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Comment on bg-2021-46

Anonymous Referee #2

Referee 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-RC2, 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.

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. 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 (I don't consider the slope site as included, only two days of measurements – we have to draw the line somewhere…), with minimal data (a handful of measurements, during half of a hydrological year, mainly confined in one season?). You also cannot really go to mechanisms, since the in-depth work is missing a bit…

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

- 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, or 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. 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, and whether your upscaling to a year makes sense. I actually looked up the seasonality myself: you monitored the rainy season if I'm correct? Than why not the dry season as well? And how would that affect your conclusions??

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

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.
- 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? Is the water table at the same level in both? 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.
- 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.
- The language needs to improve, see some specific examples below.

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..
- 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? How long between the sampling and the incubation (also important for N depletion etc.).
- 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…
- How did you 'observe' water table height in the observation wells? Just visually?
- 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.
- 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. 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 CO2 and N2O emissions.". That's just a dramatic overstating of your data. It's not even clear what you mean by that: do you mean the agricultural vs. forest site comparison? I guess not, since the N2O and CO2 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.

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

- Title: 'High' relative to what??
- L19: 'remove' large amounts of CO2 -> 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.
- L27: retaining their high CH4 -> rephrase
- L38: undisturbed peat swamp forests sequester carbon for tens of kyr. Do you mean: have been sequestering carbon for the past millennia?
- 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?

- L60: a quickly increasing disturbance à not proper English.
- L62: where is your reference for 'droughts increase ecosystem respiration'? Kind of a general statement as well, no?
- L63: explain what you mean with that positive feedback loop for the reader, please.
- L106: how many datapoints did you set to 0?
- L106: you should add a statement on why you would use a linear, and not a quadratic, fit.
- L165: 'the dry station' à do you mean the slope? Not clear: be consistent in your naming of your sites.
- 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.
- Figure 6: do you really need to show the P-value until 8 numbers after the decimal?
- 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.
- L230: consistently use N2O-N please.
- 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.
- L235: where is the toe-slope?
- L233:manioc field: be consistent in the naming
- L260-261: very strange sentence at this spot.
- 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.
- L266-267: high nitrifier denitrification while suppressing the full denitrification pathway à strange formulation: you show high N2 outgassing in your earlier section?
- 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.