterization to remain consistent with the model description in Kraus et al. (2015). The model allocates C and N between SOM pools automatically during initialization based on soil properties, most notably the C/N ratio, which influences the partitioning between relatively younger and more decomposed material. -specific measurements to directly validate the exact initial distribution between these two pools.

> Line 189: "resulted in spurious blockwise variability in soil respiration" can you describe how spurious it is? For example, respiration in a certain block becomes extremely high or low? Also, I am confused that authors change parameter values but claim "no differences in the parameterisation between the blocks", then why is there a blockwise variability in soil respiration?

The blockwise variability occurred despite using identical parameterisation across all blocks, because each block had a unique soil profile derived from its measured organic layer depths and carbon and nitrogen contents. These differences in soil profiles influenced the initial C allocation between the recalcitrant pools, which in turn affected the simulated respiration rates.

Before adjusting the decomposition rates, this led to unrealistically large differences in respiration between blocks that were otherwise similar in measured carbon stocks. For example, blocks with slightly deeper organic horizons or higher initial C content experienced high respiration during spin-up, while adjacent blocks with nearly comparable characteristics showed much lower values.

By modifying the decomposition parameters of the largest carbon pools, we reduced this artificial divergence. After these adjustments, respiration levels across blocks became more consistent and more closely aligned with their respective carbon stocks, while still reflecting real differences in peat depth and carbon content. We added some clarifying wording in the section 2.3.2 to explain why blockwise variability occurred and how the parameter modification resolved the issue.

> Line 199: "we adjusted parameters handling the photosynthesis activity (H2OREF_A) and stomata closing (H2OREF_GS)" Can you show me the actual name of these parameters? If readers cannot get detailed information about these parameters, the rest of the description on how you adjust these parameters to "avoid underestimating the GPP" does not make sense.

We agree that the parameter names and their meanings should be stated explicitly. H2OREF_A is the parameter that defines the relative available soil water content at which drought begins to reduce photosynthetic activity. H2OREF_GS defines the relative available soil water content at which stomata are fully closed due to drought stress. Both parameters range from 0 to 1, where lower values indicate that drought stress affects photosynthesis or stomatal conductance later, i.e. plants maintain activity under drier conditions. Conversely, higher values imply stronger drought sensitivity. We have clarified the full parameter names and their functional roles in the revised manuscript to ensure transparency and avoid confusion for readers.

> Line 202: "The drought periods were not seen in EC measurements" If you don't have observation based evidence, how can you be sure that low GPP is problematic?

We mean that the model was unable to reproduce the observed GPP because of a too strong response to low topsoil water content. We clarified this part to explicitly state that the EC and satellite-derived LAI data were available but showed no indication of drought-related anomalies.

> Line 226 - 236: This shall be part of calibration not initialization.

Thank you for pointing this out. We fixed the header for this section to match better with the content.

> Line 254: How did you treat tillage in LDNDC? For example, change the soil porosity and the base decomposition rate of C/N pools after tillage?

Tillage has three effects on the organic matter pools in LDNDC: 1) aboveground litter or stubble are incorporated to the soil litter pools, 2) the C and N pools are homogenized within the tillage depth and 3) the OM turnover is accelerated temporarily by up to 300 % depending on the depth and soil type and relaxed back to normal over 30 days. Physical soil properties are not affected. The tillage effects in LDNDC have been discussed in Haas et al. (2022).

We have added the following information in Section 2.3.1:

The effect of tillage is simulated by homogenizing the C and N pools within the tillage depth and by temporarily accelerating the organic matter decomposition as described in Haas et al. (2022).

> Line 256: The rest 1% of biomass is removed from the site or is kept alive?

It was removed from the site. This is updated in the revised MS.

> Line 267: Do you also calculate CH4? If not, why is the ambient CH4 level mentioned in this study?

We did not consider the exchange of CH4 in this study, however, the model simulates CH4 and requires its ambient level as a boundary condition. A note has been added to the text.

