General comments:

This is a very coarse estimation of N2O in the pan-arctic region with a lot of issues. The effort is laudable and I also want to highlight that it is one of the first attempts to model N2O budget of the Arctic, but some of the issues really need to be addressed before this MS can be published.

- The author should clearly define the extent of the pan-arctic. In Table 1, the authors showed the sites for N2O emission observations that are used for model parameterization and verification. Which sites belong to the pan-arctic? Does France belong to the pan-arctic? Boreal Forest? Tibetan Plateau? As scientists, the authors should be careful here. Please clearly define pan arctic and remove the data that do not belong to pan arctic. I also suggest to use more data from real arctic sites (currently only 4) which are available in the literature for model calibration.

Response: There are various definitions of the pan-Arctic region, and in our study, we define it as the area north of 45°N (Xiao et al., 2004; Du et al., 2016). Consequently, a portion of France falls within this region, which also encompasses boreal forests. While we made extensive efforts to gather observations, there are limitations to the available data. We incorporated data from several sites located between 42°N and 45°N, as well as sites on the Tibetan Plateau, which shares a similar climate with the pan-Arctic permafrost zone. Some higher latitude locations, like Finland, may be considered "true Arctic sites," but unfortunately, they lacked the necessary long-term observations (more than 5 months) required for our parameterization. Additionally, some of these sites were barren with no vegetation. Thus, we used some sites not in the Pan-Arctic region for model verification to show the model performance.

To provide clarity, we have now included our definition of the pan-Arctic region in the Introduction section.

Xiao, X., Zhang, Q., Boles, S., Rawlins, M., & Moore III, B. (2004). Mapping snow cover in the pan-Arctic zone, using multi-year (1998-2001) images from optical VEGETATION sensor. International Journal of Remote Sensing, 25(24), 5731-5744.

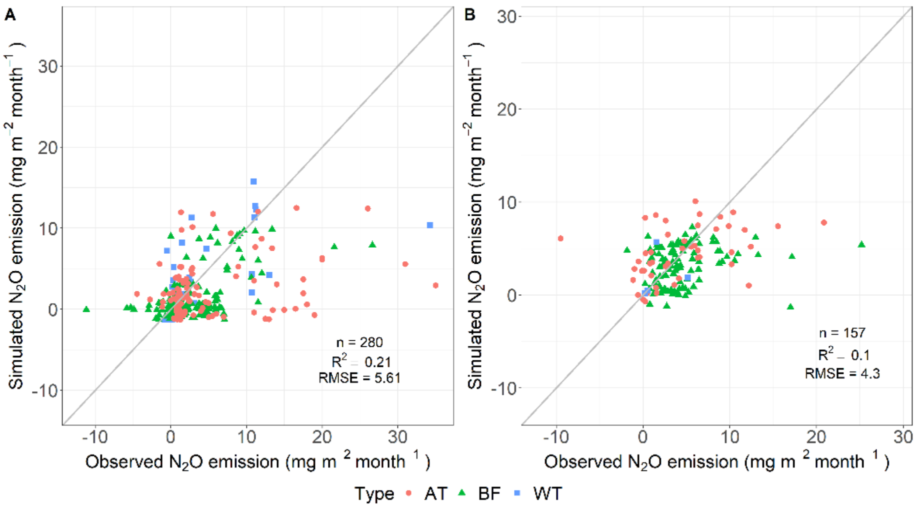
Du, J., Kimball, J. S., Jones, L. A., & Watts, J. D. (2016). Implementation of satellite based fractional water cover indices in the pan-Arctic region using AMSR-E and MODIS. Remote Sensing of Environment, 184, 469-481.

- The authors should please provide detailed description of how they collected and cleaned the site observation data in Table 1 that are used for model parameterization and verification and make the data available, so that the reviewers and readers can check if everything that the authors did were correct.

