The submitted article by Wei Zhang et al. present the new module to better describe the thermal dynamics within the hydro-biogeochmical model CNMM-DNDC. The authors set up a catchment scale modelling approach and test it on observed soil temperature, water filled pore space, CH4 and N2O emissions from three alpine ecosystems of the Tibetan Plateau. The authors conclude that their proposed module improves the reliability of all investigated measured criteria. The manuscript is well written and the results prove the benefit of the modified modelling approach.

General Comments

Starting very general, I doubt that a default model setup can be used to determine, whether a model is capable to reproduce a measured criteria or not. As such, the presented simulations of the "original" as well as "modified" model are, to me, just random, given the vast amount of internal model parameters. In my opinion both models would need a prior calibration to the local data set first, before judging whether the model is capable to reproduce the observed criteria or not. Maybe there is a parameter combination in the original mode, which performs much better than the modified version. As such, I would reject this manuscript given this general point. However, I leave this point to the Editor, as I know that it is still common to investigated not calibrated models in biogeochemical sciences. In the hydrological community, it is not. This paper covers both disciplines.

Revised.

The discussion about prior calibration to the local data has been added. "*Compared with the empirical model, one key advantage of the process-oriented models is that the models are independent of the local parameterization (Zhang et al., 2015). In this study, default internal parameter combinations of biogeochemical processes were used for the original and modified models, which have been applied in the catchment simulation in the subtropical region (Zhang et al., 2018b), due to the limit field observations (only one year) for both calibration and validation. The biogeochemical processes were predicted by the first-order and Michaelis-Menten kinetics in the CNMM-DNDC based on some defined parameters of flow fractionation. For instance, there are 17 parameters related with N_2O emission in the module of denitrification (Table S6), which would inevitably increase the uncertainty of simulation. Houska et al. (2017) found that hydro-biogeochemical models can be right for the wrong reasons, such as matching greenhouse gas emissions while failing to simulate soil moisture, which emphasized the importance for simultaneous validations of multi-variables. Thus, simultaneous validations of CH_4 and N_2O fluxes, as well as soil environment variables, were necessary for comprehensive evaluation of the model performance. In addition, the microbial ecology was recently recommended to be integrated into the biogeochemical model using a smaller number of well-defined kinetic parameters, such as MOMOS (Pansu et al., 2010; Treseder et al., 2011). Therefore, the direct control of microbial on biogeochemical processes, such as the stoichiometry of decomposer, is required to be included in the CNMM-DNDC in near future.*" (**See lines 417-430 in the revised manuscript**).

Another general point, which makes the judgement of this manuscript difficult for me is, that neither the model, nor the observation data, nor the model setups are accessible to me. Given the open access policy of BG, I was quite surprised to see that. Under these circumstances and given the not very detailed Materials and Methods section, which cites a lot, but gives very view details, I had to guess a lot. I made these guesses within this review in favor to the authors of this manuscript, assuming scientific correctness, without being able as a reviewer to check. E.g., if the given equations proper implemented in modelling code or if the model was correct set up for the characteristics of the study catchment. I feel a lot of improvement necessary within this manuscript to give the readers the possibility to understand and reproduce the results of this study. This point is not easy to cover given the current stage of the manuscript. I leave also this second general point to the Editor, to decide, whether it is reason to reject the manuscript.

Revised.

The availability of model and observation data has been stated in the Data availability section. The detailed information of model setups has been added in Tables S1−4 and the section of Materials and methods.

The detailed information about the hydrological model of CNMM has been added. "*... into the hydrological framework of the CNMM, which is fully distributed.*" (**See lines 131-132 in the revised manuscript**). "*The soil moisture was calculated based on the mass balance of precipitation, irrigation, evapotranspiration, vertical flow, lateral flow and water from a rising water table. The total water that can be infiltrated during each time step was determined by a defined maximum infiltration rate. Darcy's law was applied for predicting the vertical water flow in the soil profile. A cell-by-cell approach using a kinematic approximation was applied to route the saturated overland and subsurface flow based on DEM. The stream flow was estimated using a cascade of linear channel reservoirs (Wigmosta et al., 1994).*" (**See lines 141-146 in the revised manuscript**).

Detailed statements about the differences of the original and modified model in the soil thermal module, as well as the equation used in the original model (Table S1), have been added. "*In the CNMM-DNDC, the soil temperature was predicted by solving the one-dimensional heat conduction equation with the implicit method of Crank-Nicholson. However, despite the simple parameterization used for the calculation of soil heat capacity and thermal conductivity, the variations of soil temperature induced by the freeze-thaw cycles were also not considered (Table S1 of the online supplementary materials), which inevitably hindered its application in the seasonally frozen regions. In this study, the CNMM-DNDC was modified by replacing the above soil thermal module by a physical based module of Northern Ecosystem Soil Temperature (Zhang et al., 2003; Deng et al., 2014), which can explicitly describe the energy exchange within the soil, the active layer dynamics and the soil thermal regime in the presence of freeze-thaw cycles. ... Therefore, the CNMM-DNDC with and without the above modifications is hereafter referred to as the original and modified model, respectively.*" (**See lines 163-173 in the revised manuscript**).

"*Compared to the original thermal module, the internal heat exchange due to freezing or thawing (S) was included with improved algorithm for thermal conductivity (k). In addition, the one-dimensional heat conduction equation (Eq. 1) was solved by converting it to an explicit form in the modified model (Eqs. 2–4), while was solved with the implicit method in the original models (Table S1).*" (**See lines 192-195 in the revised manuscript**).

