

## Reponse to Reviewer 1

We would like to thank reviewer 1 for taking the time to provide a useful critique of the manuscript. We have responded to their concerns in blue and copied their original comments in black for ease of reference.

Wolf et al. (2011) got a deeper insight into the soil “black box” by conducting incubations of soil samples from different soil horizons. Finally, they identified a stratification of CH<sub>4</sub>-uptake activity within the soil profile that highlights the heterogeneity of methane cycling processes in organic soils of tropical montane forests. After such a study by Wolf et al. (2011), it would be nice to identify hot spots of CH<sub>4</sub> consumption and/or production within soils of their region and how that correlates with available nitrate, ammonium, oxygen etc

We whole-heartedly agree with reviewer 1 that overcoming the inherent difficulties posed to sampling strategies in attempting to study hot spot activity remains a particularly interesting facet of understanding fine scale heterogeneity within tropical soils (see Hall et al. (2013) for a nice example).

Furthermore, the discussion about N-inhibition or N-limitation of CH<sub>4</sub> consumption and/or production is very speculative without having any information about present methanotrophic or methanogenic community composition and/or activity, especially when the results are so different. Other processes, as well, may eventually lead to the observed positive correlation between net CH<sub>4</sub> flux and nitrate concentrations. Dependent on the nutrient status of the respective forest type, increased soil nitrate availability may stimulate plant growth that accelerates organic carbon availability via root exudation for methanogens and other microorganisms and finally lead to an increase of CH<sub>4</sub> production in anoxic microsites and a decrease of net CH<sub>4</sub> consumption (see Bodelier et al. 2011)

We remain speculative as conceptual models linking CH<sub>4</sub> exchange and N availability are complicated and we are limited to inferring possible causes in terms of changes in net exchange and field conditions. The apparent differences between Indonesia, Ecuador and Peru discussed do indeed seem to suggest a better understanding of patterns in the underlying gross processes is required. We attempt to investigate, albeit at a crude scale, the potential for microsite methanogenic activity in influencing net exchange through measurement of bulk soil O<sub>2</sub> and net CO<sub>2</sub> fluxes. We have extended the text on Page 12 to accommodate the reviewer's good suggestion: *“However, we may also have expected increases in available NO<sub>3</sub><sup>-</sup> to competitively suppress methanogenic activity (Chidthaisong and Conrad, 2000). This is counter to the observation that net CH<sub>4</sub> is positively correlated to available NO<sub>3</sub><sup>-</sup> and that emissions are most prevalent in the premontane forest. Greater below-ground productivity at lower elevations (Girardin et al., 2010), potentially driven by greater nutrient availability and temperature, may also stimulate CH<sub>4</sub> production in the rhizosphere through the supply of labile substrates to methanogenic communities or maintenance of anaerobic microsites through the O<sub>2</sub> demand of heterotrophic respiration (Bodelier, 2011). Such a mechanism, not observed in this data, might be supported by a positive relationship between net CH<sub>4</sub> and CO<sub>2</sub> fluxes (Verchot et al., 2000).”*

What is with phosphorus (see Wolf et al. 2011)? I think that nutrient status of the diverse vegetation including the deep roots within organic-rich soils of tropical montane forests may play an important role in structuring microbial community composition and activity that may be as important as soil structure and precipitation.

In addition to available nitrate and ammonium, phosphate and nitrite data were also obtained from the resin bags. No significant relationship was found between nitrite and net CH<sub>4</sub> flux, whilst, a significant negative relationship was identified between phosphate. However, this relationship was less robust than those which form the main focus of our discussion (i.e. soil temperature, available nitrate and water-filled pore space). As understanding the influence of variations in microbial functional diversity is beyond the scope of our work we omitted these data for the sake of clarity. They will however be available when the dataset is archived with CEDA.

I would remove the word significantly throughout the text. It is in almost every sentence of the “Results” section. I think it is enough if you say that A is higher than B or A influences B. If something is not significant there is no difference or influence. Additionally, you define statistical significance at p<0.05. That is enough, I think.

Following this advice, we have altered the text of results section accordingly.