Response: Observations were digitized from figures in the cited references, and all observations were summed up to monthly emissions for comparison purposes. We will include these datasets into the Purdue University Research Repository to allow the public to access proper citations for the data.

- Model parameterization and verification are two steps. What part of the site data were used for model parameterization and what part were used for verification? Looks the authors do not separate them (Figure 2).

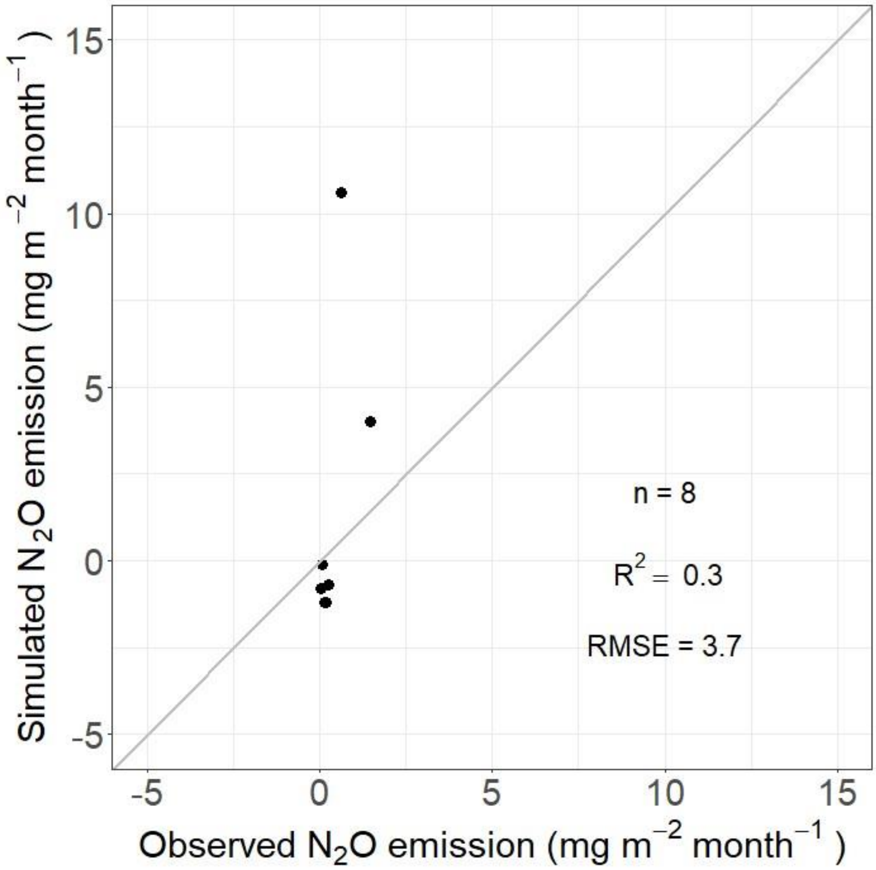
Response: Below is the new Figure 2 we are going to use. In Figure 2, we tested more sites in the high arctic. More information is shown in the attached table, we will add more information (e.g., measurement methods) to the table and add this table to manuscript.