The reason for the catchment simulation was added. "*For the target catchment, the soil water dynamics of the alpine ecosystems were determined by the precipitation, evapotranspiration, infiltration, penetration and lateral flow. Using the database, a catchment simulation of hydro-biogeochemical processes was performed with spatial and temporal resolutions of 30×30 m^2 and 3 hours, respectively, by the modified CNMM-DNDC from 2012 to 2015, which could reflect the influences of hydrological processes on soil water dynamics. Thus, the soil water dynamics of the seasonally inundated wetlands were determined by the hydrological processes without any artificial disturbances in the catchment simulation.*" (**See lines 236-241 in the revised manuscript**).

The detailed the calculation steps of soil heat capacity (C_l) and thermal conductivity (k_l), as well as the related contents about the scenario simulation of annually inundated (A-wetland) wetland have been added. See the responses below, please.

My further comments can be seen as major revision, which will hopefully help the authors to improve their manuscript, either for future publication in this journal or after potential rejection in another journal.

Specific comments

Table 1: Please specify "n" in Table caption. Please add a column for data resolution. Regarding CH4 an N2O fluxes, Tables say its daily, while Text (Line 167) says its weekly. Further, statistical indices (IA, NSI, Slope, R^2 and P) are not in line with chapter 2.4 (IA, NSE, R^2, ZIR and MRB). Also, give please give the full name of the statistical indices in the Table caption. Use NSE (Nash-Sutcliffe efficiency) instead of NSI throughout the manuscript. How is it possible that there are no values for R^2 and P, if you could calculate IA, NSI and Slope? Please give the equations for ZIR and MRB in chapter 2.4, as there are not so commonly used.

Revised.

Table 1 has been revised as the reviewer suggested by adding detailed notes. The equation applied for the zero-intercept univariate linear regression (ZIR) of the observations against the simulations has been added in Table S5. The no values for R^2 were due to the sum of regression square are larger than the sum of the total square for the zero-intercept univariate linear regression, which has been added in the note of Table 1. (**See Table 1 in the revised manuscript**).

Figure 1: In general, a good simulation of the observed soil temperature is nothing special in the state of the art environmental models. The "original" model just seems to

be wrong, so it is not a big challenge to improve that. However, still a necessary task, which shows the importance of this work. What I am missing in the methods chapter, is a clear description of the differences between the "modified" and the "original" model. Maybe a figure would help, showing the setup of both models and highlighting the differences.

Revised.

Detailed statements about the differences of the original and modified model in the soil thermal module, as well as the equation used in the original model (Table S1), have been added to make it clear. "*In the CNMM-DNDC, the soil temperature was predicted by solving the one-dimensional heat conduction equation with the implicit method of Crank-Nicholson. However, despite the simple parameterization used for the calculation of soil heat capacity and thermal conductivity, the variations of soil temperature induced by the freeze-thaw cycles were also not considered (Table S1 of the online supplementary materials), which inevitably hindered its application in the seasonally frozen regions. In this study, the CNMM-DNDC was modified by replacing the above soil thermal module by a physical based module of Northern Ecosystem Soil Temperature (Zhang et al., 2003; Deng et al., 2014), which can explicitly describe the energy exchange within the soil, the active layer dynamics and the soil thermal regime in the presence of freeze-thaw cycles. ... Therefore, the CNMM-DNDC with and without the above modifications is hereafter referred to as the original and modified model, respectively.*" (**See lines 163-173 in the revised manuscript**). "*Compared to the original thermal module, the internal heat exchange due to freezing or thawing (S) was included with improved algorithm for thermal conductivity (k). In addition, the one-dimensional heat conduction equation (Eq. 1) was solved by converting it to an explicit form in the modified model (Eqs. 2–4), while was solved with the implicit method in the original models (Table S1).*" (**See lines 192-195 in the revised manuscript**).

Line 221-224: This statement is way to bold. Firstly, you tested the model in one region only. Secondly, the models best achieved NSI value is 0.32. That's very far from a "reliable" prediction (please add some citations for comparison in the Discussion chapter, there are tons, e.g. Ford et al., 2014). Thirdly, you tested the model on top soil WFPS only. How can you generalize from there to "reliable water movement" in general? Please delete this passage and be more accurate throughout the manuscript.

Revised.

More discussions have been added in relation with the simulated soil temperature and moisture with references. "*In order to quantify the impacts of climate change on the cycling of carbon and water on the regional and global scales, several large scale ecosystem models or macroscale hydrological models, such as Terrestrial Ecosystem Model, Lund-Potsdam-Jena dynamic global vegetation model and Variable Infiltration Capacity model, have been enhanced to simulate the soil thermal dynamics at northern high latitude (Wania et al., 2009; Zhuang et al., 2001; Cuo et al., 2015; Jiang et al., 2020). In addition, the soil thermal modules were also improved in some biogeochemical models, such as DNDC and Mobile-DNDC, to*

evaluate the influences of climate warming on the biogeochemical processes in high latitude regions (Zhang et al., 2003; de Bruijn et al., 2009; Wolf et al., 2011; Zhang et al., 2012; Deng et al., 2014). Compared with the simulated soil profile temperatures by above models at different scales, the simulations in this study by the modified CNMM-DNDC were equally well, especially for deeper soil layers (e.g., Wania et al., 2009). For the validated topsoil moisture in this study, the modified model generally captured the variation trends, which were comparable with the performances of other models (e.g., de Bruijn et al., 2009; Wolf et al., 2011; Cuo et al., 2015). However, compared with the studies focused on simulating soil moisture (e.g., Ford et al., 2014), further improvements are still required to improve the model performance in simulating the soil moisture." (**See lines 336-348 in the revised manuscript**).

