

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2022-53

Anonymous Referee #1

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Referee comment on "Effects of water table level and nitrogen deposition on methane and nitrous oxide emissions in an alpine peatland" by Wantong Zhang et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-53-RC1>, 2022

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The manuscript is working on an important topic with a clear objective: to examine the response of non-CO<sub>2</sub> emissions (i.e., CH<sub>4</sub> and N<sub>2</sub>O) 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.

2. the second hypothesis points to the altered "efficiency of utilising nutrients for CH<sub>4</sub> and N<sub>2</sub>O production" by regulation of redox conditions through water table manipulation. However, no data in the current study can support such a hypothesis.

3. the global warming potential (GWP) in the current study simply sums up the non-CO<sub>2</sub> emissions (based on their radiative forcing). Without including net ecosystem CO<sub>2</sub> 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<sub>4</sub> emissions under a higher water table could be offset sufficiently by the depressed SOC decomposition, leaving the net effect unclear.

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<sub>2</sub>O 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?)

specific comments:

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

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

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

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?)

some minor corrections:

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

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).

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 WT0 can be confusing (i.e., soil-water interface; L101), is it simply "the soil surface"?

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?

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

6. L173-174. reference missed.

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

8. L180. SWC has been abbreviated in L146.

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

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

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

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 CH4 production, leading to elevated CH4 emissions (references)."

13. L276: add comma after "considerably"

14. L293-295: difficult to follow.

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

16. L343: from CH<sub>4</sub> to N<sub>2</sub>O -> from N<sub>2</sub>O to CH<sub>4</sub>

17. L369: increased -> decreased