Biogeosciences Discussions

Biogeosciences Discuss., author comment AC2
https://doi.org/10.5194/bg-2021-46-AC2, 2021

Reply on RC2

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-AC2, 2021

I have now reviewed 'High greenhouse gas fluxes from peatlands under various disturbances in the Peruvian Amazon' by Pärn et al.

This study discusses results from monitoring GHG in peatlands under various land-use systems, in the Peruvian Amazon. The issue that is raised in the paper is important and timely. Anthropogenic pressure is increasingly threatening natural peatland systems, with potential important outcomes for the regional C and GHG balance.

Thank you for the thorough review and recognition of importance.

However, I'm left a bit disappointed after having read the paper. There are too many issues and unclarities to recommend this paper for publication. And I must say that I have not enough information to assess the scientific validity of the version (see the major issues below). In my opinion, this paper needs to be rewritten to be acceptable anywhere. I have tried to give a list of 'constructive' comments below, which might help in this process.

Thank you for the comments, we will rewrite the MS accordingly.

Important, and a bit less constructive I'm afraid, is that the dataset will remain limited. To me, it seems more like an exploratory dataset that could go in a proposal for more detailed work on this, or for a kind of perspectives piece somewhere, rather than a full research article. You cannot really go to comparing the anthropogenic impact on full GHG balances with only two systems

We acknowledge the limits and the exploratory nature of our dataset. We have not claimed However, we feel that the near-simultaneous comparisons of sites under various land uses, and the hot moment of N2O emission in our observations are important and timely enough to be published in an interactive open-access journal like Biogeosciences. We will certainly tone down the generality of conclusions and try to keep within the limits of our observations.

(I don't consider the slope site as included, only two days of measurements – we have to draw the line somewhere…)

We agree that the Slope site includes the same number of observations as a

subset of the other sites, and thus does not carry the same weight as the other sites. We do not rely on any time-series analysis of the Slope site. We already excluded the Slope site from a number of analyses, such as the one presented in Figure 6. However, as the PC plot (Figure 3) shows, the Slope site sits between the Swamp and Manioc sites as expected. Within the site, soil respiration follows the water table depth, as expected. Thus, it is fair to say that despite the low number of measurements and a lack of a dimension of time, the site, rather than creating erratic noise, complements the land use gradient from a pristine swamp to an intensively used agricultural field.

with minimal data (a handful of measurements

We agree that the data are insufficient for annual GHG budgets, and we will remove all statements implying those.

during half of a hydrological year, mainly confined in one season?).

We did not time our study period to any season, nor can it be confined in a single season. The start of our study period in September 2019 is most accurately described as the end of the dry season. However, the ensuing rains only lasted up until late December. The area received hardly any rain from January till March. We do agree that an annual upscaling of GHG fluxes from this rather atypical weather is a stretch. We will remove the results of the upscaling.

You also cannot really go to mechanisms, since the in-depth work is missing a bit…

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.

The study sites are not well enough described for an informed reader to understand:

We will be happy to elaborate the study site description.

- I work on GHG balances of tropical ecosystems, but not specifically in peatland complexes. However, I did not understand the seasonality (or lack thereof) of inundation

As explained above, it would be confusing to explain seasonality in this study period. The start of our study period in September 2019 is most accurately described as the end of the dry season. The ensuing rains raised the water table to the ground level but only lasted up until late December. The area received hardly any rain from January till March, during which the water table remained close to the ground level. We will include the water table dynamic in the site description.

how the disturbed systems were drained (or not) before they were planted with agricultural crops. This really impedes the understanding of this paper, even for readers with interest and expertise in this topic.

The Slope and Manioc sites were not ditch-drained. The main factors that

effected their drying were their location on slopes, the slash and burn of forest and conventional hand-hoe tillage. We will provide information on the land-use history in the site description.

For me as a reviewer: you measured only from September to March (approx.. half a year) – it is unclear whether this encompasses a certain season

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whether your upscaling to a year makes sense.

The idea of the upscaling was to put our numbers into the context of the extent of peatland area potentially under threat. Indeed, the study period did not cover typical seasonality, but it did involve broadly the same alteration between showers and dryness as a normal year in the area. As a bottom line, we propose to remove the results of the upscaling from the manuscript and find other ways to provide context for the results.

