Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-107-AC1, 2017

Biogeosciences
Discussions

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

Torsten Diem et al.

yateh@abdn.ac.uk

Received and published: 15 June 2017

The authors would like to thank the referees and associate editor for the thoughtful and constructive remarks they have provided on our manuscript. We provide a detailed response to the referees' concerns in the sections below.

RESPONSE TO REFEREE 1

1. 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

Printer-friendly version

Discussion paper

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 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.

AUTHOR RESPONSE: Thank you to the referee for the positive remarks on our manuscript. We welcome the referees' suggestion for improvements to our manuscript.

2. 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).

AUTHOR RESPONSE: The referee makes a valuable observation about how the research is framed in the introduction, which is in-line with the suggestion of the second referee to reformulate our hypotheses so that they are more nuanced (point 17 below). With respect to the remark about removing the hypothesis about labile organic matter; we would prefer to keep this hypothesis, because several commonly used models use labile organic matter as a parameter for predicting N2O flux, including DAYCENT, DNDC, and the model ECOSSE (developed by co-author Smith) (Li, 2000;Smith et al., 2007;Werner et al., 2007). As a consequence, the negative finding from our field-based

Printer-friendly version

Discussion paper

litter manipulation is still an important result, because it suggests that labile organic matter may be a less important driver N2O flux in these montane tropical ecosystems. However, we are happy to streamline the discussion surrounding the litter manipulation study, in the interests of brevity.

3. 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?

AUTHOR RESPONSE: The referee's point about potential treatment effects from handling, transportation, and storage of soils is well-made, and we recognise that the results from the laboratory experiments represent only the potential behaviour of the soils. As far as possible, we tried to minimise potential treatment effects by transporting soils under ambient (room temperature) conditions, recognising that cold storage of tropical soils has been found to significantly alter soil process rates (Arnold et al., 2008;Verchot, 1999). We also set-up the field experiments as quickly as possible after the soils were received in Aberdeen; normally within only one or two weeks after the soils' arrival. Lastly, the laboratory incubations were conducted with intact soils, rather than sieved soils or slurries, recognising that destruction of soil structure can alter biogeochemical process rates by changing redox gradients within aggregates and altering substrate competition among anaerobes (Sexstone et al., 1985;Teh and Silver, 2006).

However, it is important to note that we were dependent upon the laboratory studies to help interpret patterns detected in the field data (e.g. responses to changes in WFPS or N availability), because the complex controls on N2O production and emission made it difficult to establish clear empirical patterns between control variables (e.g. WFPS, inorganic N availability) and emissions. We will aim to clarify all these points in the revised version of the manuscript, and more clearly acknowledge the potential limitations of our study. With respect to the question of pH changes before and after transportation; we believe it is unlikely that transportation will have significantly altered pH because we compared data from soils measured in Peru (Zimmermann et al., 2012;Zimmermann et al., 2009a;Zimmermann et al., 2009b;Zimmermann et al., 2010) against samples that were measured after transportation in the UK, and average pH values did not appear to differ. For the lab experiments described here, we unfortunately did not measure pH measured after transportation, but only at the end of the experiments. The pH values measured at the end of the experiment were, on average, half a unit higher than the pH values measured for field soils.

4. 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. 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.

AUTHOR RESPONSE: Thank you for the suggested reference; we will read the paper you have suggested, and aim to revise our discussion to include the insights gained from Mueller et al. (2015) so as to deepen the links to other parallel studies.

5. 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).

AUTHOR RESPONSE: This point is well-taken, and is in-line with referee 2’s suggestion that we should streamline the results section by only discussing the main findings and putting the other statistical results in tables (point 16 below). We will also read the references suggested, and revise the discussion to incorporate a wider discussion of recent literature.

6. L45-L48: This should also be mentioned in the conclusion section

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

7. L98: ...derived from (missing word)

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

8. L290: What is the sampling size of the background concentration measurements?

AUTHOR RESPONSE: We measured background concentrations once for every individual soil core, thus n=5 for each elevation

9. L300: What was the length of time between sampling and analysis?

AUTHOR RESPONSE: Samples were analysed no more than one week after the samples arrived in Aberdeen. Transport time from Peru to the UK varied between one and two weeks.

10. L827-L843: Remove heading and shorten section.

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

11. L880-L900: Does this section really enrich the discussion?

AUTHOR RESPONSE: The aim of this paragraph was to link the patterns in the field data with what we found in the laboratory experiments. We also speculated as to why the nitrate reducing microbes in our soils showed such a weak response to relatively large manipulations of inorganic N availability, given that we expected that the microbes would show a stronger short-term response to elevated N inputs.

12. 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.

AUTHOR RESPONSE: We will aim to introduce the concept of pH as an important controlling variable earlier in the text.

13. 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.

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

14. 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)

AUTHOR RESPONSE: These references will be incorporated in the revised version of the text, and their findings incorporated into our wider discussion of the results.

RESPONSE TO REFEREE 2

15. 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 straightforward 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.

AUTHOR RESPONSE: Thank you to the referee for the positive remarks on our manuscript and the constructive comments provided below.

