Zhang et al. describe an increase in process representation in CNMM-DNDC model. Schemes for allocation, respiration, and mortality were added. The model was set up to run at three forest sites. Model outputs were compared to eddy-covariance data. The model was calibrated and validated, and a sensitivity analysis was performed. My primary concern about this analysis is that I did not perceive a substantial contribution to modelling science. I welcome clarification from the authors, but on my reading of the manuscript, I did not see new concepts, ideas, or methods.

Revised.

The improvements of forest growth module for the CNMM-DNDC would be a valuable contribution to the corresponding modeling community which has been stated in the revised manuscript. "*To achieve such improvements is a valuable contribution to the corresponding modelling community. Firstly, as the CNMM-DNDC aimed at modelling or predicting of multiple ecosystem variables (e.g., emissions/uptakes of carbon and/or nitrogen gases from terrestrial ecosystems, evapotranspiration, productivity, soil erosion, and slope runoff and catchment discharges of particle and/or dissolved carbon, nitrogen and phosphorous at plot, ecosystem, landscape, catchment/river basin, regional or global scales) concerned in the implementation of the United Nations Sustainable Development Goals (SDGs) by 2030, the shortcomings in simulating the biogeochemical processes of forest ecosystems would hinder the effective application of the model. Secondly, the reliable CNMM-DNDC model with new growth module would have the potential to study the interactions between forest carbon pools and hydrological processes, such as the losing of soil organic carbon due to the thawing of permafrost, which has attracted more attentions under climate change.*" (**See lines 107-116 in the revised manuscript**).

Beyond the issue of the significance, I think that several other points would need to be addressed:

1) The general description of CNMM-DNDC in section 2.1.1 can be improved. I would like to know what the state variables are, what kinds of equations govern the state variables, and what kinds of boundary conditions, initial conditions, and forcing the model requires.

Revised.

The equations, state variables and parameters related to the carbon and nitrogen cycling has been added in Tables S1-S3 of the supplementary materials (**See Table S1-S3 in the revised supplementary materials**). The requirements for the simulation, including boundary conditions, initial conditions and forcing, have been detailed as reviewer suggested. *"The data required for the simulation include land use type, soil properties of individual layers (soil organic carbon, total nitrogen, clay content, bulk density, pH, etc.), meteorological forcing (hourly air temperature, precipitation, wind speed, solar radiation, etc.), biological data (plant type, nitrogen content, plant height, root depth, etc.), initial conditions (soil depth, soil temperature, soil moisture, annual amounts of dry and wet nitrogen deposition etc.), management practices (start and* *end dates, methods and/or amounts of individual management practices including tillage, fertilization, irrigation and flooding for croplands), and boundary data (start date, period, time step of simulation, depth of soil profile etc.).*" (**See lines 136-142 in the revised manuscript**).

2) I think that Equation 21 has an error. I propose that the numerator should be 1 - f_HR - R_{soilCN} / R_{litCN}. Is this a typo? A bug in the code?

Revised.

The equation has been revised. It is a typo, but not a bug in the code (**See Eq.21 in the revised manuscript**).

3) I would be interested in a description of N cycle inputs (fixation, deposition, etc.) and outputs (gas losses, leaching, etc.). Are values for these quantities known at the study sites?

Revised.

The descriptions about nitrogen fixation and deposition have been added as reviewer suggested. "*The nitrogen fixation was considered using the default value of 0.0004 kg* m^{-2} y^{-1} *in Biome-BGC during the simulation (Fang, 2022).*" (**See lines 332-333 in the revised manuscript**). "*......as well as the nitrogen deposition, were primarily obtained from the National Ecosystem Science Data Center (NESDC; https://www.nesdc.org.cn/). Based on the annual amounts of dry and wet nitrogen deposition (Jia et al., 2019; 2021), the daily dry nitrogen deposition and the nitrogen concentration in wet deposition were calculated as model driving.*" (**See lines 317-320 in the revised manuscript**).

4) More information needs to be provided on how the model calibration was carried out. What kinds of algorithms were used? How many simulations were done? How was convergence assessed?

Revised.

The statements about model calibration have been added. "*The required parameters for forest simulation, including forest type, carbon contents of leaf and stem and some of eco-physiological parameters, were primarily adapted from the field observations provided by the NESDC or from the peer reviewed literatures (Li, 2018; Li, 2019; Fang, 2022). The other eco-physiological parameters referred to the default values (Table 1). The parameter of fraction of leaf nitrogen in Rubisco (p32) was calibrated using the normalized root mean square error (NRMSE) between observed and simulated carbon and water fluxes during 2003–2007. The upper and lower boundaries of the parameter value (p32) were set as twice and half of the default value. The parameter was identified when the value of NRMSE was the minimum.*" (**See lines 326-332 in the revised manuscript**).The sources of the eco-physiological parameters have been marked in the revised Table 1, indicating the values from literatures or calibration, as well as the range of parameters used for calibration. (**See Table 1 in the revised manuscript**).

5) Line 323: The N cycle can take much longer than 13 years to equilibrate (Thornton and Rosenbloom 2005, Ecological Modeling, 189, 25-48). In what sense is the 13 year spin-up really satisfactory? Is the N cycle still far from equilibrium?

Reponses.

