

Biogeosciences Discuss., author comment AC3
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Reply on RC3

Jaan Pärn et al.

Author comment on "High greenhouse gas fluxes from peatlands under various disturbances in the Peruvian Amazon" by Jaan Pärn et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-46-AC3>, 2021

Pärn et al., 2021 report greenhouse gas fluxes from peatlands under various disturbances in the Peruvian Amazon. The investigated peatland sites covered (1) a slash-and-burn manioc field, (2) a 12-year-old secondary forest grown over a fallow pasture and a banana plantation, and (3) a natural swamp forest. While I very much acknowledge the scientific effort of data collection from a strongly understudied region of the world, I am afraid that the manuscript suffers in its current form from insufficient structural and scientific quality. This is a pity, since the additional laboratory analyses to quantify N₂ potential from soil cores from the studied sites are novel and interesting.

Generally, the introduction is weak and does not clearly introduce why studying greenhouse gases from peatlands under various disturbances is important. The introduction mentions nowhere methane which is probably the most important greenhouse gas in these ecosystems and only a couple of lines are spent on carbon dioxide. Moreover, the data coverage is poor; Only 9 sampling sessions, over a period of 7 months (Sept 2019 to March 2020) were conducted at two sites (swamp forest; manioc field) and only four sessions during two consecutive days in Sept 2020 were conducted at the remaining secondary forest site. The reader is left with no explanation on why this irregular sampling strategy was chosen. Lastly, the discussion section does not contextualize the findings with other scientific literature, especially the aspect of disturbance. The word disturbance does not even appear in the discussion. Again, methane as an important greenhouse gas in these ecosystems is barely discussed. Given these substantial drawbacks of the study I recommend rejection of the paper in its current form.

Thank you for the thorough and fair comment. We will take the points forward and improve the manuscript accordingly. Indeed, we took the importance of GHG from various disturbances in the tropics for granted to the reader of the journal. We tried to keep the introduction brief and summary at the expense of widely known detail on methane and carbon dioxide fluxes. We also spared the reader from technical details on sampling strategy planning. We also agree on the reviewer's criticism on the underuse of the general concept of disturbance. We believe the manuscript will substantially benefit from expansion on these points and will be glad to improve on them.

Specific comments:

Abstract

General: The Abstract suffers from vague statements and needs to be more concise.

Agreed. We will remove all statements that are vaguely connected to the main discussion.

Line 22: Which 'changes'? Land-use? Climate?

The statement referred to the changes mentioned in the couple of statements above – drought, cultivation and other changes contributing towards the varying oxygen content. However, we will remove this vague statement.

Line 25: At which frequency?

We will detail the frequency here.

Line 26: moderate compared to? Give concrete flux numbers.

Agreed. We will provide the information.

Line 27: 'slight water table drawdown'. Pls be more specific. From inundated conditions? How much is 'slight'.

Good point. We will provide the change in water table in cm from the inundated level.

Line 29: 'Nitrifier-denitrification was the likely source mechanism' based on which underlying data? This is too speculative.

Indeed, we cannot exclusively identify nitrifier denitrification as the mechanism of N₂O production. Therefore, we agree to remove the statement from the Abstract and Conclusions. However, the absence of nitrate still rules out denitrification and nitrification leaving nitrifier denitrification as the only candidate mechanism that directly produces N₂O in the soil. Moreover, as Hergoualc'h et al. (2020) identified nitrifier denitrification as the main N₂O production mechanism in the site before, we feel that the discussion in lines 211–226 is justified.

Introduction:

Line 42-45: How do these numbers relate to each other? Does that mean that these peatlands are a hotspot within the Amazon rainforest hotspot?

Yes, the Peruvian peatlands are a potential contributor within the larger Amazonian hotspot, and we will alter the sentence to state that.

Line 54: This sentence is out of context.

True. The statement needs a transition sentence.

Line 59: There is no transition to the C cycle and related CO₂ emissions.

We intended this paragraph as a summary of all three GHGs. However, we take the point that in this form it may be misleading to the C fluxes again. We will restructure the Introduction section for a more logical order.

Line 62: Can you give a reference for this statement. Drought first and foremost reduces photosynthesis and consequently less assimilates are available for autotrophic and heterotrophic respiration.

A FLUXNET synthesis (Schwalm et al. 2010 Global Change Biology) should do the job.

Line 65: consider a better transition to the soil part of the introduction

Agreed.

Materials and Methods:

General: No structuring into subsections

Indeed, we did not structure this section into subsections like we did not anywhere else. However, we will consider the structuring here.

Line 76: suggest renaming site to 'Swamp primary forest'

Line 76: suggest renaming site to 'Swamp secondary forest'

The correct order of words would probably be 'Primary swamp forest' and 'Secondary swamp forest'. Otherwise, we agree.

