Biogeosciences Discuss., 10, C8175–C8182, 2014
www.biogeosciences-discuss.net/10/C8175/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Methane and nitrous oxide fluxes from the tropical Andes" *by* Y. A. Teh et al.

Anonymous Referee #2

Received and published: 23 January 2014

In this work by Teh and colleagues, the results of a large scale study examining the role of soil trace gas fluxes of nitrous oxide and methane are investigated in the Peruvian Andes along an elevation gradient. The work represented is original, novel, and uses methods that are sound. Trace gas data are famously heterogeneous in space and time. Undaunted by this heterogeneity (or by landslides), the authors investigate trace gas fluxes across an elevation gradient, while replicating across space at each elevation, and time for all elevations. On the whole the writing is good, the findings are sound, and the conclusions are strong. I do have many suggestions, ranging from strong to more minor. I'll lay out the stronger suggestions first, then have a line by line highlighting of the more minor interspersed with specific reference to where the major ones play out.

Major suggestions:

1) While the writing largely serves the authors well, there are several things that would help the reader better see the patterns in the data as they read, or clarify concepts or methods. This includes things such as sticking either with elevation band or habitat as the descriptor (currently, one is used largely in the text, the other on the figures), not using both. It also includes simplifying the presentation of numbers by not overwhelming the reader with a sea of text, numbers, and units. It also includes correctly labeling columns in tables (currently, the wet season has drier soils than the dry season. . .which I think is a typo, not reality).

2) On the subject of methods, the O2 method is not adequately described here or in the cited paper. It sounds like there is a buried bottle that is sampled, or some such thing. Hard to know. This then makes it difficult to assess the meaning of the data. The O2 data seem a little strange. Those in 90+% WFPS have what would seem to me to be surprisingly high O2 concentrations, given what I imagine the C content of these soils to be.

3) While I applaud the appropriate and elegant use of stable isotopes to tease out potential production of N2O and N2 from these soils, I have some concerns about this data. While the patterns seem to make sense, my main concern is that the soil used is from below the rooting zone. I did not find clear evidence of why this soil was used, but given that denitrification requires not only nitrate, but also a supply of labile C , below the rooting zone is not where I would imagine the denitrifying hotspots for these ecosystems would be. Thus it seems like a big oversight of the experimental design.

4) While the overall results and findings of this study seem to be fairly compelling, the statistics should likely be redone. This point won't change the overall story, and may even strengthen the patterns. For the full details, see detailed comments below. For the highlights, the ANOVAs look like there are large differences in variance for the different elevation bands, and most of the data do not appear to be normally distributed.

Given the nature of the beast, this is not surprising. However, the data violates the assumptions underlying a parametric statistical analysis such as that employed by the authors. I would suggest using a non-parametric test instead. For the linear regression analysis, data are log-log transformed, yet even the transformed data do not appear to be normally distributed. Thus using a linear regression is not appropriate.

5) I think it would be nice if it was clear in the abstract and introduction that the data and extrapolation in this work is based on soil flux measurements made by chambers, then extrapolated up. Otherwise, it is very unclear until in the methods that this is what was done. Also the title makes it sound like it is just fluxes from, when in reality there are also fluxes of methane into soils at least in some seasons and elevation bands. While I don't know that more discussion of fluxes from water bodies, flux of gases through plants, production and release of CH4 in plants, etc. is necessary anywhere in this paper, I do think it would be good to make it extremely clear in the discussion that these results are for the soil fluxes. It is stated that for the sake of this study lakes were ignored because they are hard to study (or some such thing). What is reported in this study was a hell of a lot of work, and no easier than studying lakes. Still, might want to either more clearly delineate or rationalize why they are ignored for the sake of this study. Small portion of landscape? Other studies already cover that? Simply not the focus of this study.

Line by line minor and less minor comments and suggestions:

P17398, L17, Seems like this may not be the primary reference. Also, if it is in plants, is it abiotic?

