Comments 1：

The manuscript is working on an important topic with a clear objective: to examine the response of non-CO_2 emissions (i.e., CH_4 and N_2O) to the manipulation of water table and N deposition in an alpine peatland ecosystem. Unfortunately, several major concerns from methodologies and discussions in the current draft make it unacceptable for publication by Biogeosciences.

major comments:

1. the sampling frequency in the year 2018 is too low to capture the temporal variation of the gas fluxes (only five sampling events were conducted over five months). Therefore, the cumulative emissions calculated contain high uncertainty.

Reply: Thanks for the comments. At the beginning of the experiment in 2018, we planned to measure GHG fluxes once a month, and this was confirmed by the previous study listed below, which used monthly GHG flux to assess the temporal variability and cumulative emissions. We agree that monthly sampling is not optimal for investigating temporal variations in 2018, and so we measured the GHG emissions frequently in 2019. We hope that the 2018 data could be better supported by the more frequent 2019 data. Furthermore, our main goal was to compare the different treatments and not to assess temporal variations. Still we agree that the high uncertainty connected to the cumulated fluxes should be better acknowledged and we have added this in the discussion. In Line 245-247 as follows:

R. Cao, Y. Chen, X. Wu, Q. Zhou, Sun S (2018) The effect of drainage on CO_2, CH_4 and N_2O emissions in the Zoige peatland: a 40-month in situ study. Mires and Peat 21:1-15

It is not known whether the low frequency of GHG sampling in 2018 could cause uncertainties in the cumulative CH_4 emissions in 2018, but this was better supported by the more frequent events in 2019.

2. the second hypothesis points to the altered "efficiency of utilising nutrients for CH_4 and N_2O production" by regulation of redox conditions through water table manipulation. However, no data in the current study can support such a hypothesis.

Reply: In fact, we did lack of enough data to fully support the hypothesis about utilizing nutrients for CH_4 and N_2O production by regulation of redox conditions through water table manipulation. And so, we focus on the following hypotheses as follows in L72-74:

The effects of N deposition on CH_4 and N_2O emissions would be associated with WT levels, with high CH_4 and N_2O emissions at high WT levels.

3. the global warming potential (GWP) in the current study simply sums up the non-CO_2 emissions (based on their radiative forcing). Without including net ecosystem CO_2 exchange, or change of SOC stock, critical limitations exist in the significance of this work (the effects of treatments (water table and N deposition) on the GHG budget of the studied ecosystem). The elevated CH_4 emissions under a higher water table could be offset sufficiently by the depressed SOC decomposition, leaving the net effect unclear.

Reply: Thanks for your comments. We agree that our study could not show the GWP budget without data from CO_2 exchange or SOC stock, and hence, we deleted all the results about the GWP in the manuscript. We are now trying to focus on the non-CO_2 GHG emissions, to see the response of CH_4 and N_2O emissions in the peatland to the increased N deposition levels at different water table levels.

4. discussions are generally shallow, and some parts are inappropriate. For example, many discussions are more like introductions instead of discussions (e.g., L276-277;

L316-319; L348-365, etc.). Section 4.4 is simply not implications, but background information that fits appropriately in the introduction, except for a few lines in the last paragraph. Discussions on denitrification and microbial N_2O production (L309-315) are weak as soil TN is the only N measured. Discussions state some findings are "quite novel" (L321-322; L344-346) but fail to justify them (what is the implication and the potential contribution/influence if these are considered novel findings?)

Reply: We have carefully revised the discussion based on the reviewer comments. We have thoroughly revised the discussion of the manuscript, including the Section 4.4 implications (now Section 4.3). We carefully revised the parts about denitrification and microbial N_2O production as follows in L291-293. We have already deleted the “quite novel” parts and revised the implications in Section 4.3.

The N deposition increased soil TN ($F = 4.49$, $P = 0.002$) in our study and is likely to supply more N substrate (NH_4^+ and NO_3^-) in soil (Zhu et al., 2020). The consequently increased N substrate could potentially activate the microbial process of N_2O production and increase N_2O emissions (Yue et al., 2021).

specific comments:

1. the units of the cumulative emissions (i.e., the main result) are confusing. Why are they "g C/N m-2 yr-1"? Based on the equation provided (L162), they should be "g C/N m-2" and calculated by integration over the growing season. Did the authors extrapolate the calculation to the non-growing seasons?

Reply: Thanks for helping us revising these mistakes, we revised all the units for cumulative emissions as g C/N m^{-2} instead of g C/N m^{-2} yr^{-1} in the manuscript. We did not extrapolate the calculation to the non-growing seasons.

2. related to the question above, what happens to the non-growing season? any gas sampling was conducted from the mesocosm? Due to the low temperature, probably soils are frozen and thus the microbial activities are low, but the authors are recommended to include the explanation in the methodology and justify (with proper references) that emissions from growing seasons heavily dominated the gas fluxes.