Figure 2: what is the Pastaza-Maraniñon Basin? This should be mentioned in the text as well.

Agreed. We will define the basin in the text.

Line 85: How many chambers were installed at the swamp site?

The number is 10. We will state that in the text.

Line 85: stations? Why not calling them plots?

The even more correct name is 'point'. We will change that throughout the text.

Line 86: How long before gas sampling where the collars installed?

We allowed a stabilisation time of several days before the sampling. We will specify this in the Methods section.

Line 92: How much gas was sampled and how? With a syringe and needle through a septum?

We sampled the gas through tubes and medical three-way taps straight into 50mL vials. We will specify this in the Methods section.

Line 100: How was CO₂ measured? How was the GC calibrated?

We determined CO₂ concentration with the same flame ionisation detector. We will specify the calibration in the text.

Line 104: How much of the data was affected by this quality check?

We set 32 out of the 165 N₂O datapoints to 0. We did not set any CO₂ or CH₄ datapoints to 0. We will include the information in the Material and Methods section.

Line 128: A ~7cm by 6cm soil core is not very representative.

It is representative of the 50cm diameter collar. We sampled a soil core from each collar.

Line 137: How were the samples taken from the continuously flushed vessel?

Through a tube reaching the headspace through a hermetic septum.

Line 137: How was this done exactly? N₂ is not easy to measure. How was the GC calibrated?

We detected N₂ concentration on the same analyser as N₂O. N₂ concentration is not more difficult to measure than N₂O (at least we have not seen it stated in literature as such). N₂ flux is difficult to measure, but we solved this with the intact soil core technique. We will provide detail on the N₂ concentration detection and its calibration.

Line 138: Give equation.

Will do that.

Results and Discussion

Line 154-159: Can the water table change be illustrated graphically along with the actual fluxes of CO₂, CH₄, and N₂O?

Yes, we will do that.

Line 155: 'slope forest' is not the same site name as specified in the methods. Same for 'palm swamp'.

We will change the text for consistency in the site names.

Line 165: dry station? I assume the authors refer to the toposequent stations? This needs to be clarified in the method section.

We will name the toposequent transect points in the Methods section.

Line 166: which site is the 'young swamp forest'?

This is the Slope site. We will change the statement consistency in the site names.

Line 167: Why does the dry station represent the optimal moisture for soil respiration?

Both dryness and wetness (lack of soil oxygen) may curb soil respiration. The dry point had intermediate soil moisture and showed the highest respiration rates.

Line 172: Which site is the swamp peat?

It is "The Swamp peat ...". We defined the sites in lines 76–78. According to that, the Swamp was "a natural forest in the Quistococha lake floodplain".

Figure 4: Not been referred to in the text. I guess this should be done in line 173.

We will correct that.

Figure 5: Why not CO₂ flux instead of soil respiration. That would be more in line with the other fluxes.

'CO₂ flux' would be wrong as our opaque chambers exclude photosynthesis and only represent soil respiration.

Line 176: I assume the authors refer to the eddy covariance technique.

Yes, will specify that.

Line 180: prevail --> dominate

We will change that.

Line 188-194: 6 lines of discussion for CH₄ only. This needs to be extended.

We will do that.

Line 195-198: Sudden switch to N₂O fluxes with mentioning of soil respiration. Hard to follow the discussion.

The switch follows a paragraph break, so it is hardly more sudden than the switch from CO₂ to CH₄. However, we should probably emphasise the paragraph break more. The surge in soil respiration caused by the water-table drawdown should probably be introduced in the CO₂ paragraph.

Line 200: From what do the authors conclude at this point that N₂O was produced from NH₄?

We stated in line 149: "The waterlogged swamp peat did not contain a detectable amount of NO₃⁻." This leaves only NH₄ and its non-NO₃ products as potential sources of N₂O.

Line 211-227: Very speculative paragraph.

Not sure what would be a more adequate discussion of the N₂O fluxes, soil NH₄, NO₃, water and O₂ content measurements and candidate mechanisms behind the N₂O fluxes listed in the literature, including earlier corroborating studies at the same site.

Line 235: toe-slope swamp forest? Same as site 'slope'?

Yes. We will standardise the site naming throughout the text.

Line 238: Are there any other papers which investigate N₂O reduction to N₂ in tropical systems? What are the implications for the N cycle?

No, to our knowledge not. We state the main implication for the N cycle in line 241: "Thus, the N₂O likely produced from nitrifier denitrification in March was

consumed by denitrification.”

L251: Upscaling paragraph lacks info on CH₄.

Following the suggestions of the other two reviewers, we will remove the upscaling paragraph altogether.