The statements have been revised throughout the manuscript. "*The performances of the modified model in simulating the soil profile temperature and topsoil moisture indicate that the modified CNMM-DNDC can generally predict the soil thermal and topsoil moisture dynamics in the three alpine ecosystems, which is crucial for correctly simulating soil hydrology, plant growth and biogeochemical processes.*" (**See lines 275-278 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC well simulated the CH_4 fluxes of the three typical alpine ecosystems.*" (**See lines 291-292 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC has the potential to estimate N_2O emissions in seasonally frozen regions.*" (**See lines 306-307 in the revised manuscript**). "*These results indicate the efficiency of the incorporated module in simulating soil thermal and topsoil moisture dynamics in seasonally frozen regions.*" (**See lines 348-350 in the revised manuscript**). "*The model performances of simulating various variables for three typical alpine ecosystems in the Rierlangshan catchment imply that the modified CNMM-DNDC can be applied to predict the thermal dynamics and fluxes of CH_4 and N_2O from alpine ecosystems in seasonally frozen regions.*" (**See lines 430-433 in the revised manuscript**). "*This study implies that a hydro-biogeochemical model, such as the modified CNMM-DNDC, are able to predict soil thermal dynamics, topsoil moisture and fluxes of CH_4 and N_2O in seasonally frozen regions with an improved physical-based soil thermal module.*" (**See lines 475-477 in the revised manuscript**).

Line 235-237: I do not understand how the results show that the model "simulated the CH4 fluxes [..] at the catchment scale". The set up might be fully distributed (which is a guess by me and not very clear stated in the Material and Methods section). However, you test the model on local scale measurements. So, you can only state that the model is able to reproduce the local measurements with the given statistical accuracy. Please be consistent with this comment throughout the manuscript. Further, I do not understand why a fully-distributed model set-up is needed herein to test the local measurements as you do not have any spatial measurements. If it is needed, it needs some justification within the manuscript and some results on the spatial scale, e.g. a map of the N2O and CH4 emissions.

Revised.

The detailed information about the hydrological model of CNMM has been added. "...

into the hydrological framework of the CNMM, which is fully distributed." (**See lines 131-132 in the revised manuscript**). "*The soil moisture was calculated based on the mass balance of precipitation, irrigation, evapotranspiration, vertical flow, lateral flow and water from a rising water table. The total water that can be infiltrated during each time step was determined by a defined maximum infiltration rate. Darcy's law was applied for predicting the vertical water flow in the soil profile. A cell-by-cell approach using a kinematic approximation was applied to route the saturated overland and subsurface flow based on DEM. The stream flow was estimated using a cascade of linear channel reservoirs (Wigmosta et al., 1994).*" (**See lines 141-146 in the revised manuscript**).

The reason for the catchment simulation, as well as the simulation results, was added. "*For the target catchment, the soil water dynamics of the alpine ecosystems were determined by the precipitation, evapotranspiration, infiltration, penetration and lateral flow. Using the database, a catchment simulation of hydro-biogeochemical processes was performed with spatial and temporal resolutions of 30×30 m^2 and 3 hours, respectively, by the modified CNMM-DNDC from 2012 to 2015, which could reflect the influences of hydrological processes on soil water dynamics. Thus, the soil water dynamics of the seasonally inundated wetlands were determined by the hydrological processes without any artificial disturbances in the catchment simulation.*" (**See lines 236-241 in the revised manuscript**). "*For the catchment simulation, the simulated annual CH_4 emissions ranged from -2.35 to 73.0 kg C ha^{-1} yr^{-1} from November 2013 to November 2014 (Fig. S4a).*" (**See lines 289-291 in the revised manuscript**). "*For the catchment simulation, the simulated annual N_2O emissions ranged from 0.01 to 0.74 kg N ha^{-1} yr^{-1} from November 2013 to November 2014 (Fig. S4b).*" (**See lines 305-306 in the revised manuscript**).

The statements have been revised throughout the manuscript. "*The performances of the modified model in simulating the soil profile temperature and topsoil moisture indicate that the modified CNMM-DNDC can generally predict the soil thermal and topsoil moisture dynamics in the three alpine ecosystems, which is crucial for correctly simulating soil hydrology, plant growth and biogeochemical processes.*" (**See lines 275-278 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC well simulated the CH_4 fluxes of the three typical alpine ecosystems.*" (**See lines 291-292 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC has the potential to estimate N_2O emissions in seasonally frozen regions.*" (**See lines 306-307 in the revised manuscript**). "*These results indicate the efficiency of the incorporated module in simulating soil thermal and topsoil moisture dynamics in seasonally frozen regions.*" (**See lines 348-350 in the revised manuscript**). "*The model performances of simulating various variables for three typical alpine ecosystems in the Rierlangshan catchment imply that the modified CNMM-DNDC can be applied to predict the thermal dynamics and fluxes of CH_4 and N_2O from alpine ecosystems in seasonally frozen regions.*" (**See lines 430-433 in the revised manuscript**). "*This study implies that a hydro-biogeochemical model, such as the modified CNMM-DNDC, are able to predict soil thermal dynamics, topsoil moisture and fluxes of CH_4 and N_2O in seasonally frozen regions with an improved*

physical-based soil thermal module." (**See lines 475-477 in the revised manuscript**).

Figure 3: From the differences in the WFPS and soil temperature, I do not see any reason why the original model would produce such a high N2O peak and the modified model (assuming that the described soil temperature routine was the only thing changed, which is not 100% clear in chapter 2.1.2) does not explain such a vast difference in either denitrification and nitrification processes. This needs some more detailed discussion in chapter 4.3, also by showing the different model internal processes of nitrification and denitrification of the two model set ups explaining the difference.

Revised.