Site	Classification	Location	Country	Latitude	Longitude	Soil bulk density (g cm-3)	pH	Time	Reference
AT	Upland grassland	South of Edinburgh	Scotland	N 55.9	W 3.5	1.4	5.3	2002-3 to 2003-12	Jones et al. (2005)
AT	Upland grassland	Siggen	Germany	N 47.8	E 10	1.3	6.6	1996-11 to 1998-3	Glatzel et al. (2001)
AT	Alpine grassland	Eastern Swiss Alps	Swiss	N 46.78	E 9.87	0.85	4.3	2010-12	Mohn et al. (2013)
AT	Dry Tundra	Disko Island	West Greenland	N 69.3	W 53.5	1.3	5.5	2015-8, 2016-7 to 2016-8, 2018-6 to 2018-9	Xu et al. (2023)
BF	Boreal forest	Jutland	Denmark	N 51.7	E 9.9	1.3	4.2	2008-3 to 2010-6	Christiansen et al. (2012)
BF	Boreal forest	Northern Tyrolian Limestone Alps	Austria	N 47.6	E 11.6	1.3	7	2006-5 to 2009-8	Heinzle et al. (2023)
BF	Boreal forest	Southern Bavaria	Germany	N 48.5	E 11.2	1.1	3.8	2004-1 to 2008-12	Wu et al. (2010)
BF	Boreal forest	Solling uplands	Germany	N 51.7	E 9.7	1.3	4	2015-3 to 2015-10	Wen et al. (2017)
BF	Boreal forest	Lapland	Finland	N 67.4	E 26.6	1.1	4.9	2012-7 and 2012-10	Dinsmore et al. (2017)
WT	Wet Tundra	Devon Island, Nunavut	Canada	N 75.7	W 84.6	1.5	6.9	2004-6	Ma et al. (2007)
WT	Wet tundra	Ny-Ålesund	Norway	N 78.9	E 11.9	0.6	6.7	2009-7	Chen et al. (2014)
WT	Peatland	Seida	Russia	N 67	E 63	0.1	3.7	2011-8	Siljanen et al. (2019)
WT	Peatland	Taymyr	Russia	N 75.4	E 107.7	1.2	6.4	2011-8	Siljanen et al. (2019)

- I would like that the authors further test the performance of the model in simulating N2O. The co-author here Narasinha Shurpali has N2O data from eddy covariance. Please use this continuous eddy covariance data to further test the performance of your model. If the model works well, the reviewers and readers will then have more confidence in your model.

Response: We obtained data from Dr. Narasinha Shurpali and conducted tests with it. Below figure shows the comparation. Regrettably, our model did not perform as well as we had hoped. Upon further examination, we believe the primary reason for this discrepancy is that Dr. Narasinha Shurpali's observations are based on energy crops, a vegetation type that our model has not been adequately parameterized for. Instead, we utilized parameters from alpine tundra, which introduced uncertainties into our simulations. Therefore, we did not include the data and model comparison in the revised manuscript since the site is an energy crop system.



- Peatlands are an important part of the high-latitude region and the highest emitters of N2O among natural arctic ecosystems. But, peat is not considered in the model. How does your model represent N2O fluxes from peatlands?

Response: During the model parameterization process, we included two sites which are non-tree peatlands. The parameterization is extrapolated to those peatland grid cells. In addition, we noticed that N_2O emissions from peatlands are relatively minimal when compared to the total N_2O emissions in our simulated region (as reported by Hugelius et al., 2020, at 0.022 ± 0.005 Tg N), accounting for less than 2% of the total (0.022/1.2). We will add a new section to the Discussion to provide further clarification.

Hugelius, G., Loisel, J., Chadburn, S., Jackson, R. B., Jones, M., MacDonald, G., ... & Yu, Z. (2020). Large stocks of peatland carbon and nitrogen are vulnerable to permafrost thaw. Proceedings of the National Academy of Sciences, 117(34), 20438-20446.

- the discussion is not well organised. while the main point, modelled N2O emission, receives far too little attention and is poorly referenced, the authors ramble and repeat themselves about N2O uptake and N deposition, which is interesting, but has relatively minor importance here. please re-organize and elaborate on the N2O budget and emissions and the microbial pathways (nitrification, denitrification). also, some assumptions can be made for future emissions based on the sensitivity analysis made with variable temp and preciptation.

Response: Thank you for your valuable suggestions regarding the organization of the Discussion section. We will enhance the content by incorporating a more in-depth analysis of the N_2O budget, emissions, and microbial pathways. Additionally, we will introduce assumptions regarding future emissions. Your input is greatly appreciated and will contribute to the overall quality of our manuscript.