16. 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.

AUTHOR RESPONSE: Thank you for these useful suggestions. It was, admittedly, difficult to find a very simple and elegant way of presenting the data, given the diverse experiments and complex results. The referee's suggestion is well-taken, and in-line with the first referee's remarks (point 5 above). We will aim to revise the results so that the statistical outputs are summarised more neatly in tables, and only the major findings referenced in the main body of the text.

17. 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?).

AUTHOR RESPONSE: This comment is also broadly in-line with observations made by referee 1 (point 2 above). We will revise the hypotheses in order to make them more nuanced, and to better-reference our earlier published study.

18. 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.

AUTHOR RESPONSE: We analysed the data in a number of different ways in order to explore not only instantaneous but lagged responses of N2O flux to rainfall. Unfortunately, because we did not have large enough number of data points, we were unable to employ more sophisticated time series methods (e.g. AR, ARIMA) to evaluate whether or not the apparent lags in the data were real, and were reliant on more simple methods of analysis such as repeated measures ANOVA. We were unable to pinpoint lag effects using this method of analysis, although this is not to say these lags do not in fact exist, merely that we were unable to detect them using the sampling method and analysis tools that we employed.

19. 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?

AUTHOR RESPONSE: We will aim revise the text to better highlight that this apparent bimodal trend is derived from the overall (pooled) dataset, and may not be universal or as straight forward for all study sites.

20. 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.

AUTHOR RESPONSE: The reference to N-rich or N-poor is relative, and largely in reference to the parallel Ecuadorian transect, where similar types of studies have been conducted by our German colleagues. We will revise the text to provide a better regional context for our statements.

21. 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)?

AUTHOR RESPONSE: We will revise the manuscript to clarify the point that there is not so much a gradient as a step change or shift in N2O flux from premontane forest to the other habitats.

22. 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?)

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

23. 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?

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text. We will revise the sentence structure to clarify meaning.

24. 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.

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

25. L 95: check spelling "areally"

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

26. 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.

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text. This is probably a typological/grammatical error that we missed when editing the manuscript.

27. L 152: I like how the authors also analyzed topographic landforms. However, through- out 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.

AUTHOR RESPONSE: Topography/landform was treated as a categorical variable in our repeated measures ANOVA or ANCOVA tests. For the laboratory incubations, two soil cores were sampled from each landform. With respect to terminology; we will go through the text a revise the language so that we keep to the same terminology so as to avoid reader confusion.

28. L250: This sentence essentially repeats the statement in L240

AUTHOR RESPONSE: This sentence will be removed in the revised version of the manuscript.

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

AUTHOR RESPONSE: Yes.

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

AUTHOR RESPONSE: Soil oxygen content was measured every time soil gas flux was sampled.

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

AUTHOR RESPONSE: The overall period for the incubation was 48 hours. For the late phase of the incubation, we calculated the flux rate over 24 to 48 hours.

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

AUTHOR RESPONSE: Please see point 31.

33. 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.

AUTHOR RESPONSE: The text will be revised to correct this oversight.

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

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

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

AUTHOR RESPONSE: We were referring to the overall trend of decreasing N2O flux with increasing elevation. We will revise this sentence to try and clarify our meaning.

36. 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)?

AUTHOR RESPONSE: When evaluating for the effect of N addition level on N2O flux, the ANOVA pooled data across all other categories (i.e. site, incubation phase) to compare the difference in N2O flux among N treatments. Because of the high level of variability in N2O flux among study sites and incubation phases, the net effect was that the ANOVA found no clear signal of N addition level alone. The lack of trend is not an artefact of the ANOVA calculation per se, but rather represents the high level of variability among soils from different study sites and differing responses of N2O flux during different incubation phases.

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

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

RESPONSE TO REFEREE 3

38. Diem et al. report on a remarkably large and comprehensive set of observations and experiments examining N2O fluxes across the Kosnipata tropical elevation gradient in Peru. This was clearly a lot of work. The combination of high temporal resolution chamber observations with WFPS, 15N and litter experiments makes the study particularly compelling. I have four suggestions. First, there a few aspects of the 15N tracer work that require further clarification. Second, I recommend the authors consider scaling their observations to annual values. Third, depending on details of the 15N tracer methods, I suggest the authors consider making use of the N2: N2O flux ratios from the incubations to estimate total N gas losses from these ecosystems if appropriate. Finally, I think the authors could do a better job at contextualizing their work with reference to other studies and its global implications.

AUTHOR RESPONSE: We thank the referee for the positive remarks on our manuscript. We will endeavour, in the revised version of the manuscript, to address the four key suggestions that the referee has outlined here, as we believe that the referee's suggestions will enable us to produce a manuscript that better frames and contextualises our research in a wider regional and global context.

