Biogeosciences Discussions

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Comment on bg-2021-46

Yit Arn Teh (Referee)

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-RC1, 2021

GENERAL COMMENTS

This is a topically relevant paper, given how little is known about peatlands in South America, and growing interest in understanding how tropical peatland management worldwide influences regional and global exchanges of greenhouse gases. This paper adds to the growing body of knowledge on the biogeochemistry of South America peatlands, and nicely integrates field-based flux measurements with more process-based laboratory assays. However, while I am broadly supportive of this paper, I think that the paper needs to be revised in order to enhance reader understanding and to acknowledge potential limitations with the experimental methods and design.

First and foremost, given that this study is of limited geographic scope, with data collected over a relatively narrow window of time (i.e. only 6 months of measurements), the authors need to do more to acknowledge that it may be difficult to generalise their findings to larger spatial domains or longer periods of time. For example, I was concerned that the authors were over-reaching when they extrapolated their data over the entire Pastaza-Marañon foreland basin region (see point 10 below).

Second, the authors need to clarify for non-expert readers if their measurements captured both wet and dry seasons (see point 4 below), given that water table depth and other environmental conditions vary substantially between wet and dry seasons, with a strong wet/dry season signal in CH4 and other trace gas emissions detectable not only from

Amazon-basin wide studies of atmospheric chemistry and from smaller scale, site level studies of ecosystem gas exchange (e.g. Wilson et al. 2016). Seasonal affects will have ramifications not only for their field data, but could also influence their incubation results, as water table and other environmental conditions will influence the status of the microbial community at the time of soil collection; e.g. soils collected for incubation during the wet season could have a different activity profile or functional composition from the same soils collected during the dry season.

Third, the cold storage of soil for incubations is problematic and could lead to significant treatment effects (point 8). Historic studies by Louis Verchot, Marife Corre, Ed Veldkamp and others have demonstrated adverse impacts on N cycling microbes (relevant to this study given it's focus on N2O), and many tropical research teams now transport soils at room temperature or conduct laboratory experiments near their field sites to avoid these treatment effects. While potential treatment effects do not invalidate the incubation studies presented here, the authors need to acknowledge the potential issues caused by cold storage and discuss how this may impact their interpretation of the results.

Fourth, on a more technical point, the authors need to clarify how they treated non-linear data and chambers that show potential evidence of ebullition. Fitting linear curves to non-linear data or excluding ebullition data will tend to underestimate flux rates.

Specific comments are provided in the section below.

SPECIFIC COMMENTS

- Lines 70-72: Since this study only investigated a sub-set of land-uses in the region, it would be clearer and more transparent if the authors indicated here which land-use types they focused on in this paper, with a brief justification for why they have concentrated on these land-uses in particular.

- Lines 76-80: For readers unfamiliar with the Roucoux et al. (2013) paper, I recommend expanding the site description for the human-affected sites so it's clearer how human intervention has altered these study sites.
- Lines 91-93: Please clarify how many samples were collected over a 60 minute period; i.e. 4 time points (0, 20, 40, 60 minutes) or 3 (20, 40, 60 minutes).
- Lines 95-96: Did these sampling campaigns cover both wet and dry seasons? This is not clear - please clarify this in the narrative. Also indicate in Table 1 what season the campaigns were conducted in so it's clearer if there was even sampling between seasons.
- Lines 101-102: How was CO2 determined? Did the instrument have a methanizer? Also – N2 flux is mentioned in the Results and Discussion section, but it's not clear how N2 was measured in the field flux measurements – this must be clarified.
- Lines 102-104: How were non-linear data treated or chambers which showed evidence of ebullition (i.e. erratic or very large non-linear changes in concentration)? Were these data discarded or included?
- Line 106: Treating non-detectable fluxes as zeroes (rather than as a "n/a" or flux at the limit of detection) is a judgement call, given that there is a line of reasoning which argues that treating these data as zeroes biases your dataset towards zero values, when in fact these data points may be producing/consuming gases below the limit of detection. I recommend that the authors provide some justification for this judgement call, given that this is a non-trivial decision.
- Line 116 and line 128: It is important to recognise that storage of tropical soils at low temperatures can negatively influence soil microbial communities and microbial activity, given that tropical microorganisms are not cold-adapted and can be severely impacted by storage at sub-ambient temperatures. There is quite a long history of research on this topic, and I recommend that the authors familiarise themselves with the peer-reviewed literature on this topic. The search string "cold storage tropical soil microbial activity" in Google Scholar produces at least half a dozen relevant references, including historic papers by Verchot (1999) Soil Sci Soc Am J and Arnold et al. (2008) Soil Bio Biochem on the effects of cold storage on N cycling in in tropical soils.
- Line 201: Mention of DNDC at this point in the narrative comes from left-field, since DNDC and modelling were not discussed before this. It's not clear from the narrative if the authors used DNDC in their research or if they are drawing on findings from modelling studies to interpret their findings. This needs to be clarified as it is confusing.
- Lines 251-255: Given the small geographical and temporal scope of this study, I think that the authors are over-reaching when they upscale their fluxes to the entire basin. While these kinds of back of the envelope exercises are interesting and important for progressing the discussion, I think the authors need to be more circumspect about the claims they are making. In this instance, I recommend that the authors revise these sentence construction to make it clear that these numbers are highly speculative and represent first order estimates to gauge relative importance. To be clear, I'm not necessarily saying that the authors should remove these calculations, but rather they should change the language so it's clear that there estimates are a speculative exercise, rather than certain predictions of the emissions potential of the basin.
- Lines 147-261: With respect to reporting fluxes in the body of the text, and I recommend that the authors make it clear when they are referring to field data or incubation data, given that results from laboratory incubations are often not directly comparable to field measurements because of differences in measurement scale, methodology, different handling/treatment effects, and problems of comparing open system (field) versus closed system (laboratory) measurements. The paper as it is currently written doesn't clearly distinguish between data from these two different types of studies.