I actually looked up the seasonality myself: you monitored the rainy season if I'm correct?

September to March would be a rainy season in a normal year, but hardly any rain fell from January till March. Therefore, it would be misleading to call it a typical rainy season for the Peruvian Amazon.

Than why not the dry season as well? And how would that affect your conclusions??

We did not design our study to represent a typical or any year. A main assumption of our study was that the GHG result from the immediate environmental conditions. Thus, apart from issues with the upscaling, our conclusions remain valid.

- Additionally, you write in L 85: "on the slope and manioc sites, we installed three toposequent stations … ". Some more info is needed here: what was the exact setup.

We will provide landscape profiles to the study site description section.

And moreover: what with the forest sites? No toposequent stations? So how was the setup there, then?

The Swamp site was located in perfectly flat terrain. We established the chambers in no particular sequence to any topographic features. We will declare that in the site description.

Other major issues

The build-up and communication of the key message of the manuscript is problematic. The introduction actually shows this well: 1) Peatlands are important C stocks, 2) tropical peatlands vulnerable, 3) tropical peatlands are vulnerable, 4) very important for N2O, 5) diving into the mechanistics behind the N2O, 6) Amazon basin important for N2O globally, 7) again mechanistics in peatland N2O, then ending the paragraph with 'Brazil is also a major contributor to the global increase in N2O emissions during the last decades, owing to the increase in N fertilization'. Then the next paragraph you continue suddenly on the C sequestration in the peat GHG balance. You go on about microbial C respiration and conclude than that drought-induced tree mortality is saturating the Amazon C sink. The tree dieback described in Hubau et al. (earlier shown by Brienen et al. in 2015) has nothing to do with a positive feedback loop of microbes that acclimatize to rising temperature. Also the link with this 'drying' of forests themselves through el nino effects and then your 'human disturbance' is not clear to me. Do you want to look at climate change effects on the peatland forests, or do you want to focus on the effects of converting forests to agricultural fields? Not clear. While many of the statements might be factually correct, it doesn't necessarily set out a comprehensible intro for the reader.

Thank you for pointing out the inconsistencies in the text! We will order the statements more logically and remove the ones not directly relevant to the main points of the paper.

- Your end of your intro sets out the objectives: to fill the knowledge gap and to identify environmental drivers of GHG fluxes across gradients of land use and water table. I don't see this reflected in the set up: if we forget about the slope site (only two measurement days), you only have two systems, so we can hardly call this a gradient. It is completely unclear how comparable they are in there topographic setting: is the disturbed field like the forest site, but then converted?

Generally – yes, both are on a peaty soil and practically every aspect of ecological difference between the two sites is caused by the slashing of forest, burning and agricultural use. We will describe the land use history in the study site section.

Is the water table at the same level in both?

As the extremely different soil water contents (0.8 m^3 m^{-3} vs 0.15 m^3 m^{-3}) indicate, the water table was close to the ground in the Swamp site while it was about 1.5m below the ground in the Manioc site. Before the slash-and-burn agriculture, the water table was, on average, at the same level

Furthermore: how don't really identify the environmental drivers at play, right? You just measured all of them, but did not really quantify the importance of one vs. the others in governing the GHG balance? It's more that you look at some bivariate correlations and explain those, rather than to work with the full set of explanatories you have at hand.

We did test the correlations between the greenhouse gas fluxes and all environmental characteristics, both within and across the sites. We only displayed the significant correlations and, to tighten the communication, we did not display the insignificant correlations. However, to quantify the relative importance of the significant correlations, it is probably best to show the correlation matrices.

- I'm in general not against having a joint 'results and discussion' section, but in this case it becomes all very unclear. One (out of many) examples: in L154 you write 'that nutrients may have enhanced heterotrophic CO2 and N2O production', while at this point you did not report anything about the fluxes themselves yet.

We agree to move the discussion to a separate section.

- The language needs to improve, see some specific examples below.

We will ask a native English-speaking editor to proofread the manuscript.