The details have been added to make it clear. "*In this study, the soil carbon and nitrogen pools were initialized by the observed data. The initial state of forest was constrained by the carbon contents of leaf and stem based on the observed biomass, as well as the proportion among different organs, which was not the model's native dynamics (Table S5; Thornton and Rosenbloom, 2005).*" (**See lines 336-339 in the revised manuscript**)

6) The OAT sensitivity analysis is problematic. From the Discussion section, the authors seem to be aware that it is problematic. As things stand in the manuscript, I do not have confidence in the sensitivity analysis results. It would be improvement to show scatterplots so that the linearity of the response could be assessed, but that still wouldn't solve the problem of parameter interactions. Using something like the Morris method would address these issues, and wouldn't really require more iterations.

Revised.

Both the OAT and Morris method has been applied for the sensitivity analysis. Monte Carlo Monte Carlo test with Latin hypercube sampling was used in the Morris method with the iterations of 2000. (**See lines 371-387 in the revised manuscript**). "*The global sensitivity analysis using Morris method (Fig 5) showed similar results at the CBM site with selected sensitive parameters of SLA (p30), FLNR (p32) and carbon allocation rate of new fine root to new leaf (p10). Annual GPP and ER fluxes at the QYZ site were sensitive to SLA (p30), FLNR (p32) and LFRT (p6) using the both methods of OAT and Morris. However, only canopy light extinction coefficient (p28) was identified as a sensitive parameter at the DHM site, which was not identical to the results of OAT.*" (**See lines 483-487 in the revised manuscript**). "*For the results of global analysis, SLA, FLNR and LFRT were also identified as sensitive parameters. But the effects of carbon allocation rate of new fine root to new leaf or canopy light extinction coefficient on annual GPP and ER could not be ignored at the site of CBM or DHM, respectively. Canopy light extinction coefficient determines the amount of absorbed photosynthetically active radiation and thus regulates the GPP and ER (White et al., 2000). The analysis of eco-physiological parameters suggests that the sensitive parameters may be consistently influential, independent of sites or the type of sensitivity analysis. But the ranking of the parameters may vary according to specific species and regions (Raj et al., 2014).*" (**See lines 605-611 in the revised manuscript**). "*Thus, global sensitivity analysis using the Morris method was also applied which can reflect the interactive effects of changes in multiple parameters/inputs (Odongo et al., 2013; Raj et al., 2014). The results of both methods were not totally consistent for three sites, which proved the limitations of OAT and necessity of global sensitivity analysis considering the comprehensive effects of* *multiple parameters/inputs (Saltelli et al., 2000; Odongo et al., 2013).*" (**See lines 636-639 in the revised manuscript**).

7) I would have liked to have seen a discussion of whether the fitted parameter values were reasonable, and whether the variation across sites made sense in terms of basic biology.

Revised.

The calibration and sources of eco-physiological parameters have been detailed as mentioned above. The discussion about parameters among three sites has been added as reviewer suggested. "*The eco-physical parameters used in this study are comparable with those measured or calibrated in other studies (Tables S8−S9; Li, 2018; Li, 2019; Fang, 2022). Due to the limited studies at the QYZ site, the required eco-physical parameters, excluding the calibrated one, were directly derived from those at the DHM site. The values of FLNR showed increased tendency from low latitude to high latitude, supporting the vigorous growth of trees during growing season at the CBM site (Li, 2019). Li (2018) found that the parameters of carbon allocation rate of new fine root to new leaf (p10), carbon allocation rate of new stem to new leaf (p11) and vapour pressure deficit for the start of conductance reduction (p36) show high spatial heterogeneity for DBT and EBT. In this study, the above three parameters also varied along the latitude.*" (**See lines 554-560 in the revised manuscript**).

8) To me, it is a problem that error bars are almost entirely missing. What is the uncertainty in model predictions?

Revised.

The model simulation error has been calculated using the model relative biases during model validation and Monte Carlo Monte Carlo test with Latin hypercube sampling, which has been presented in the Text S3 and Figure 2 (**See Text S3 and Figure 2 in the revised supplementary materials and manuscript**).

9) Model development focused on things like allocation and mortality, yet there was no validation of observables related to allocation and mortality. If allocation and mortality are things that are added to the model, the authors should present direct evidence that these schemes are producing acceptable results (for example: comparison of observed and simulated wood growth; comparison of observed and simulated mortality; etc.).

Reponses

For the original model, allocation was considered only for aboveground biomass and belowground biomass without the transportation among different organs or tissues, which is not suitable for trees. Therefore, the new growth module was introduced to perfect the scientific processes of the model. The observed fluxes of carbon and water were used for model validation, but the observed data of allocation and mortality were not available. "*Due to the limited available observations at the three sites, validations* *of allocation and mortality, which were newly introduced key processes, were not available in this study.*" (**See lines 310-312 in the revised manuscript**).

10) English usage throughout the manuscript is problematic. The manuscript needs to be proofread for grammar and style.

Revised.

The English usage has been revised throughout the manuscript.

Technical comments:

1. What is a "humad" (line 174)?

Reponses.

In the model, the "humads" indicates the liable humus, while the "humus" indicates the resistant humus, which is derived from the biogeochemical model of DNDC. The decomposition rates of above two components are different. "*The humads and humus defined in the DNDC indicate the liable humus and resistant humus with different decomposition rates, respectively*." (**See lines 182-183 in the revised manuscript**).

2. How does pH affect the model?

Reponses.

The statement has been added to make it clear. "*In the model, pH short-term variations after urea application for uplands and paddy fields and soil acidification after tea plantation can be simulated.*" (**See lines 154-155 in the revised manuscript**).

3. I think it would be nice to have Table S3 in the main text.

Revised.

The table has been added in the main text as the reviewer suggested. (**See Table 1 in the revised manuscript**).