

Interactive comment on “Complex controls on nitrous oxide flux across a long elevation gradient in the tropical Peruvian Andes” by Torsten Diem et al.

Anonymous Referee #1

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In 2014, some of the authors of the present publication published in Biogeosciences (doi:10.5194/bg-11-2325-2014) a paper entitled “Methane and nitrous oxide fluxes across an elevation gradient in the tropical Peruvian Andes”. It was a very interesting paper because there is only little information about soil nitrous oxide fluxes and their controls in tropical montane forest soils. In their one-year study they pointed out that nitrous oxide fluxes were primarily driven by denitrification and that nitrate availability was the principal constraint on soil nitrous oxide fluxes followed by soil moisture. In the present study Diem and colleagues extended their time-series to multi-annual time scales to identify controls of longer-term climatic variability, soil moisture and substrate availability on nitrous oxide fluxes in greater detail. They found out that habitat/elevation

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site, a proxy for nitrate availability under field conditions, was the best predictor for nitrous oxide fluxes. It is a great study. I have only few suggestions.

Major suggestions:

I would suggest to reformulate the introduction and the hypotheses. The main message is that habitat/elevation – a proxy for NO₃ availability in the field – is the best predictor for N₂O flux and that seasonal differences of N₂O flux and environmental variables were most pronounced at the lower montane forest site, where N₂O flux was best explained by a combination of temperature, WFPS and N-availability. I would remove substrate availability and/or labile organic matter because it does not enrich the discussion but rather blur the main message. I think it is sufficient to discuss an absent correlation between N₂O flux and variations in leaf-litter fall in one or two sentences and not in a whole discussion section (L827-L843).

At the moment it seems that results and discussion section are dominated by the description and interpretation of the experimental results in the lab. I am very sceptical whether the results from the laboratory-based nitrogen and WFPS manipulations can be directly linked to the results obtained in the field, especially when they are as puzzling and surprising as in the present study (i.e. WFPS-manipulation study). Substrate availability, nutrient limitations and a cascade of active microbial community composition may have drastically changed during transportation from the field site in Peru to Aberdeen. As long as there is no clearer picture about the active microbial community in the samples before and after transport, all of the nutrient and trace gas flux observations during incubation experiments have only potential implications. Additionally, the ratio of N₂O to N₂ production is pH-dependent. Did you check for potential pH changes upon transportation?

What I find more fascinating is the observation of a negative relationship between WFPS and N₂O flux in the field. The authors suggest that increasingly anaerobic conditions may stimulate N₂O reductase activity and lead to greater denitrification to N₂.

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This strengthens the assumption of Mueller et al. 2015 who suggested that gaseous N loss was likely dominated by N₂ rather than N₂O in Ecuadorian montane forest soils. Taken together, this finding may be generalized to tropical montane forest ecosystems.

This leads me to another suggestion. Many parts of the discussion section read like a repetition or better description of the results section (e.g. L740-L760; L814-L818; L851-L858; L869-L876; L881-L891). Moreover, the links between different parts are laborious (e.g. L730-L734; L751-L755; L784-790; L880). I think it is necessary to make the reading more “fluid”. Many sentences in the results and discussion section begin with “For example” (e.g. L534, L620, L689, L745, L814). I think the discussion section would benefit if present results would be more interpreted in the light of recent publications (e.g. Baldos et al. 2015; Mueller et al. 2015; Nottingham et al. 2015).

Minor comments:

L45-L48: This should also be mentioned in the conclusion section L98: ...derived from (missing word) L290: What is the sampling size of the background concentration measurements? L300: What was the length of time between sampling and analysis? L827-L843: Remove heading and shorten section. L880-L900: Does this section really enrich the discussion? L906-L907: “Nitrous oxide flux originated primarily from nitrate reduction rather than from nitrification, probably due to low pH soil condition”. Influence of pH has not been discussed in previous sections. L912: It should be clearly stated whether results were obtained from incubation experiments or from the field. Table1, Figure 3: Table and figure are very difficult to read. May be you can upload tables and figures in a higher resolution.

References: Baldos et al. 2015 (DOI: 10.1890/14-0295.1) Mueller et al. 2015 (DOI: 10.3389/feart.2015.00066) Nottingham et al. (DOI:10.5194/bg-12-6071-2015)

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