The manuscript BG-2016-393 titled “Soil trace gas fluxes along orthogonal precipitation and soil fertility gradients in tropical lowland forests of Panama” presents a comprehensive dataset of trace gas fluxes from the tropics and is highly recommended to get published. However, during the review of the manuscript several sections need major improvement before publishing.

General comments:

In your data several new aspects are visible, but not presented and discussed in an appropriate way:

(1) Since in tropical ecosystems soil moisture is highly variable, while temperature is fairly constant (can be seen in your dataset: while gravimetric soil moisture changed from 1.2 to 0.4,soil temperature changed from 27 to 23°C. In other words 66% change of moisture, while temperature changed 15%.). Based on that it can be expected that changing soil moisture is the major driver of trace gas emissions. However, in your study a co-correlation of soil moisture and soil temperature is discussed. This is highly interesting, but not yet well presented. You should be able to demonstrate that air temperature at your sites was fairly constant, therefore the most of the change in soil temperature should be attributed to co-correlation to soil moisture changes. Based on theoretical considerations (e.g. Q_{10} value) you should be able to give an estimate about how much of the change in CO_2, CH_4, N_2O emission could be caused by temperature only and by the combined soil moisture/temperature effect.

(2) The general “parabolic relationship” of CO_2 and soil moisture might be influenced by combining all data point from all sites. It seems actually that the emission follow more actual soil moisture than rainfall gradient. For a more comprehensive analysis, it might be helpful to include correlation coefficients for rainfall, soil moisture, soil temperature, NO_3^- and NH_4^+. Since in the whole paper all figures show data points with individual symbols for each site, it seems reasonable to use different symbols for each site (Fig3). Furthermore, is there a reason why N_2O is not shown in relationship to soil moisture? It might be helpful for a more process based discussion and the role of aerobic CH_4 oxidation coupled to denitrification in this soils? Predominantly the soils are a net-sink for CH_4, and you measured N2O and NO3 but did not discuss the coupling of processes yet (see e.g. Zhu et al. 2016 aerobic methane oxidation coupled to denitrification). It would be more appropriate to convert gravimetric soil moisture into either whc or WFPS to normalize somehow for the soils from different site.

(3) If soil temperature, soil moisture, and soil properties would dominate the CO_2, CH_4, N_2O, and NO fluxes, the data points (Fig.3) should result separate functions over time. The fact, that they are overlaying each other suggests, that other parameters, which are not yet discussed might affect CO_2, CH_4, N_2O, and NO fluxes. As such it should be discussed how abundance (and activity?) of functional microbial groups will change within the rainfall and fertility transect?

(4) Without any additional literature reference the transfer from Tamai et al., 2003 for methanotrophs to methanogens is hard to buy. In Tamai et al., 2003 a negative correlation between CH_4 uptake rate and Al was found. Table 2 shows that your inhibition might be possible for P8, P19, P32, but not for the others. However, these 3 sites show actually the lowest CH_4 fluxes in the rain season 2011 (Fig. 2). Shouldn't a correlation of net flux and Al result in a positive correlation if inhibition of methanotrophs based on Tamai et al., 2003 is assumed? If your assumption would be valid, how can you explain a simultaneous inhibition of methanotrophs which could cancel out your inhibition of methanogens? Since methanotrophs and methanogens are different functional groups of microbes, I think this is speculative.

(5) For me it seems more plausible that a combination of pH, BS and ECEC which show strong correlations as well, might result a stronger impact for CH_4 flux. And a correlation of ^{15}N might point towards coupled methane oxidation and denitrification (e.g. Zhu et al., 2016)? Based on the microbial processes it can be assumed that CH_4 oxidation should contribute to CO_2 formation. However, this is indicated by a correlation of only -0.24 (CH_4 and CO_2) in Table 5. Consequently, a potential coupling of aerobic methane oxidation and denitrification might result only -0.07 (CH_4 and N_2O) in table 5. Finally the introduction and discussion would highly benefit to be focused more on microbial processes.

Minor comments:

Introduction

It might be better for the reader to follow the different microbial processes which cause the production and consumption of each trace gas rather than jump from effects of temperature to moisture to soil properties on CO_2, CH_4, N_2O and NO? Overall the introduction is missing a clear structure.

You are writing about methanotrophs and methanogens, but for the other trace gases you don't include any information about the processes and functional microbial groups.

Line 40: Studies (without references) either include references or refer to a comprehensive list in supplement.

Line 65/66: take care of terminology, maybe define once? Net CH_4 flux consists of production (positive) and consumption (negative). Furthermore, it should be mentioned that production occurs even under negative net CH_4 flux, but consumption is predominant.

Material and Methods

Line 149 "soil trace gas flux measurement": you can only measure mixing ratios. Fluxes are the result of a second order calculation.

Line 150 "fluxes were measured"?

Line 168 Please specify what gas did you flow through the chambers? Ambient air, synthetic air?

I recommend including the formulas to calculate CO_2, CH_4, N_2O (static) and NO (dynamic), plus the trapezoid rule to calculate the annual fluxes that the reader does not have to look up several other papers to follow the calculations.

Results

The results are majorly focusing on the descriptive correlations. Why the major results of CO_2, CH_4, N_2O, NO fluxes is not presented here? For me these are the major results obtained from the field by hard work (Fig1 and Fig2).

Line 291 Due to different soil properties for each site, it seems not very helpful to present Fig. 3 and talk about a "parabolic relationship".

Discussion

Statement about what might cause the NO_3^- differences? Wet deposition, if yes, are there values from literature?

The connection of the trace gas fluxes to microbial processes is missing. E.g. the correlation of CH_4 fluxes (net uptake) is negatively correlated to 15N natural abundance. Does this point towards a CH_4

production coupled to denitrification? And could this coupling be less relevant in the dry season versus the wet season and thereby result amplified correlations in the dry season?

Figures:

Error bars are missing for Fig 3, 4, and 5

Fig. 4 a, b, c should include a 0 line for easier understanding. Fig. 4a might be better to bin data into moisture classes of 10%. Less data points will make the figure easier to understand and better show trends. Error bars can be included. Would it make more sense to average the single points and report error bars to highlight the grouping in different fertilizer regimes Fig 4b? That might be helpful for discussion?

Fig. 5: Where was the NO ambient mixing ratio measured? Close to the ground (chamber height) or 2m height? Are there references available for such high NO ambient mixing ratios and possible sources? Based on Remde et al (1989) it might be helpful to plot NO release rate versus ambient NO mixing ratio at same moisture and temperature for each site. Furthermore, only data points for a range of soil moisture and soil temperature should be selected.