Biogeosciences Discussions

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Comment on bg-2021-228

Anonymous Referee #1

Referee comment on "Sources of nitrous oxide and the fate of mineral nitrogen in subarctic permafrost peat soils" by Jenie Gil et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-228-RC1, 2021

The manuscript by Gil et al. addresses a pronounced research gap by investigating gross N turnover and inorganic N fates as well as N_2O emissions in permafrost peatlands, including N_2O source partitioning. They report much higher gross ammonification and nitrification for vegetation free compared to vegetated peatlands, which is explained e.g., by absent plant competition for N. Such detailed N cycle process knowledge is very scarce for permafrost ecosystems, which still makes prediction of permafrost nitrogen climate feedbacks highly uncertain. So this clearly is a timely study even if the experiments were already conducted more than 10 years ago. The overall manuscript quality is fine.

The quality of such field 15N studies to assess gross N turnover is strongly depending on a thorough experimental setup and this difficult task mostly appears to have been done very competent and thoroughly. On the other hand, the chosen experimental setup with mirrored 15N labelling and all of its advantages and disadvantages was obviously designed to run the Ntrace model to estimate N turnover rates which then was not done. So the reader wonders, why not? Due to addition of ammoniumnitrate in all treatments, gross nitrification rates are likely stimulated by substrate addition, which needs to be considered and discussed. Another issue could be that gross rates of N turnover were calculated based on day1 to day3 data with day3 being a clear outlier in 15N recovery for bare peat (much lower than at day1 and day 5; Fig. S1) – did this low 15N recovery lead to a bias in gross N turnover estimates, eventually because 15N was quickly leached in some labelling plots? Further, the temporal dynamics of 15N recovery in the nitrate pool after 15N-nitrate labelling is problematic. Data show that there is an increase in recovery between day0 and day1, a decrease between day1 and day3 which is used to calculate gross ammonification, followed by another increase. I suppose therefore that atom%15N enrichment of nitrate also shows no persistent dilution. Hence, choosing other time steps for calculating gross nitrification might reveal completely different results or even negative rates. Were there probably quick cycles of abiotic fixation and release of nitrate? Or is this originating from problems with 15N labelling as described above? Based on these thoughts it appears to me that gross nitrification rates in this study might be pretty unreliable. Considering this would require major changes in the discussion section.

Specific comments

P3 L 6: The sentence „Denitrification releases usually more N2O under wetter, more anaerobic conditions…" should be further specified as it is otherwise misleading. Under very anaerobic conditions N2O emissions by denitrification are expected to decline as denitrification until the terminal product N2 is favored.

P 3 L 23: Do you mean microbial immobilization? Please specify.

P 5 L 30 probably a few more details how gas samples were transferred (overpressure? Pre-evacuated vials?)

P 5 L32 Leakage test with a standard gas can be conducted only for other vials

P 5 L 35 ff: the authors write that N2O emissions calculated based on only two concentration measurements were compared with adjacent static chambers that had higher sampling frequency. Good, but what was the outcome of this comparison?

P8 L 18 The abbreviation "T" for treatment might be misunderstood by the reader as extraction time steps, so probably choose another abbreviation for treatment.

Statistics section: Only N2O mentioned, what about gross N turnover?

P9 L 18: N2O measurements are reported for a little more than a month – I therefore would not speak of "seasonal patterns"

Figure 3: It shows 15N excess? The caption states that it is 15N enrichment, which is not true. Please clarify. Generally the 15NO3- seems quite problematic and cannot be explained by gross nitrification but probably by 15N nitrate fixation and release in organic matter? This would question the gross nitrification calculations – showing the atom% excess enrichment would further help to judge this. A thorough and critical discussion is needed here.

P13 L 18: "…27 and 90 times higher.." – sure, dividing by very small rates is giving such impressive numbers which are however a bit misleading as rates at VP were hardly present. And: please give the rates related to soil dry weight as well not only related to cm3.

Table 2: please add rates also related to sdw, there is plenty of space. NO3- consumption: 0.00 – so were rates negative and set zero? Please clarify

Conclusion section: pretty long, and a lot of repetition of results, I suggest to strongly shorten.