Biogeosciences

Discussions

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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 #2

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The authors address the complex issue of N2O emissions that is globally, even more for tropical forests, and particularly for montane tropical forests widely unconstrained. The experimental setup in the field and in the laboratory were designed to capture mechanisms that affect N2O production and emissions. These effects include soil moisture, substrate availability (both mineral nitrogen and labile organic matter), soil moisture, oxygen, and temperature. They further analyzed more indirect predictors such as biome type, topography, seasonality, year to year variability - as well as interacting effects among these potential drivers for N2O production. The major outcome of this study is that the controls on N2O emissions remain elusive and in parts counter existing knowledge. In particular, the study finds little seasonal variability despite strong

seasonality in wetness. Further, soil moisture experiments suggest not the straight-forward controls as they are being used in conceptual and numerical models. The exhaustive work done in soils in difficult and previously unsampled environment, as well as (in my view) important laboratory experiments that complement the field work. The data deserves dissemination to the scientific public. However, I do have some suggestions and comments on the presentation and interpretation of the data.

Organization: The sheer number of observations and experiments, the exhaustive statistical analysis makes, and the resulting (complex pattern) makes it hard to write a clean story. Yet I think the authors should give the presentation some more thought. The result section is full of statistical test results, I am wondering if the tests applied and their results would not be better confined to tables, while the result text focuses more on the most important patterns.

Hypotheses: I would love to see a bit more nuanced hypotheses: Teh et al., 2014 already show an "odd" relationship with soil moisture (i.e. unexpected highs during dry season compared to wet season). Could better hypotheses be developed based on this earlier data? In light of previous work done at the site, H1 and H2 are fairly generic. Similarly, since the paper also addresses elevation gradients (or transitions from premontane tropical forests to montane grasslands, perhaps there are potential to use that gradient to set up additional hypotheses (What are expectations if compared to [seasonally dry] lowland tropical systems?).

Seasonality: Looking at the time series, it seems to me from the get go there is no direct seasonal effect. However, there are curious seasonal patterns: Soil moisture seems to lag quite a bit the precipitation (i.e. soil moisture seems to increase at the beginning of the dry season before it diminishes, while soil moisture continues to decline after the onset of the wet season). Much harder to discern, but just eyeballing the data in Fig 3, it seems there is a seasonal pattern of N2O emissions that it out of phase with seasonality, and is also out of phase with soil moisture. I do not have a mechanistic explanation how such lags can be formed given that often the first rain

leads to strong pulses in denitrification. Nor do I know whether the patterns I seem to recognize are really there if further scrutinized. Yet I am wondering if there should be some exploration with the inclusion of lag in the analysis. Perhaps the authors toyed with it and did not pan out, However, I would be curious to know either way.

Bimodal soil moisture response: The authors put strong emphasis on the bimodal soil moisture response of N2O emissions with peaks at 90 % and 50 % water filled pore space – stating it both in the abstract and the conclusion. However, this is in my view not clearcut, occurring only in some of the sampled soils. The results and the discussion acknowledge this. Is there a way to nuance the abstract and conclusion, such that the result do not come over as overstated?

Gradient nitrogen-rich -> nitrogen poor. In several places there is mention that the premontane and the lower montane habitats are nitrogen rich, whereas the higher elevations are considered nitrogen poor. It is perhaps worthwile to define N rich and N poor explicitly (for example by resin bag mineral N). This seems to be very important, given that nitrate availability may be a strong driver for N2O production.

Yet Figure 2 suggest that with respect to N2O emission, only the lowest forest has significantly higher emissions. But the authors also imply in some places (including in the abstract) that there is a continuous gradient in N2O emissions. Is this in conflict with each other (Although probably having altitude as predictor may lead to statistically significant N2O gradients)?

Detailed (and minor) comments

Abstract L31: The statistical analysis does not show such a gradient, rather premontane forest was had much higher emissions than the rest (Figure 2). This may be a bit nit-picking on my part (I can see that the average in the lower montane forest is higher, but also has higher variability). Perhaps regress against altitude?)

Abstract L40: Is the sentence starting with "This bimodal.." is a bit empty, not add much

information. What is the complex relationship, what environmental variables?

Abstract L45: I think somewhere in the main text – perhaps discussion – it should be better laid out and evidenced that habitat is a proxy of NO3 availability.

L 95: check spelling "areally"

L 98: Sentence starting with "Nitrous oxide": the use of parenthesis seems odd.

L 104: Check the sentence – placement of "for" in the next line seems odd.

L 152: I like how the authors also analyzed topographic landforms. However, throughout the paper it is not clear, how these landforms were binned and weighted to form a habitat-wide data sets. Also, where were the samples taken from for the laboratory manipulations? Further, can the terminology be kept a bit more consistent? Throughout the manuscript, it is referred to as topography, landscape feature, landform, and basin landform. I assume they are all the same, but I suggest to use a consistent designation for this categorical variable.

L250: This sentence essentially repeats the statement in L240

L260: I assume the amount of litter added corresponds to the amount of litter falling in 1 month?

L483: Did you test for oxygen as a predictor, or was oxygen only assessed one time?

L506: >24 hour incubation: Over what period were the fluxes averaged?

L667: Again, how long is the >24h period?

L726: The figure shows that premontane habitat is significantly different from the other, and not that the lower elevation forests (premontane, and lower montane forest) are significantly different from the higher elevation forests.

L835: check the sentence starting with "Moreover,. . ."

L859: This sentence is not clear. What do the authors mean by "This pattern"

L884: It is hard to believe that NO3 additions did not stimulate N2O emission. Just eye-balling Fig 5 suggests, it seems that N2O flux over the incubation period increased with increasing NO3 levels added. Is there some artifact because of the way the ANOVA has been done (admittedly this is a weak point on my part – but maybe a recheck and some explanation is possible to enlighten me and the readers)?

Supplementary figure: Please add the habitat to the x-axis for completion