P17401, L 15: I would love to see some maps. I envisioned a somewhat simple elevation gradient, but then in exploring on Google Earth, I found out that my conception did not match up with reality one bit.

P17403, L19: Would like to see some clarification in the methods here. Specifically, no detail is given about what a soil gas equilibration chamber consists of, and how it is

sampled.

P17404, L2: suggest replacing "avoid" with "minimize", as it seems one month is not necessarily long enough to avoid artefacts given the potential persistence of roots for longer than one month.

P17404, L14: Suggest changing "Fluxes rates" to either "Flux rates" or "Fluxes".

P17405, L15: Suggest replacing "rhizosphere" with "rooting zone", as the word "rhizosphere" is increasingly used to refer to the soil that is proximal to roots.

P17406, L1: Why look for denitrification below the rooting zone? Considering denitrification is driven by C availability, oxygen, and dissolved inorganic nitrogen, it seems like shallower soils would have an advantage for everything save oxygen.

P17407, L 4: Data were transformed for ANOVA, but what is behind the transformations used for linear or non-linear regression? Should be similar constraints (normal distribution), but the log/log transformation of O2 and methane flux

P17407 L21: May be just a matter of preference, but I would like to see either habitats, or elevation bands, not both here and throughout. I think table 1 is effective in presenting them and what they are, but then either stick to habitats or elevation bands throughout the remainder of the manuscript. Similarly, while it puna is convenient and shorter than montane grasslands, I would personally prefer the nomenclature to be montane grasslands. It is less specific, more descriptive, and more understandable to the uninitiated.

P17408, L1: Here and throughout the remaining and preceding pages, units are expressed for each number presented. I think it is much more understandable to do a comma separated list of numbers, followed by the units:

-0.16 ± 0.13 mgCH4 –C m-2 d-1, -0.64 ± 0.08 mgCH4 – C m-2 d-1 and -0.82 ± 0.08 mg CH4 – C m-2 d-1

Could become: -0.16 ± 0.13, -0.64 ± 0.08, and -0.82 ± 0.08 mg CH_4 – C m-2 d-1

Also, a negative "uptake rate" would be a release of CH4, wouldn't it? Consider just using flux, since the previous clause says that they are net sinks.

P117409, L1-18: What is the rationale for presenting both WFPS and VWC in the main text? This (for me) feels like it causes the sentences to be bloated, and the conveyance of information is much less informative. Consider making the statement that both numbers are presented in the table, but only present on set in the text.

P17410, L15: NO2- or NO3-?

P17410, L21: Consider changing g resin-1 to g-1 resin, as it is per gram of resin, not g per resin.

P17411, L11-L19: As I commented on earlier, negative uptake would be release. Consider changing terminology. Also, if a negative flux gets more positive, meaning less negative, that is an increase not a decrease. I suggest either discuss flux as positive numbers (seems like a bad idea), or talk about the fluxes as becoming less negative, representing a decrease in uptake. This is kind of a pain, uses more words, but I think it will help make this clearer.

In case it is not clear from my comments, I think this kind of precision in language is important. You see the same thing in the literature when discussing water potential, wherein increasing water availability is described as a decrease in water potential. However, given that water potential is a negative, it is actually increasing.

P17412, L8: Looks like table columns got mixed up.

P17414, L2: "both VWC and WFPS, respectively" suggests that two sets of stats are given, yet only one set is given. That being said, I think only one set of data needs to be shown here, and throughout the manuscript with the exception of the Table 3.

P17414, L12: Consider changing kg soil-1 to kg-1 soil, as it is per kg of soil, not kg per

soil.

P17415, L3: It is a gradient in the tropics, but it is an elevation gradient, not a tropical gradient. I think this could be clarified to say tropical elevation-gradient.

P17415, L18: Is poor drainage the factor, or low relief coupled with poor drainage?