Page 7, Line 5+6: How did you measure particle density and porosity

We have added this information to Page 7: *“Plot bulk densities were determined from the weight of volumetric soil samples after oven drying at 105 °C for 24 hours. Forest type particle density was determined from measurement of bulked plot samples using a 10 ml pycnometer (Klute and others, 1986).”*

Page 4+5: Could you clarify how many plots were installed, in total

A total of 9 plots were installed; three plots were installed in each of the three forest types. See Page 5, Line 8 – 13:

*“Within each forest type three plots of 20 by 20 m were established approximately three months prior to the start of reported measurements in an attempt to minimise the effects of disturbances involved with installing sampling equipment (Varner et al., 2003). Within forest types the distance between plots ranged from ~ 100 to 1000 m. The plots in the premontane forest were each situated on a ridge, slope and flat feature between elevations of 1070 to 1088 m asl. Similarly, the lower montane forest plots were established on ridge, slope and flat features between elevations of 1532 to 1768 m asl. In the upper montane forest two plots were situated on slopes and the third on a ridge at elevations between 2811 to 2962 m asl.”*

Page 9, Line 22 and Table 3; Figure 4: As far as I understand, you have 3 plots per elevation (these are your independent samples if you say they were randomly selected;  $n=3$ ). Now, you can do linear regression between your variables of interest among these three points but in my opinion you are not allowed to do linear regression among all samples (9 plot means) of the elevation gradient because they are not independent! You can check whether your forest type means differ from each other but not a linear regression among 9 plot means.

Reviewer 2 also raised this point and we have copied this response there. Our decision to treat measurement plots within a 'forest type' (or elevation band) as independent replicates of net  $\text{CH}_4$  exchange is based on the assumption that spatial autocorrelation is limited to short distances (i.e. operating at sub-plot scales of ~ 1 to 10s of m). The plots in our study were more than 100 m apart. We treat our observations as longitudinal data to investigate the possible drivers of the relationship between net  $\text{CH}_4$  flux and elevation within our study area. In an attempt to synthesise this information, we then discuss  $\text{CH}_4$  exchange in terms of the ecosystem transitions (or 'forest types') seen across the landscape. This approach is adopted from the literature, for example, across montane forest landscapes (Purbopuspito et al., 2006, p.3) and more recently across lowland tropical forest landscapes (Hassler et al., 2015). We state our approach in the manuscript on Page 6, Line 19 – 21: *“Despite the three plots within each forest type broadly occurring within the same forest stand they were considered independent replicates of forest type as spatial correlations between net  $\text{CH}_4$  fluxes in tropical forests are small (Ishizuka et al., 2005a; Purbopuspito et al., 2006)”*. However, we acknowledge the concerns of both reviewers. The use of site ( $n = 3$ ) or plot ( $n = 9$ ) means in correlation tests does not fundamentally change the pattern or effect size of the relationships which form the basis of our discussion in section 4.2. For example, focussing on the most robust relationships identified between  $\text{CH}_4$  exchange and environmental conditions (Table 3 (Pearson's  $r > 0.8$ ,  $p < 0.05$ ,  $n = 9$ ) and then graphed in Figure 4):  $n = 3$ , net  $\text{CH}_4$  flux vs. elevation (Pearson's  $r = -0.85$ ,  $p = 0.35$ ), soil temperature (Pearson's  $r = 0.86$ ,  $p = 0.34$ ), WFPS (Pearson's  $r = 0.99$ ,  $p = 0.10$ ) and  $\text{NO}_3$  (Pearson's  $r = 0.92$ ,  $p = 0.25$ ). In an attempt to minimise confusion caused by the text, we have altered the somewhat unclear use of 'site' on Page 4 to fall into line with the way we treat the data and how the experimental approach along this transect has previously been described (e.g. Teh et al., 2014, p.2)

#### References

Hall, S. J., McDowell, W. H. and Silver, W. L.: When wet gets wetter: decoupling of moisture, redox biogeochemistry, and greenhouse gas fluxes in a humid tropical forest soil, *Ecosystems*, 16(4), 576–589, 2013.

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