- Spin up is not described in the method. Please describe it. This is just one case. The authors should describe all the steps that you did in the method in detail. Please check if anything else is missing and describe the missing steps.

Response: In this revision, we will provide a more detailed description of the modeling process, including aspects such as spin-up.

- all N2O uptake data should have a negative sign.

Response: We will check the whole manuscript to make sure all the N_2O uptake has a negative sign.

Specific comments:

Lines 44-45: double check if all citations are listed in the reference section. I could not find e.g. the reference for Vigot et al. 2017
Response: It appears to be a typo in the author's name. It should indeed be "Voigt," and we will ensure that this reference is listed correctly in the manuscript. We will also conduct a thorough review to confirm that all citations are accurately listed.

Lines 55-56: this is incorrect. Please rephrase to make this sentence read “Soil moisture controls the oxygen availability for microbes and thus nitrification and denitrification rates.” Add to the next sentence that denitrification operates under high water content and nitrification under low water content, and that generally denitrification produces much more N2O.
Response: We will revise this sentence; however, it would be inaccurate to claim that denitrification produces much more N_2O without considering the influence of

nitrification and other controlling factors. We will make sure to revise the sentence to accurately represent the relationship between denitrification, nitrification, and N_2O production, considering specific soil moisture levels and other relevant factors.

Lines 70-76: what about the model QUNICY? this model has been calibrated for permafrost regions and N2O emissions have been simulated. Make sure to appropriately cite this reference in your MS
Lacroix, S. Zaehle, S. Caldararu, J. Schaller, P. Stimmler, D. Holl, et al. Glob Chang Biol 2022 Vol. 28 Issue 20 Pages 5973-5990
Response: Thanks for pointing out this study, we will cite this study in Introduction and Discussion sections.

Line 89: 'Here we revised the N cycling algorithms in TEM by incorporating the loss of nitrogen through gas emissions'. Looks this is not correct. Yu, 2016 and Yu and Zhuang, 2019 already considered the loss of nitrogen through gas emissions (Quantifying global N2O emissions from natural ecosystem soils using trait-based biogeochemistry models).
Response: Yu & Zhuang (2019) did account for N_2O gas loss, however, the detailed method of modeling N_2O emissions is different in our current version. Instead of including ammonia-oxidizing bacteria and nitrite-oxidizing bacteria (Yu & Zhuang, 2019), here we revised the N cycling algorithms in TEM by incorporating the loss of nitrogen through gas emissions with empirical equations, the uptake of N_2O from the atmosphere, and additional inputs of organic nitrogen and carbon resulting from permafrost thawing. We have added additional clarification in the Method section to outline the difference.

Line 91: 'and additional inputs of organic nitrogen and carbon resulting from permafrost thawing (Fig. 1)'. This point is not shown in Fig. 1.
Response: We aimed to create a generalized conceptual figure, because permafrost thawing is not occurring uniformly across our simulation region, we didn't include this process in the original figure. Now we added a dash line arrow to the organic box. The new figure is attached here.