> Line 268: Remove "the" from "similar to the those measured"

Thank you. This is corrected as suggested.

> Line 314: Is the length of the vector equivalent to the length of the observed time series?

Yes, the length of simulated values was the same as the length of observed values. However, as we had up to four observed values in one time step, the corresponding simulated value was related to all observed values in the given time step. See the comment related to Figure 7. We will clarify that one simulation output can relate to several measurements, if the time stamps align.

> Line 336: Describe how you define and calculate N2O balances?

This is described now in the revised MS.

> Line 348: "Soil measurements during the winter time were unreliable and should not be emphasized due to the measurement problems when the

> soil is frozen or close to that point." This is not your result. Shall move to section 2.

Thank you. This is moved as suggested.

> Figure 7: It seems like some of the modeled values are repeating, reflected by the almost same modeled values against completely different values from chamber measurement. Can you explain this phenomenon?

That is true. In principle, there have been four chamber measurements at one block, which all are plotted against the simulated value at the given time step. This is now clarified in the revised figure caption.

> Line 463: "However, the simulated respiration differed between shallow and deep peat fields, which led the model to underestimate the ecosystem respiration for the shallow peat fields and overestimate it for deep peat fields." can be simplified to "However, the model underestimates the ecosystem respiration for the shallow peat fields and overestimates it for deep peat fields." Also, you can merge this one with the previous one paragraph.

Thank you. This is simplified as suggested.

> Line 466: "the other years were either under or overestimated by up to a factor of two". Can you provide a certain evaluation about how this over-/underestimation of N2O can bring impact to your major conclusion of N2O balances under different WTD scenarios?

We agree that this is an important question, since some studies (e.g. Cowan et al., 2025) suggest that increasing N2O emissions might negate the effect of CO2 mitigation from raising the WTD. The uneven model performance for N2O raises a valid concern of whether the model accurately captures the response of N2O emissions to the changes in moisture conditions. We address this uncertainty in Section 4.2 and further discuss it in Section 4.3.

Based on the available data we are unable to quantitatively assess how the uncertainty of the N2O emissions impacts the overall conclusions. However, we note that in the strongly mitigated 30 and 15 cm scenarios even large increases (+50-100 %) in N_2O emissions would not cancel the mitigation from reduced CO_2 emissions. However, the conclusions regarding small WTD interventions (the 50 cm scenario) could be affected if the N_2O production was found to increase steeply with small changes in the WTD. Measuring the N_2O fluxes over agricultural sites with a high water table would be valuable in reducing the model uncertainty and for understanding how the WTD response of N_2O emissions interacts with pedoclimatic conditions.

> Line 482: "supports the hypothesis that increasing the water table can suppress nitrification and subsequently reduce the availability of nitrate for denitrification" Did LDNDC produce the similar phenomenon? I believe most of the popular models have these two fluxes as output so I would recommend authors to check model outputs.

Thank you for suggesting this. LDNDC produced similar phenomenon and nitrification was suppressed when increasing WT. We added the results to section 3.2.2 and expanded the discussion in section 4.3.

> Line 500: "This sensitivity analysis showed that our findings regarding the CO_2 emissions were more robust to parametrisation than the absolute CO_2 emissions." Can you rephrase this sentence? It is a bit tough for me to interpret?

The sentence is rephrased in the revised MS, making it easier for everyone to interpret.

> Line 512: Incomplete sentence. "The scenario with a 50 cm average WTD required on average a 31 cm higher water table for deep peat blocks and a 44 cm higher water table in shallow peat blocks"

The sentence is completed in the revised MS.

> Line 519: "below the organic soil horizon in the shallow peat blocks". What is the depth of the O horizon for shallow and deep peat blocks, respectively?

We added here a reference to Table 1 where the information can be found. The shallow and deep peat blocks are defined in the Section 2.1.

> Table S1. Add explanation of all parameters you have calibrated in table S1.

Fixed.

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