Detailed statements about the differences of the original and modified model in the soil thermal module, as well as the equation used in the original model (Table S1), have been added. "*In the CNMM-DNDC, the soil temperature was predicted by solving the one-dimensional heat conduction equation with the implicit method of Crank-Nicholson. However, despite the simple parameterization used for the calculation of soil heat capacity and thermal conductivity, the variations of soil temperature induced by the freeze-thaw cycles were also not considered (Table S1 of the online supplementary materials), which inevitably hindered its application in the seasonally frozen regions. In this study, the CNMM-DNDC was modified by replacing the above soil thermal module by a physical based module of Northern Ecosystem Soil Temperature (Zhang et al., 2003; Deng et al., 2014), which can explicitly describe the energy exchange within the soil, the active layer dynamics and the soil thermal regime in the presence of freeze-thaw cycles. ... Therefore, the CNMM-DNDC with and without the above modifications is hereafter referred to as the original and modified model, respectively.*" (**See lines 163-173 in the revised manuscript**). "*Compared to the original thermal module, the internal heat exchange due to freezing or thawing (S) was included with improved algorithm for thermal conductivity (k). In addition, the one-dimensional heat conduction equation (Eq. 1) was solved by converting it to an explicit form in the modified model (Eqs. 2–4), while was solved with the implicit method in the original models (Table S1).*" (**See lines 192-195 in the revised manuscript**).

The detailed information of model setup parameters has been added in Table S3. (**See Table S3**).

The discussion about the intensive N_2O emissions has been added. "*The intensive N_2O emissions simulated by the original model resulted from the overestimated soil temperature for the alpine wetlands. Firstly, as the presence of ice could impede the water movement, the water lateral flows were promoted by the original model due to the neglecting of freeze-thaw cycles. These further resulted in the lower simulated soil moisture as compared with the modified model (Fig. S5), which provided favorable oxygen conditions for N_2O production. Meanwhile, the simulated high soil moisture by the modified model provided feasible anaerobic conditions for thoroughly denitrification. Secondly, higher simulated soil temperature by the original model also facilitated the mineralization, which provided more available mineral nitrogen.*" (**See**

lines 387-394 in the revised manuscript).

Line 344-347: Again a very bold statement, speaking of "hydrology" when testing a model with WFPS measured at the upper 6 cm of the soil (what about evaporation, overland flow, infiltration, groundwater recharge,…); speaking of "nitrogen cycling" when testing against N2O emission, which is only 1% of the total N emissions (what about N2, NH3, NO, the nitrogen stored in the soil,…), speaking of "carbon cycling" while not looking at CO2 emissions or the changing carbon storage. Please rephrase and stick to the investigated processes throughout the manuscript.

Revised.

The statements have been revised throughout the manuscript. "*The performances of the modified model in simulating the soil profile temperature and topsoil moisture indicate that the modified CNMM-DNDC can generally predict the soil thermal and topsoil moisture dynamics in the three alpine ecosystems, which is crucial for correctly simulating soil hydrology, plant growth and biogeochemical processes.*" (**See lines 275-278 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC well simulated the CH_4 fluxes of the three typical alpine ecosystems.*" (**See lines 291-292 in the revised manuscript**). "*These results indicate that the modified CNMM-DNDC has the potential to estimate N_2O emissions in seasonally frozen regions.*" (**See lines 306-307 in the revised manuscript**). "*These results indicate the efficiency of the incorporated module in simulating soil thermal and topsoil moisture dynamics in seasonally frozen regions.*" (**See lines 348-350 in the revised manuscript**). "*The model performances of simulating various variables for three typical alpine ecosystems in the Rierlangshan catchment imply that the modified CNMM-DNDC can be applied to predict the thermal dynamics and fluxes of CH_4 and N_2O from alpine ecosystems in seasonally frozen regions.*" (**See lines 430-433 in the revised manuscript**). "*This study implies that a hydro-biogeochemical model, such as the modified CNMM-DNDC, are able to predict soil thermal dynamics, topsoil moisture and fluxes of CH_4 and N_2O in seasonally frozen regions with an improved physical-based soil thermal module.*" (**See lines 475-477 in the revised manuscript**).

Section 4.4: Interesting section, however, atm out of the scope of the manuscript (which implies so far the testing of a changed module in a model). If this section is supposed to be included in the manuscript, please extend, title, abstract, methods section and add a figure are table to visualize this discussion. I would recommend to delete.

Revised.

The related contents about the scenario simulation of annually inundated wetland have been added in the sections of Abstract, Introduction, Materials and methods, Results, Discussions, Conclusions and Fig. 4. If the reviewer still suggests to deleting this part, we would delete this in the next version.

Abstract

"*Consistent with the emissions determined from the field observations, the simulated aggregate emissions of CH_4 and N_2O were significantly reduced due to wetland degradation and were dominated by a reduction in CH_4 emissions.*" (**See lines 29-31**

in the revised manuscript).

Introduction

"*Nearly one-third of the wetlands in China are located in the Tibetan Plateau and more than 60% of those were fens (Gong et al., 2010; Wei et al., 2015). Due to intentional drainage for pasture enlargement during the past decades, more than 90% of the fens have degraded and turned to seasonally inundated or wet meadows (Wei et al., 2015). This unprecedented large-scale land use change inevitably influences the exchange of CH_4 and N_2O fluxes at the soil-atmosphere boundary (e.g., Yao et al., 2019; Zhang et al., 2019; Zhang et al., 2020). Zhang et al. (2019) found that intentional drainage has resulted in great reduction of CH4 emissions. However, the nitrogen mineralization with falling water table for the degraded wetlands may increase the production N_2O (e.g., Hatano, 2019; Tan et al., 2020; Zhang et al., 2020). Therefore, we hypothesize that the CNMM-DNDC with improvements in soil thermal dynamics would have the potential to quantify the impacts of wetland degradation on CH_4 and N_2O fluxes.*" (**See lines 102-110 in the revised manuscript**).

"*The aims of this study were to ... (iii) quantifying the effects of wetland degradation on CH_4 and N_2O fluxes using the validated model.*" (**See line 117 in the revised manuscript**).

Materials and methods

"*In order to quantify the effects of wetland degradation on CH_4 and N_2O fluxes by the validated model, a scenario simulation for the annually inundated wetland was performed at the catchment scale through arbitrarily setting the minimal water table of the validated alpine wetland as 5 cm.*" (**See lines 242-244 in the revised manuscript**).