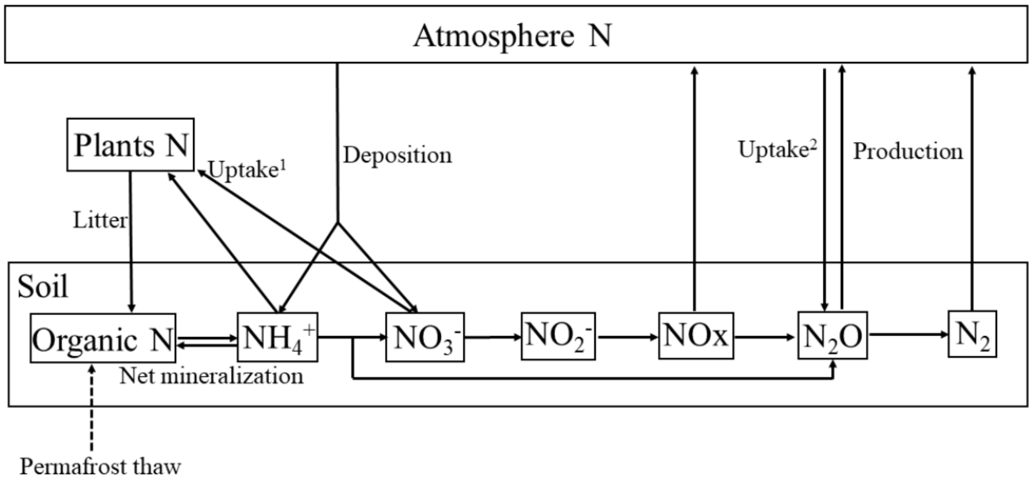


Figure 1: 'the difference between mineralization (organic N mineralized to inorganic N) and mobilization (inorganic N to organic N)'. First, it should be immobilization instead of mobilization. In addition, in the figure, you just show the immobilization of NH4+. I would like to know, if the immobilization of NO3- is considered in the model? or only the immobilization of NH4- is considered? In addition, I would like to know, if the model can consider the DNRA process and the heterotrophic nitrification process?

Response: We revised mobilization to immobilization. Our immobilization only considered immobilization of NH_4^-. We did not consider the DNRA process and heterotrophic nitrification because of lack of quantitative studies.

Lines 104-106: where did the authors get data on soil gas concentration of N2O from artic sites to estimate N2O uptake? i do not find these data in the MS. please display.

Response: Soil N_2O concentration was calculated by the net N_2O emission in 30 cm, we assumed soil N_2O accumulated in 30 cm before emitting out. We updated this information in the Method section.

Line 113: What kind of observational data is used?

Response: We will update the observational data in the table by adding the measurement methods.

Line 114: what is Nip?

Response: It is a typo. We will check the equations and parameters names for the whole manuscript.

Line 135: consider using the word "depth" instead of "deep".

Response: We have made the change.

Lines 135-138: Two bold assumptions are evident here. Firstly, it's widely acknowledged that carbon (C) and nitrogen (N) content tends to decrease with increasing profile depth, a fact well-documented in the literature. Secondly, the established understanding is that the main reservoir of C and N is relatively stable, indicating its slow decomposition rate. Consequently, the assumption that half of the nitrogen pool undergoes mineralization within a few years appears, from my perspective, to be an overestimation. Nevertheless, if better data cannot be acquired, I suggest that the authors, at the very least, account for the uncertainties linked to these assumptions in their model (lines 177-180).

Response: Soil organic carbon typically decreases with depth in low shallower layers (0-1m), however, from Wang et al., Figure S8 shows the soil organic carbon stock does not always decrease with depth down to 25m on the Tibetan Plateau. In our simulation, the active layer at many sites extends beyond 1 m. Unfortunately, we don't have access to deep layer profiles of carbon stock in the pan-arctic region, and the vertical profile of nitrogen stock is also unavailable, even in shallow soil depths. Using decreasing C stock by depth but stable N stock will cause large uncertainties because our N mineralization depends on the ratio of soil C/N, we would like to make soil C and N model in the same way to keep the balance. Above are reasons that we assume they are uniformly distributed.

Regarding the second question: We assumed that half of the added nitrogen is organic N, meaning that there are half other formats of N that will not undergo N mineralization. N mineralization of these extra organic N is the same as the mineralization of internal N. The rate is controlled by many factors, it is important to note that this organic N will not be entirely utilized in a single year; instead, its mineralization will occur gradually over time.

We have revised the Discussion in the manuscript to provide further clarification and added it to the Discussion about the uncertainties.