Reply: We did not conduct any sampling in the non-growing season during the two years. As you mentioned, the low temperature and microbial activity in soil implied that the non-growing seasons GHG emissions have only a minor contribution to the yearly budget, which was also confirmed in the previous study, listed below. We also justified in the methodology about the GHG sampling only in the growing seasons in L113-116.

Reference: Peng H., Guo Q., Ding H. et al. Multi-scale temporal variation in methane emission from an alpine peatland on the Eastern Qinghai-Tibetan Plateau and associated environmental controls[J], Agricultural and Forest Meteorology, 2019, 276-277.

Justified part: The yearly budget of GHG emissions in the Zoige peatland was dominated by the growing season GHG emissions (Peng et al., 2019), therefore, we measured the CH_4 and N_2O fluxes with the sampling events of 1-3 times per month during the growing seasons in 2018 and 2019 in our study. In total, 16 sampling occasions of individual fluxes were recorded for CH_4 and N_2O.

3. another unit issue for GWP. if cumulative emissions have the unit of "g C/N m-2 yr-1", why GWP ended up with "g CO_2-eq m-2" based on the equation provided (L171)? shouldn't it be "g CO_2-eq m-2 yr-1"?

Reply: We deleted all parts related to GWP, while focusing on the non-CO_2 emissions in the peatland.

4. the experimental design on the levels of N deposition includes an unrealistically high dose (i.e., 160 kg N ha^{-1} y^{-1}). It is fine to examine the relationships, but proper efforts should be made to justify such a design in the discussion.

Reply: In consideration of the possible non-linear relationship between the GHG emissions and N deposition levels, multi levels of N depositions are essential in the experimental design. We did discuss about the levels of N deposition in the experiment, and probably the level of 0-40 kg N ha^{-1} y^{-1} was high enough to stimulate the near future N deposition. The reason for designing such a high dose (i.e., 80 or 160 kg N ha^{-1} y^{-1}) is that we want to consider the possible N input from fertilizers or livestock excreta. we believe the additional results generated from the high-level N deposition could best support the conclusion of relationships between the GHG emissions and N deposition levels from 0-40 kg N ha^{-1} y^{-1}, making the linear or non-linear fitting more reliable. Moreover, this is not the first design of such a high level of N deposition in the Qinghai-Tibetan Plateau (listed below), and we briefly added one sentence (listed below) in the methodology to support the design of such a high dose of N deposition in L106-108.

The added sentence: The three lowest levels (N_0, N_{20} and N_{40}) are covering the gradient of current and near-future deposition levels while the two highest levels (N_{80} and N_{160}) represent levels of N-enrichment resulting from extreme deposition possibly levels possibly combined with fertilization.

The new Referece: Qu S., Xu R., Yu J. et al. Nitrogen deposition accelerates greenhouse gas emissions at an alpine steppe site on the Tibetan Plateau[J], Science of the Total Environment, 2021, 765: 144277.

5. for the calculation of cumulative emission, the authors can simply describe it like "linear interpolation between sampling events using the trapezoidal rule" instead of providing the equation and explanation for the notations (L161-166). instead, the authors are recommended to provide equations for calculating the gas fluxes rather than

simply saying "calculated by the slopes of linear regression between gas concentrations" (is it corrected with temperature? atmospheric pressure?)

Reply: We revised the description about the calculation of GHG flux in L161-164, and revised the description for cumulative emissions in L128-134.

some minor corrections:

1. L77: highlevel -> high-level N deposition

Reply: We revised it in L72 as follows.

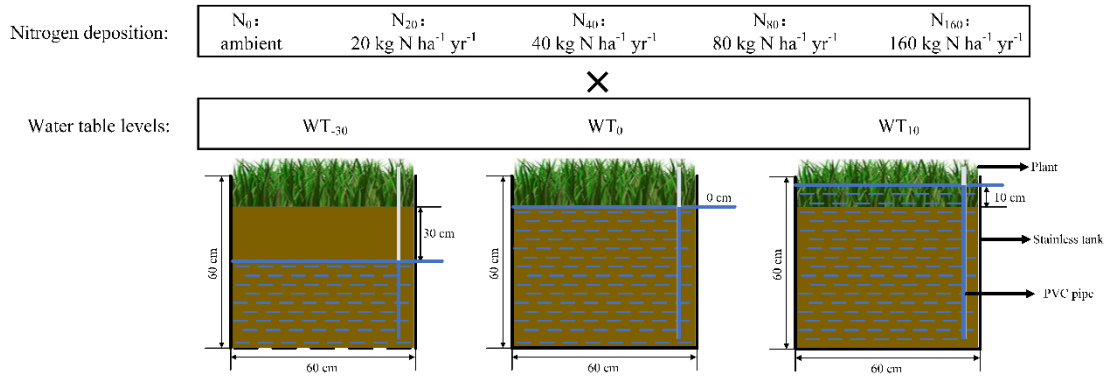
a slight increase in N deposition might stimulate both CH_4 and N_2O emissions, but a high-level N deposition would inhibit CH_4 emissions while N_2O emissions would reach a threshold.