Results

"*For the scenario simulation of annually inundated wetlands, the simulated aggregate emissions of CH_4 and N_2O were 7.2 Mg CO_2eq ha^{-1} yr^{-1} with CH_4 and N_2O emissions of 158 kg C ha^{-1} yr^{-1} and 0.0 kg N ha^{-1} yr^{-1}, respectively (Fig. 4). The simulation indicates that wetland degradation resulted in decreased CH_4 emissions but increased N_2O emissions. However, the increased N_2O emissions could be totally offset by the reduced CH_4 emissions, thus finally leading to the decreased aggregate emissions of CH_4 and N_2O from degraded wetlands than permanently inundated wetlands but still much higher than those of adjacent wet alpine meadows.*" (**See lines 318-323 in the revised manuscript**).

Discussions

"*Zhang et al. (2019) found that annual CH_4 emissions from permanently inundated wetlands were 7.1 times higher than those from degraded wetlands with seasonal inundation. Assuming N_2O emissions were zero for permanently inundated wetlands (e.g., Kolb and Horn, 2012; Hatano, 2019), the aggregate emissions of CH_4 and N_2O were estimated at 10.8 Mg CO_2eq ha^{-1} yr^{-1} for natural alpine wetlands based on observations used for model validation in this study, which were higher than the scenario simulation. Both the observations and simulations showed that in comparison to annually inundated wetlands, wetland degradation stimulated N_2O emissions to a small extent but reduced CH_4 emissions to a large extent. Thus,*

compared to that from natural wetlands, the aggregate emissions of CH_4 and N_2O from degraded wetlands were largely reduced but still higher than those of adjacent wet alpine meadows. These results were consistent with the field observations of CH_4 and N_2O emissions along different water table transects in the Zoige peatland, which were primarily driven by soil water content and SOC (Zhang et al., 2020)." (**See lines 438-447 in the revised manuscript**).

Conclusions

"*Both the observed and simulated CH_4 and N_2O fluxes from alpine wetlands and meadows, as well as the results from the simulated annually inundated wetlands, indicate that wetland degradation resulted in a significant reduction in the aggregate emissions of CH_4 and N_2O.*" (**See lines 472-474 in the revised manuscript**).

Please add a comparison of the model results with other studies investigating wetlands, meadows, forest. Also a comparison to different models, which implemented and tested thermal dynamics in their models. And what about other studies investigating freeze thaw cyclings with hydro-biogeochemical models, e.g. deBruijn et al (2009). I am surprised not to see many of the relevant literature within the discussion, please add.

Revised.

More discussions have been added in Discussion section with references. "*For the studies focused on simulating CH_4 emissions from wetlands by the large-scale ecosystem models, the model validation with field observation is difficult due to coarse spatial resolution (e.g., Zhuang et al., 2004). For the biogeochemical model, such as DNDC, the dynamics of CH_4 emissions from wetland and peatland in the northern permafrost regions were well simulated (Zhang et al., 2012; Deng et al., 2014), which showed consistent seasonal variations and magnitudes as those in this study.*" (**See lines 365-369 in the revised manuscript**). "*Field observations showed that the soil freeze-thaw cycles occurred in seasonally frozen regions not only increased the availability of nitrogen and carbon substrates by disrupting of soil aggregates but also affected the structure, population and activity of the microbes, and thus influencing the emissions of N_2O (e.g., Song et al., 2019). de Bruijn et al. (2009) have explored the combined mechanisms for simulating freeze–thaw related N_2O emissions, which were the promoted anaerobiosis and denitrification due to reduced gas diffusion derived from soil frost and snow cover, and the stimulated microbial growth due to easy decomposable organic carbon and nitrogen derived from the dead microbes during freeze-thaw cycles. Wolf et al. (2011) introduced an impedance factor to parameterize the reduced water flow between layers in presence of ice, which could captured the freeze–thaw related N_2O emissions for ungrazed steppe. In the CNMM-DNDC, threshold values of soil temperature were set to trigger the death of microbes during the freezing period and stimulate the production of NO, N_2O and N_2 using substrates derived from the dead microbes during the thawing period, which was similar to one of the mechanisms explored by de Bruijn et al. (2009). However, compared with the simulated freeze–thaw related N_2O emissions by other studies, the simulated dynamics of peak emissions due to freeze-thaw cycles in this study were inconsistent with those from the field observations. Thus,*

improvements are required to incorporate some other effective mechanisms to better capture the dynamic characteristics." (**See lines 399-412 in the revised manuscript**).

Technical corrections

Line 22: Change "as" to "is"
Revised.
The sentence has been revised as the reviewer suggested. "*This study is one of many efforts to fulfil such an expectation, which was performed to improve the CNMM-DNDC by incorporating a physical-based soil thermal module to simulate the soil thermal regime in the presence of freeze-thaw cycles.*" (**See lines 21-23 in the revised manuscript**).

Line 34: I don't understand how the model can be used to "evaluate the sustainability". Please rephrase.
Revised.
The sentence has been rewritten to make it clear. "*As the original CNMM-DNDC was previously validated in some unfrozen regions, the modified CNMM-DNDC could be potentially applied to estimate the emissions of CH_4 and N_2O from various ecosystems under different climate zones at the site or catchment scale.*" (**See lines 32-35 in the revised manuscript**).

Line 44: Expression "during long periods" please be more accurate and give some numbers.
Revised.
The sentence has been revised. "*...the soil organic carbon stored since the Last Glacial Maximum has been lost to the atmosphere via methane...*" (**See lines 45-46 in the revised manuscript**).

Line 125: Needs a reference to the concept.
Revised.
The reference has been added. "*The "hole in the pipe" concept was applied to calculate N_2O production during nitrification, which is influenced by the soil moisture, temperature and pH (Li, 2016).*" (**See lines 152-153 in the revised manuscript**).