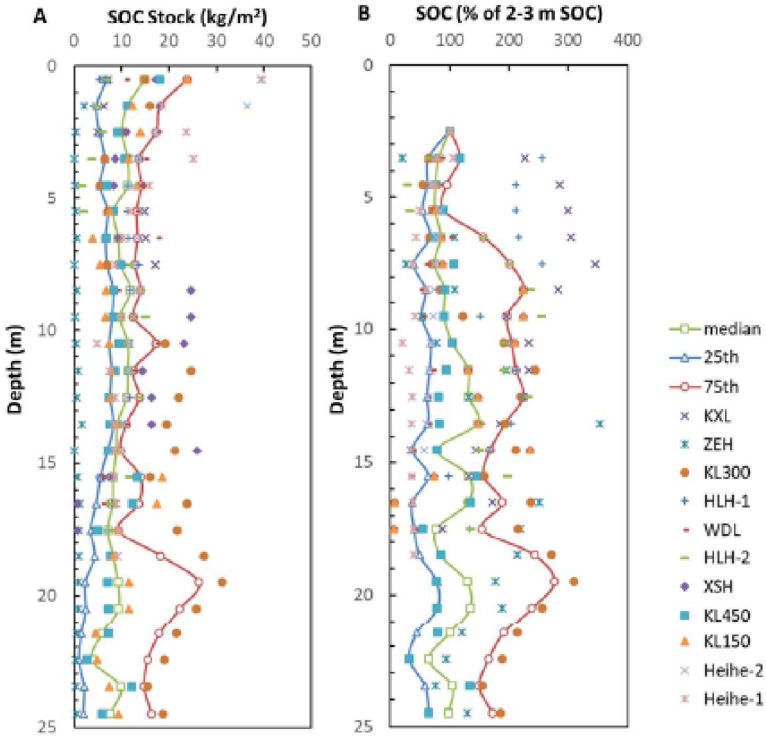


Fig. S8. Deep SOC profile down to 25 m depth according to observations from 11 deep boreholes. (A) Absolute SOC profile (unit: kg/m^2) calculated from the original data provided by Mu et al. (*24*) from the 11 deep borehole observations. (B) Relative SOC profile as percentage of 2-3 m SOC stock with 2-3 m SOC set to be 100% as the baseline (i.e., SOC stocks below 3 m at 1 m depth interval divided by 2-3 m SOC stock at each borehole). The results from the 11 boreholes are shown by the dots. The median, 25th percentile and 75th percentile values of different borehole results at each 1 m depth interval down to 25 m are connected and shown by the solid lines, respectively.

Line 140-143: as mentioned in the general comments, I am curious about the rationale behind incorporating numerous boreal forests and sites from France in model calibration and parameterization, given that these sites are situated well beyond the the pan-Arctic zone. Several of these forests lack permafrost entirely and, as such, should ideally be excluded from the analysis. Furthermore, to align with the manuscript's title appropriately, alpine sites should also be omitted. This revision would result in merely four wet tundra sites available for model calibration and parameterization, a quantity that is evidently insufficient. It would be more appropriate for the authors to use additional published N2O emissions data from

Arctic sites for model calibration and parameterization. I point towards the work of Voigt et al. (2020) for these data.

1. Voigt, L. van Delden, M. E. Marushchak, C. Biasi, B. W. Abbott, B. Elberling, et al.

Distributor: PANGAEA 2020 DOI: 10.1594/PANGAEA.919217

Response: We define the pan-Arctic region as encompassing areas north of 45°N latitude. Consequently, this definition includes parts of France as well as boreal forests. To enhance the comprehensiveness of our model, we incorporated additional data from Voigt et al. (2020), which pertains to high Arctic sites, into our model performance assessment. The outcomes of these tests are elaborated in response to the third comment. Besides, Voigt et al. (2020) concluded that "vegetated soils in permafrost regions are often small but evident sources of N_2O during the growing season (~30 μg N_2O–N m^{-2} day^{-1})" is within the range of our simulation. We will add this reference into discussion about N_2O emission.