Methodological unclarities:

- You installed the collars: but it is unclear whether these were installed once, and then re-used throughout the monitoring period? Or re-installed every time? Did you allow the collar to 'equilibrate' for a couple of days after installation? It has been shown that right after installation you disturb the soil enough to boost mineralization..

We installed the collars twice: early September and early January, and allowed a stabilisation time of several days before the sampling. We will specify this in the Methods section.

- For your He-O2 method: ig will be important here what you use for soil moisture levels in the incubation. You state the 'flushing depended on the soil moisture', but I don't see how you set the moisture level? Did you just take the moisture from the soil as it was sampled?

Our statement 'The flushing time depended on the soil moisture.' is just an *a posteriori* observation on how long it took to establish the new equilibrium in the intact soil core, i.e. to replace the air inside (mostly containing N2) with the artificial gas mixture. Therefore, we did not set the moisture level, we just replaced the soil air and report that the flushing time was longer in the wetter soils.

How long between the sampling and the incubation (also important for N depletion etc.).

Transport and storage time after the sampling was approximately a week.

- Did you overpressurize when transferring the gas samples from the chambers to the 50ml glass vials? Not doing so will likely introduce dilution effects when transferring the sample to the GC…

We did not overpressurise but our GC system does is fully sealed and under vacuum, thus excluding any dilution with lab air. The GC demand of gas is 25–30ml, which is half the volume of our 50ml sample. The vacuum pulls the gas sample into the sealed system.

- How did you 'observe' water table height in the observation wells? Just visually?

We observed water table height using a tape measure. We will specify this in the Material and Methods section.

- L 115: a peat sample was collected from each chamber after the sampling sessions in September and March à do you mean after each session? Does this mean that you reinstalled chambers at every sampling occasion? Cfr. Comment above: then your fluxes would likely be affected by the disturbance of forcing a chamber collar in the soil.

As we state in the Material and Methods section, we collected soil twice: "A peat sample of 150 to 200 g was collected from each chamber between 0 to 0.1 m depth after the sampling sessions in September and March." We installed the collars twice: early September and early January, and allowed a stabilisation time of several days before the gas sampling. We will specify this in the Material and Methods section.

- Slope monitoring: so 4 'sampling' events, clustered on two consecutive days. So basically two days of monitoring. Same day measurements are obviously not independent and temporally autocorrelated (you also need the statistical tools to deal with that in your correlations, correlation and GAM assume independent samples). I'm sorry, but that is really too limited to go to a GHG balance. More general: the monitoring took place on a different amount of days in the three sites, and on different time points. This would be "ok-ish" to go to inter-site comparisons if you would have a lot of measurements, but with the limited sample set, I don't see how you can scientifically justify these comparisons.

Thank you for the critical evaluation of our Slope sample. Not sure, though, whether the gas samples from the same site on different months are statistically independent from each other and whether that is an essential problem. We acknowledge that the monitoring of the Slope site was short, and the data can only be used to compare with subsamples of similar extent, such as the September observations of the Swamp and Manioc sites (like in Fig. 7).

- Especially not if your manuscript conclusion is 'Our study shows that even moderate drying in the Peruvian palm swamps may create a devastating feedback on climate change through CO_2 and N_2O emissions.". That's just a dramatic overstating of your data.

We agree that this statement tried to summarise too many results into one general statement. We will remove this statement and add sentences that are more directly limited with our results.

- It's not even clear what you mean by that: do you mean the agricultural vs. forest site comparison? I guess not, since the N_2O and CO_2 fluxes are in the same range there? So it must be that you mean the drying of the forest site itself? But I do not see data to support that statement? All unclear to me, after having spent quite some time with this manuscript, and that is not how it's supposed to be unfortunately.

This statement encompassed two observations: 1) The Manioc field retained high CO_2 and N_2O emissions after the conversion (which itself caused high but unmeasured CO_2 emissions from burning, and prevented CO_2 sequestration in trees); 2) We link the high CO_2 and N_2O emissions from the Swamp forest with the seasonal water table drawdown. We agree that the points can be communicated better and will try to break them down better.