Line 134-136: Please simplify structure of the sentence
Revised.
The sentences have been rewritten. "*In the CNMM-DNDC, the soil temperature was predicted by solving the one-dimensional heat conduction equation with the implicit method of Crank-Nicholson. However, despite the simple parameterization used for the calculation of soil heat capacity and thermal conductivity, the variations of soil temperature induced by the freeze-thaw cycles were also not considered (Table S1 of the online supplementary materials), which inevitably hindered its application in the*

seasonally frozen regions. In this study, the CNMM-DNDC was modified by replacing the above soil thermal module by a physical based module of Northern Ecosystem Soil Temperature (Zhang et al., 2003; Deng et al., 2014), which can explicitly describe the energy exchange within the soil, the active layer dynamics and the soil thermal regime in the presence of freeze-thaw cycles." (**See lines 163-169 in the revised manuscript**).

Line 134-140: It remains unclear to me, whether these changes are already done in the cited publications, or if that's the new part.

Revised.

This part has been rewritten to make it clear. "*In the CNMM-DNDC, the soil temperature was predicted by solving the one-dimensional heat conduction equation with the implicit method of Crank-Nicholson. However, despite the simple parameterization used for the calculation of soil heat capacity and thermal conductivity, the variations of soil temperature induced by the freeze-thaw cycles were also not considered (Table S1 of the online supplementary materials), which inevitably hindered its application in the seasonally frozen regions. In this study, the CNMM-DNDC was modified by replacing the above soil thermal module by a physical based module of Northern Ecosystem Soil Temperature (Zhang et al., 2003; Deng et al., 2014), which can explicitly describe the energy exchange within the soil, the active layer dynamics and the soil thermal regime in the presence of freeze-thaw cycles. ... Therefore, the CNMM-DNDC with and without the above modifications is hereafter referred to as the original and modified model, respectively.*" (**See lines 163-173 in the revised manuscript**).

Line 149: What are these numbers behind organic matter, mineral, water, ice and air? I have to assume they are dynamic for each time step.

Revised.

These sentences have been revised. "*The dynamic soil heat capacity (C, J m^{-3} ℃$^{-1}$) is the weighted average of the heat capacity for five constituents, including organic matter ($C_{l,\,OM}$), minerals ($C_{l,Min}$), water ($C_{l,\,Water}$), ice ($C_{l,\,Ice}$) and air ($C_{l,\,Air}$) (Eq. 5). The values of heat capacity for organic matter, minerals, water, ice and air were 2.5×10^6, 2.0×10^6, 4.2×10^6, 2.1×10^6 and 1.2×10^3 J m^{-3} ℃$^{-1}$, respectively (Huang, 2000).*" (**See lines 181-184 in the revised manuscript**).

Equations 1-6: Why not add an example with a given organic matter, minerals, water ice and air values.

Revised.

The detailed the calculation steps of soil heat capacity (C_l) and thermal conductivity (k_l) have been added as the reviewer suggested. (**See Eq. 5–13 in the revised manuscript**).

Line 153: Why in 35 m? Is that a constant? Or the depth to which the model can be applied to?

Revised.

The sentences have been revised to make it clear. "*The simulated soil depth (including bedrock) is user-defined.*" (**See line 140 in the revised manuscript**). "*The simulated soil depth was defined as 35 m due to the lower boundary conditions of the thermal dynamics, which was set as the geothermal heat flux at a soil depth of 35 m. The simulated soil profile (0−35 m depth) was divided into 23 layers, including the soil (0−1.5m) and bedrock (1.5−35m).*" (**See lines 230-233 in the revised manuscript**).

Line 175: Details about the instruments used for the soil temperature and WFPS measurements are relevant within this manuscript, please add.

Revised.

The detailed instruments used for field measurements have been added in the Table S2 of the online supplementary materials. (**See Table S2 in the revised manuscript**).

Line 188: Which pedo-transfer function?

Revised.

The applied pedo-transfer functions were detailed in the Table S4 of the online supplementary materials. (**See Table S4 in the revised manuscript**).

Line 192: Why was the model run in 3 hours resolution if the metrological data input is hourly available?

Revised.

The time resolution is determined by both the model and meteorological data, which were at hour scale during field observation, but daily scale at the other period. "*The temporal and spatial resolutions are also user-defined according to the driving data of climate (generally in 3 hours) and digital elevation model (DEM).*" (**See lines 140-141 in the revised manuscript**). "*a climate dataset of 3-hour weather data (air temperature, precipitation, wind speed, solar radiation, longwave radiation, and humidity), which were obtained from the meteorological station in the target catchment for the years with field observations (2013.11–2015.10) and were adapted from the daily data at the Zoige Meteorological Station (provided by the National Meteorological Information Center: http://data.cma.cn/; last access: 10th June, 2020) for other years;*" (**See lines 222-226 in the revised manuscript**). "*Using the database, a catchment simulation of hydro-biogeochemical processes was performed with spatial and temporal resolutions of 30×30 m^2 and 3 hours, respectively, by the modified CNMM-DNDC from 2012 to 2015...*" (**See lines 237-239 in the revised manuscript**).

Figure 2: Please use same y-axis range throught subplots. It is odd to have WFPS up to 1.2. Please use range from 0-1.

Revised.

The figure has been revised as the reviewer suggested. (**See Fig. 2 in the revised manuscript**).

Figure 1-5: It would be way easier to interpret if there would be one figure for each land use with all the fluxes. Maybe even only one figure including all fluxes.

Revised.

The figures have been adjusted to make the fluxes of CH_4 and N_2O from all alpine ecosystems in one figure. (**See Fig. 3 in the revised manuscript**).