P17419, L8: What about C? Are these soils high C, which would be expected to push denitrification towards N2 as well? They may not be; hard to know given only C:N ratios are given for these soils.

P17429, Table 1: Good information. Would like to also see soil C, bulk density, and texture (if they are all available) in this table. Bulk density is, as it was used to calculate WFPS.

P17430, Table 2: What are these rooting zone numbers based on? They seem pretty shallow, but not inconsistent with the shallow depths of some of these soils. Also, soil samples are reported in the text as having been taken from below the rooting zone, yet the soil sample depths indicate that with the exception of the Lower montane forestâĂŤsoil samples were taken from the lower range of the rooting zone.

P17431, Table 3: Looks like wet and dry season got mixed up. Also, these oxygen numbers seem a bit surprising. At 90+% WFPS, O2 is not even below 10%. Are the plants pumping oxygen down into the soil, or are these measurements just a proxy or index for soil O2?

P17433, Table 5: Here and in the text, it would be helpful to make clearer that these are the estimates from the terrestrial component of the ecosystems, and ignore the aquatic portion. While it appears that the aquatic component is not a major component of the surface area, I it would expect they would modify these numbers if they were included.

P17434, Fig 1: Looks like there are some pretty major differences in variance between elevation bands, and I would imagine that some of that is driven by the seasonal differences that are subsumed in these data. It also is apparent (especially in the methane from the Puna where the mean sits outside of the interquartile range) that these data are non-normal in their distribution, likely due to the inclusion of both seasons in this analysis. It seems like non-parametric ANOVA approach would be much more appropriate for this treatment of the data. Suggest redoing stats here, but check that I'm correct in this assessment.

Also, in all the figures, elevation bands are referenced. This leads to a disconnect with the text where habitat descriptors are largely used to describe the data. Consider revising one or the other to make it more transparent and comparable.

P17435, Fig 2: These look more reasonable, but still, non-normal and with a big range in variance. Suggest non-parametric ANOVA.

P17436, Fig 3: I am convinced that the pattern represented by this data is valid, but I am troubled by the presentation of the data, as well as by the use of linear regression to analyze and present the strength and magnitude of the relationship. Presumably, log transformations were used to normalize non-normal data. . .however, the distribution of X and Y under the log transformation looks very non-normal with a cluster of data at high O2 and low CH4, and a smattering of points in between there and low O2 and high CH4. This violates the assumptions of linear regression, and thus these statistics are invalid. Consider presenting the data as non-transformed. Might also be that there is a threshold of O2, and below that threshold there is a meaningful linear relationship if the data are presented in untransformed form.

P17438, Fig 5: From the text of the caption and the data, it is not clear to me what the comparison being made is. If the comparison made in the statistics is not within an elevation band, but rather within a given flux category, I think it would be clearer to present the data grouped by flux category.

Either way one views this, the stats results here look very strange. I realize that this isn't a totally helpful thing to say, but lacking the data to pore over, it is the best I can do.

Assuming the comparison is within an elevation band between fluxes, how can 3200-3700 have an N2O flux and N2O+N2 flux that are basically identical due to a low N2 flux, yet they are significantly different? At the same time, the N2O flux and N2 flux are hugely different, yet they are statistically similar?

Alternatively, assuming that the comparison is within a type of flux, the N2O+N2 flux comparisons are strange and troubling. Given that 600-1200 value is lower and with smaller error than 3200-3700, and given that both are much lower than 1200-2200, it doesn't make sense that 3200-3700 is significantly lower than 1200-2200 while 600-1200 is not.

Also, if the comparison is between different fluxes at each elevation, it is troubling that this ANOVA is a comparison between two component fluxes, and a third "observation" which is the sum of those two fluxes. As defined, the sets of observations being studied in an ANOVA need to be independent, thus a flux that is a linear combination of two other fluxes being compared violates this assumption.

Interactive comment on Biogeosciences Discuss., 10, 17397, 2013.