2. L78: "aerobic conditions"? are the authors trying to mention redox conditions? similar expressions occur in several parts of the remaining text, consider rephrasing (e.g., L295, L383).

Reply: Yes, we meant altered redox conditions, and we revised the aerobic conditions as redox conditions in the manuscript.

3. descriptions of the mesocosm design and treatment manipulation are not very clear, the authors are recommended to include a supplementary figure for a clear illustration. in particular, the definition of WT_0 can be confusing (i.e., soil-water interface; L101), is it simply "the soil surface"?

Reply: We are sorry this phrase led to some confusion. In our study, the WT_0 means that the water level is just at the soil surface. We reformulated this explanation in the manuscript, and we added a supplementary figure to better illustrate the design as follows.



4. L158: despite -> regardless of. Also, the description of how the GAM is applied could be oversimplified. Ask this question may help the authors to improve the description: does the current description sufficient for peer researchers to reproduce the analysis?

Reply: We now revised the details about the application of GAM in the R listed below in L157-159. We hope this description could be clear enough to reproduce to the analysis.

Via the R package "mgcv" (Wood, 2017), we used method "gam" to perform the GAM analysis and method "predict.gam" to see the response value of GHG emissions along the N deposition gradient from 0 to 160 kg N ha^{-1} yr^{-1}.

5. L166-168. difficult to follow. how can the heterogeneity be reduced?

Reply: We now rephrased the description about this part listed below in L161-164.

The cumulative GHG emissions in the growing seasons of each year were calculated by linear interpolation between sampling events using the trapezoidal rule (Goldberg et al., 2010). In addition to the cumulative GHG emissions between the first and the last sampling event, the GHG emissions from 1st June to the first sampling and from the last sampling to 30th September were taken into consideration.

6. L173-174. reference missed.

Reply: Thanks, but we deleted all the parts about GWP including L173-174.

7. L175. "by applying the statistic R software" -> "using R"

Reply: we now revised it in L165 as follows:

Statistical analysis was carried out using R (version 3.4.3) (R Development Core Team, 2017).

8. L180. SWC has been abbreviated in L146.

Reply: Thanks, we already revised it in L148 as follows:

SWC was determined by using a TDR300 moisture meter (Spectrum Technologies Inc., Plainfield, Illinois, USA).

9. L205-206: "the highest value occurring"-> "with the highest value observed"

Reply: we now revised it in L194.

The response of the cumulative CH_4 emissions to N deposition was non-linear under WT_0 and WT_{10} conditions (Figure 3), with the highest value observed in the N_{20} treatment.

10. L245: "combination" -> "interaction"

Reply: We deleted the GWP part including this.

11. L267: add "CH4" before "emissions"

Reply: we revised it in L237 listed below.

This result partially supported hypothesis I – that N deposition would consistently enhance N_2O emissions and that increasing N deposition would increase CH_4 emissions until a threshold is reached (hypothesis I).

12. L273-274: needs rephrasing. Note that the study did not measure oxygen content, and therefore the expression like "oxygen content declined" is not appropriate. Consider: "With higher WT levels, SWC increased and likely formed more anaerobic conditions conducive to CH_4 production, leading to elevated CH_4 emissions (references)."

Reply: Thanks, and we revised the sentence listed below in L249-251.

With higher WT levels, SWC increased and likely formed more anaerobic conditions conducive to CH_4 production, leading to elevated CH_4 emissions (Evans et al., 2021, Hoyos-Santillan et al., 2019, Zhang et al., 2020).

13. L276: add comma after "considerably"

Reply: We revised this whole part and deleted the “considerably”.

14. L293-295: difficult to follow.

Reply: We rephrased the whole part to make it clear and readable listed below in L267-269.

We speculate that the WT levels were associated with nutrient exploitation by microorganisms and, accordingly, that the higher WT levels promoted diffusion of the added N in the water-filled soil pore, N thus becoming readily accessible in the microbial process (Wang et al., 2017).

15. L305: reference format: Gao et al. (2014).

Reply: We revised this part in Discussion and deleted it.

16. L343: from CH_4 to N_2O -> from N_2O to CH_4

Reply: We deleted the whole part of GWP including “from CH_4 to N_2O”.

17. L369: increased -> decreased

Reply: We revised the the whole part of Section 4.3-Implications for future GHG emissions in alpine peatlands, including “increased”.