Line250-252: Again, a too broad generalization from one model run and one study area. Please stick with the expression to the investigated processes, e.g. "These results indicate that the modified CNMM-DNDC has the potential to estimate N2O emissions in a seasonally frozen region."

Revised.

The sentence has been revised as the reviewer suggested. "*These results indicate that the modified CNMM-DNDC has the potential to estimate N_2O emissions in a seasonally frozen region.*" (**See lines 306-307 in the revised manuscript**).

Figure 6: Why is there now a second wetland (A-wetland) which hasn't been shown before? Please stay consistent. Again it would be easier to group the study areas, as the results here are hard to read. Maybe a table would help to.

Revised.

The related contents about the scenario simulation of annually inundated wetland have been added in the sections of Abstract, Introduction, Materials and methods, Results, Discussions, Conclusions and Fig. 4. If the reviewer still suggests to deleting this part, we would delete this in the next version.

Abstract

"*Consistent with the emissions determined from the field observations, the simulated aggregate emissions of CH_4 and N_2O were significantly reduced due to wetland degradation and were dominated by a reduction in CH_4 emissions.*" (**See lines 29-31 in the revised manuscript**).

Introduction

"*Nearly one-third of the wetlands in China are located in the Tibetan Plateau and more than 60% of those were fens (Gong et al., 2010; Wei et al., 2015). Due to intentional drainage for pasture enlargement during the past decades, more than 90% of the fens have degraded and turned to seasonally inundated or wet meadows (Wei et al., 2015). This unprecedented large-scale land use change inevitably influences the exchange of CH_4 and N_2O fluxes at the soil-atmosphere boundary (e.g., Yao et al., 2019; Zhang et al., 2019; Zhang et al., 2020). Zhang et al. (2019) found that intentional drainage has resulted in great reduction of CH4 emissions. However, the nitrogen mineralization with falling water table for the degraded wetlands may increase the production N_2O (e.g., Hatano, 2019; Tan et al., 2020; Zhang et al., 2020). Therefore, we hypothesize that the CNMM-DNDC with improvements in soil thermal dynamics would have the potential to quantify the impacts of wetland degradation on CH_4 and N_2O fluxes.*" (**See lines 102-110 in the revised manuscript**).

"*The aims of this study were to ... (iii) quantifying the effects of wetland degradation on CH_4 and N_2O fluxes using the validated model.*" (**See line 117 in the revised**

manuscript).

Materials and methods

"*In order to quantify the effects of wetland degradation on CH_4 and N_2O fluxes by the validated model, a scenario simulation for the annually inundated wetland was performed at the catchment scale through arbitrarily setting the minimal water table of the validated alpine wetland as 5 cm.*" (**See lines 242-244 in the revised manuscript**).

Results

"*For the scenario simulation of annually inundated wetlands, the simulated aggregate emissions of CH_4 and N_2O were 7.2 Mg CO_2eq ha^{-1} yr^{-1} with CH_4 and N_2O emissions of 158 kg C ha^{-1} yr^{-1} and 0.0 kg N ha^{-1} yr^{-1}, respectively (Fig. 4). The simulation indicates that wetland degradation resulted in decreased CH_4 emissions but increased N_2O emissions. However, the increased N_2O emissions could be totally offset by the reduced CH_4 emissions, thus finally leading to the decreased aggregate emissions of CH_4 and N_2O from degraded wetlands than permanently inundated wetlands but still much higher than those of adjacent wet alpine meadows.*" (**See lines 318-323 in the revised manuscript**).

Discussions

"*Zhang et al. (2019) found that annual CH_4 emissions from permanently inundated wetlands were 7.1 times higher than those from degraded wetlands with seasonal inundation. Assuming N_2O emissions were zero for permanently inundated wetlands (e.g., Kolb and Horn, 2012; Hatano, 2019), the aggregate emissions of CH_4 and N_2O were estimated at 10.8 Mg CO_2eq ha^{-1} yr^{-1} for natural alpine wetlands based on observations used for model validation in this study, which were higher than the scenario simulation. Both the observations and simulations showed that in comparison to annually inundated wetlands, wetland degradation stimulated N_2O emissions to a small extent but reduced CH_4 emissions to a large extent. Thus, compared to that from natural wetlands, the aggregate emissions of CH_4 and N_2O from degraded wetlands were largely reduced but still higher than those of adjacent wet alpine meadows. These results were consistent with the field observations of CH_4 and N_2O emissions along different water table transects in the Zoige peatland, which were primarily driven by soil water content and SOC (Zhang et al., 2020).*" (**See lines 438-447 in the revised manuscript**).

Conclusions

"*Both the observed and simulated CH_4 and N_2O fluxes from alpine wetlands and meadows, as well as the results from the simulated annually inundated wetlands, indicate that wetland degradation resulted in a significant reduction in the aggregate emissions of CH_4 and N_2O.*" (**See lines 472-474 in the revised manuscript**).

Line 262-272: Please move to the discussion chapter. Also, I see what the authors want to express here, however, investigating the climate impact from different landuse is not expressed as scope of the manuscript.

Revised.

The part has been moved to discussion part. The related contents about the scenario

simulation of annually inundated wetland have been added in the sections of Abstract, Introduction, Materials and methods, Results, Discussions, Conclusions and Fig. 4. See the response of the last comment, please.

Line 275-278: I would have liked to see some more insight into the model internal processes and differences here. E.g. a picture showing the different soil layers in the models and compare the WFPS, maybe similar to Figure 12 in Haas et al (2013) or Figure 7 in Klatt et al (2017).

Revised.