Some specific comments, but not exhaustive I'm afraid:

- Title: 'High' relative to what??

Substantial comment. What we meant is high relative to sequestration or zero balance. However, probably it is better to remove this adjective from the title.

- L19: 'remove' large amounts of CO_2 -> you make it sound as if the flux into the system is exceptionally high, while it's my understanding that it's mainly the stock that is high. So 'store' would be better here.

Agreed.

- L27: retaining their high CH_4 -> rephrase

Agreed.

- L38: undisturbed peat swamp forests sequester carbon for tens of kyr. Do you mean:

have been sequestering carbon for the past millennia?

We mean thousands of years but the policy of Biogeosciences is not to use words for numbers but to use k for thousands. However, if that is misleading, we will be glad to use millennia instead.

- L52: unclear: the amazon has an exceptionally high 10% share of nitrification in N2O production. Do you mean that 10% of the produced NO3 is further emitted through N2O?

The 'high 10% share of nitrification in N2O production' means 10% of the N2O is produced in nitrification. Denitrification and other processes are responsible for the 90%. However, it may be that this point is too specific for the introduction.

- L60: a quickly increasing disturbance à not proper English.

We will let a native-speaking editor to proofread the manuscript.

- L62: where is your reference for 'droughts increase ecosystem respiration'? Kind of a general statement as well, no?

For us, the fact that soil drying and warming increase ecosystem respiration is textbook material. We will cite a reference for that.

- L63: explain what you mean with that positive feedback loop for the reader, please.

Agreed.

- L106: how many datapoints did you set to 0

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.

- L106: you should add a statement on why you would use a linear, and not a quadratic, fit.

Agreed. The linear fit is the only one that does not assume either saturation or quasi-exponential rise in concentration.

- L165: 'the dry station' à do you mean the slope? Not clear: be consistent in your naming of your sites.

'The dry station (water table –0.7 m; soil water content 0.26 m3 m–3; soil temperature around 26 ºC at 10 cm depth) of the young swamp forest' is the dry station of the Slope site. We will correct that.

- L167: that station represents the optimal soil moisture: you cannot say this. You make a relative comparison here, while optimal would be on an 'absolute' basis.

The point here is that we observed the highest soil respiration at this moisture, not the driest one.

- Figure 6: do you really need to show the P-value until 8 numbers after the decimal?

We can just state '$p<0.01$' but not sure whether that would convey the same

information.

- L208: when you make a comparative statement like this, it would be good to also give those numbers to the reader. ‘Agreed with huge N2O emissions from floodplains’ à how high where those ‘huge’ fluxes.

We will provide the figures from the earlier papers.

- L230: consistently use N2O-N please.

Will do that.

- L213-227 is a long speculation of several potential reasons for the combined observation of low NO3 and low N2O. At line 224 the authors write ‘third’, while this is already the fourth potential reason. This whole section is speculative and can be shortened in my opinion.

Agreed. We will shorten that paragraph.

- L235: where is the toe-slope?

We will specify that.

- L233:manioc field: be consistent in the naming

Agreed.

- L260-261: very strange sentence at this spot.

We will remove it.

- L 263: please be consistent in the naming of your different systems. What do you mean with arable peatland? The agricultural fields? Use this throughout the manuscript.

Agreed.

- L266-267: high nitrifier denitrification while suppressing the full denitrification pathway à strange formulation: you show high N2 outgassing in your earlier section?

N2O is the intermediate product of denitrification. High N2O emission with high denitrification potential itself is evidence of incomplete denitrification. The N2 potential in the Swamp measurements was high in September compared to the other, dry sites but it was modest compared to the long-inundated March samples with low N2O emissions.

- Also: you actually don’t show nitrifier denitrification. You list a number of potential mechanisms and many rely on earlier work to say that it is ‘likely’ nitrifier denitrification. You would need tracing or isotopocule data to infer that.

A fair point. We agree to remove the statement from the Abstract and Conclusions and shorten the discussion in lines 211–226. 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. In addition, Hergoualc’h et al. (2020) identified nitrifier denitrification as the main N2O production mechanism in the site. Therefore, we will keep nitrifier denitrification in the discussion in lines 211–226.