Table 1: I am not aware of all the sites, but double check the classification. At least the Russian site in Voigt et al. (2017) is not a wet tundra. It is a permafrost peatland (dry, raised peatland) and an upland mineral soil. Maybe use simply the term "Arctic Tundra"? Again, please clearly define pan-arctic and remove the data that do not belong to pan arctic from the Table, model analysis and runs and MS as such.
Response: Vegetation WT includes wet tundra and vegetated peatland. We checked the vegetation composition in supporting materials, this site is a peat plateau with vegetation, thus it is included. We will add more explanation about the vegetation classification.

Table 2: please provide the full name of each parameter.
Response: We will check the whole manuscript to make sure all the parameters have full names.

Line/Chaper 167: Again: the author should clearly define the extent of the pan-arctic. is the boreal region inlcuded? alpine? Tibetan Plateau only? the permafrost region of the northern latitudes? if the latter is true, the title needs to be changed.
Response: We define the pan-Arctic region as encompassing areas north of 45°N latitude. We use some studies from the Tibetan Plateau for parameterization because these sites have permafrost soils and similar climate to the arctic.

Line 197: there is a typo in "pan-arctic"
Response: We have made this change.

Line 227/Chapter 3.3.

can the authors elaborate a bit on why you did not find large effects of precipitation changes on N2O emissions? that is a bit unexpected, given that soil moisture is primary control of N2O production and consumption. could it be that precipitation changes do not translate into soil moisture changes? the authors mention that there were large regional changes in nitrification rates (what about denitrification, by the way?), but i do not get how the total N2O production from the different microbial pathways then does not change.

Response: The summed change in N_2O emissions due to precipitation did not exhibit as significant variation as temperature. This can be attributed to the fact that we evenly assign monthly precipitation data to a daily scale, which, when precipitation translated to soil moisture, did not yield substantial changes in soil moisture despite a 30% variation in monthly precipitation. Consequently, both nitrification and denitrification rates did not experience substantial shifts. However, that different sites displayed varying responses. Dry sites exhibited more pronounced changes compared to wet sites. We plan to include a map in the Appendix to visually represent this variability.

This could also be attributed to a potential trade-off between nitrification and denitrification. If nitrification decreases with higher soil moisture levels, it might not supply sufficient NO_3^- for denitrification. Consequently, even when soil moisture conditions become more favorable for denitrification, the denitrification rate may not experience a significant increase.

While the summed nitrification and denitrification rates did not exhibit substantial changes, it's important to note that individual sites displayed varying responses to shifts in soil moisture. We will add a map or histogram figure to the Appendix to show the spatial pattern.

Line 240/Discussion

the discussion is absolutely not in line with the results. the authors discuss N2O uptake, N deposition while the modelling work focuses on N2O emissions and the effect of permafrost thaw.

First off, the discussion should start with a chapter on N2O emissions (4.1.) and not with the role of N2O uptake in the net emissions. this process has relatively minor importance here (about 10% of the emissions, as the authors state themselves), and this discussion on N2o uptake should be considerably toned down (e.g. completely delete lines 277-288, not focus of the MS). the discussion on the result of N2O emissions is generally missing or far too short (lines 335-347), and needs to be stronger in the MS (again at the beginning of the discussion, 4.1., at least one page, while reduce the discussion about N2O reduction by more than half). also, compare your results much more with all the data available from the literature. It should be stressed that the estimate of the pan-arctic N2O budget the authors provide is one of the largest currently reported in the literature.

Second, the whole discussion about N deposition comes out of a blue. this topic should be part of the introduction, methods and results, only then it can be discussed.

Response: Thank you for your valuable suggestions regarding the restructuring of our Discussion section. We plan to expand upon the topic of N_2O emissions by drawing comparisons with a broader range of relevant literature. Additionally, we will introduce the concept of N deposition more comprehensively to enrich the discussion as you suggested.