The Fig. S5 has been added to show the simulated soil profile moisture of the alpine meadow by the original and modified models, which resulted in the different simulation of N_2O emissions. "*The intensive N_2O emissions simulated by the original model resulted from the overestimated soil temperature for the alpine wetlands. Firstly, as the presence of ice could impede the water movement, the water lateral flows were promoted by the original model due to the neglecting of freeze-thaw cycles. These further resulted in the lower simulated soil moisture as compared with the modified model (Fig. S5), which provided favorable oxygen conditions for N_2O production. Meanwhile, the simulated high soil moisture by the modified model provided feasible anaerobic conditions for thoroughly denitrification. Secondly, higher simulated soil temperature by the original model also facilitated the mineralization, which provided more available mineral nitrogen.*" (**See lines 387-394 in the revised manuscript**).

Line 304: Needs a reference where global warming effect on CH4 emissions where investigated.

Revised.

The reference of a review has been added. "*This result implies that global warming may trigger intensive CH_4 emissions from degraded wetlands, which could partly serve as a trade-off for the decreased CH_4 emissions due to the lower water table level in degraded wetlands (Gong et al., 2020).*" (**See lines 363-365 in the revised manuscript**).

Line 305-306: To be able to understand this sentence, this manuscript needs a table showing the relevant model setup up parameters (meteorology, soil, management, vegetation). Maybe something similar as Table 1 in Houska et al (2017).

Revised.

The detailed information of model setup parameters has been added in Table S3. (**See Table S3 in the revised manuscript**).

Line 311: How was the influence of the clay fraction in the CH4 uptake investigated? Interesting point, but this statement comes out of the blue, as it was not part of the Methods and the Results sections.

Revised.

The reason of high CH_4 uptake in the forest has been rewritten to make it accurate.

"Both observations and simulations showed that the CH_4 uptake in alpine forests was higher than that in alpine meadows, which was mainly attributed to the high SOC content of the alpine forests in the simulation. Methane uptake by upland soils is a biological process governed by the availability of CH_4 and oxygen as well as the activity and quantity of methanotrophic bacteria in soils (e.g., Liu et al., 2007; Zhang et al., 2014). In the model, the simulated CH_4 uptake was positively related to the SOC content, which is closely related to the population size of methanotrophic bacteria. Thus, the SOC content primarily contributed to the differences in CH_4 uptake from alpine meadows and forests, as the values for forests were more than twice of those for meadows (Table S3)." (**See lines 369-375 in the revised manuscript**).

Line 328: How did the authors achieve and control an inundation in the model?

Revised.

The detailed information about inundation has been added. *"For the target catchment, the soil water dynamics of the alpine ecosystems were determined by the precipitation, evapotranspiration, infiltration, penetration and lateral flow. Using the database, a catchment simulation of hydro-biogeochemical processes was performed with spatial and temporal resolutions of 30×30 m^2 and 3 hours, respectively, by the modified CNMM-DNDC from 2012 to 2015, which could reflect the influences of hydrological processes on soil water dynamics. Thus, the soil water dynamics of the seasonally inundated wetlands were determined by the hydrological processes without any artificial disturbances in the catchment simulation. In order to quantify the effects of wetland degradation on CH_4 and N_2O fluxes by the validated model, a scenario simulation for the annually inundated wetland was performed at the catchment scale through arbitrarily setting the minimal water table of the validated alpine wetland as 5 cm."* (**See lines 236-244 in the revised manuscript**).

Line 335-336: I do not understand this sentence. Is the process of "disruption of soil aggregates" as well as the "structure, population and activity of the microbes" really incuded in CNMM-DNDC. The materials and methods section is missing a description of the relevant included process. And I assume, these processes are not included, so please rephrase.

Revised.

The sentence has been revised to make it clear, which aimed at explaining the mechanism of peak N_2O emissions during freeze-thaw cycles. *"Field observations showed that the soil freeze-thaw cycles occurred in seasonally frozen regions not only increased the availability of nitrogen and carbon substrates by disrupting of soil aggregates but also affected the structure, population and activity of the microbes, and thus influencing the emissions of N_2O (e.g., Song et al., 2019)."* (**See lines 399-402 in the revised manuscript**).

Line 344: Where is the "detection limit" of the used N2O measurement technique?

Revised.
The detection limit of N_2O measurement has been added. "*...which resulted from both measurement errors due to low fluxes around detection limits (± 0.41 g N ha^{-1} d^{-1}) and...*" (**See lines 415-416 in the revised manuscript**).

Line 387: Change to " implies that a hydro-biogeochemical model"
Revised.
The sentence has been revised. "*This study implies that a hydro-biogeochemical model, such as the modified CNMM-DNDC, are able to predict soil thermal dynamics, topsoil moisture and fluxes of CH_4 and N_2O in seasonally frozen regions with an improved physical-based soil thermal module.*" (**See lines 475-477 in the revised manuscript**).

References:

A.M.G. de Bruijn, K. Butterbach-Bahl, S. Blagodatsky, R. Grote: Model evaluation of different mechanisms driving freeze–thaw N2O emissions, Agriculture, Ecosystems & Environment, 133, 196-207, https://doi.org/10.1016/j.agee.2009.04.023, 2009.

Ford, T. W., Harris, E., and Quiring, S. M.: Estimating root zone soil moisture using near-surface observations from SMOS, Hydrol. Earth Syst. Sci., 18, 139–154, https://doi.org/10.5194/hess-18-139-2014, 2014.

Haas, E., Klatt, S., Fröhlich, A. et al. LandscapeDNDC: a process model for simulation of biosphere–atmosphere–hydrosphere exchange processes at site and regional scale, Landscape Ecol, 28, 615–636, https://doi.org/10.1007/s10980-012-9772-x, 2013.

Houska, T., Kraus, D., Kiese, R., and Breuer, L.: Constraining a complex biogeochemical model for CO2 and N2O emission simulations from various land uses by model–data fusion, Biogeosciences, 14, 3487–3508, https://doi.org/10.5194/bg-14-3487-2017, 2017.

Revised.
These relevant references have been added which were highlighted in the Reference section.