39. 15N tracers: It would appear that the WFPS experiment was not a true "tracer" experiment but is also a N addition experiment and is therefore confounded. For the lower elevation sites, 200 ug N/g soil is not trivial. Are you sure that the background NO3 values are correct? The reported NO3-N values from soil extractions of âĹij150 ug/g are approximately 5-10 times higher than those observed in across most high N old- growth tropical forests worldwide. Tracer experiments often add < 0.5 ug/g at 15NO3 of âĹij99 atom percent. Further, unless I missed it, there is no description of

Printer-friendly version

Discussion paper

the isotopic enrichment levels (per mil or atom percent). This needs to be included.

AUTHOR RESPONSE: Upon closer inspection, we realised that the values reported in the table are incorrect, and that the actual amount of N added was in fact much smaller than reported in the text. For example, for the WFPS experiment, the added amounts were 200 ng N/g soil for the lower elevation sites and 20 ng N/g soil for the higher elevation ones. For the N addition experiment, the values of N reported are the total amount of N added for the soil sample, and need to be normalised so that the values are reported on a per g soil basis. Thus, the true amounts of 15N tracer added in both the WFPS and N addition experiments are in fact in-line with the "trace" amount more typical of these types of 15N labelling experiments. We will correct this in the revised version of the text. With respect to the level of isotopic enrichment; we applied the tracers at a 30 atom % level (see line 309). We will add this information to the description of the WFPS experiment to ensure clarity of expression.

40. Scaling: Given the seasonal representation of the sampling, I think annual scaling could be justified. When scaled annually, the mean N2O-N emissions (0.27 mg N m-2 day-1) would be âĹij 0.98 kg N ha-1 yr-1 with peak fluxes of âĹij2.7 kg N ha-1 yr-1. On average, chamber studies and models find that N2O losses from undisturbed humid tropical soils are âĹij1-4 kg N ha-1 yr-1 (See van Lent et al. Biogeosciences 2015 and Werner et al. Global Biogeochemical Cycles 2007). So, these values fit right in.

AUTHOR RESPONSE: In our earlier paper (Teh et al., 2014), we provided area-weighted annual estimates, and are happy to conduct a similar simple extrapolation exercise here to provide area-weighted seasonal flux estimates. While these simple estimates are not as robust as those derived from process-based models, they are useful in terms of furthering the discussion about the potential role that these types of montane tropical ecosystems play in regional and global atmospheric budgets.

41. N2 fluxes: Given the response to the first point above, I suggest considering approximating total N gas losses from these ecosystems. Despite potential artifactual

Printer-friendly version

Discussion paper

contributions of the incubations (disturbance, N additions) one could calculate rough N2 losses assuming equal N2:N2O ratios at a given WFPS as measured during the chamber work. This could be insightful as there are many chamber-based N2O estimates for tropical forests published but very few for total N gas fluxes because it's difficult to measure. Eyeballing the 15N2 versus 15N2O flux ratios (âĹij20 to 80) and applying these to the chamber observations would yield N2 fluxes of âĹij20 – 216 kg N ha-1 yr-1. The lower-end flux is possible (see Fang et al. PNAS 2015) but the upper end estimate is highly unlikely. Such total N export rates could never persist in a near-equilibrium forest as even the lower end is higher than average N mineralization and annual plant uptake and far exceeds external N inputs in tropical forests (see Brookshire et al. Geophysical Research Letters 2017).

AUTHOR RESPONSE: We will take this into consideration, and will calculate potential N2 emissions for our sites from the N2O/N2 ratios. However, as the referee already suggests, these values, at the upper end of the range, may be unreasonably high, most likely due to the fact that the conditions for the laboratory experiments do not match conditions in situ, and may overestimate field flux rates. However, this exercise may be useful in generating wider discussion in the community, and enabling us to develop hypotheses for future testing.

42. The beauty of the Kosnipata gradient is that it represents a quasi-space-for-climate change substitution. More could be done with this context in the introduction and discussion. Further there are many other papers examining denitrification in tropical landscapes (some of them mentioned here) that would benefit the narrative to include.

AUTHOR RESPONSE: This remark is in agreement with concerns raised by other referees, and we will improve the introduction and discussion to include more recent research on this topic.

RESPONSE TO REFEREE 4

43. Diem et al. present a comprehensive set of lab and field data relating to controls

of soil nitrous oxide flux across an elevation gradient in the Peruvian Andes. As both long-term field measurements and lab-based manipulations are included, they are able to approach the discussion of N2O fluxes in these ecosystems from several different directions. This was excellent work that will be a valuable addition to our current knowledge of N-oxide fluxes and tropical montane ecosystems. However, the authors could really improve the paper by taking some additional time to craft a more integrated presentation/summation of their study. The results section, in particular, should be revised. A well-designed table or figure (or combination) could provide a fascinating and useful summation of the different experiments, while eliminating the repetitive text. Instead, the text of the results section should highlight the most important results – much of this could be moved from the discussion section, which can then be condensed and re-focused to provide a bit more literature context about the different aspects of the results being discussed.

AUTHOR RESPONSE: Thank you for these very positive remarks. The referee's suggestions for improvement are broadly in-line with other referees' concerns (e.g. point 3) about the manner in which the results have been presented, and we will revise the text to address these concerns.

44. Line 105: substrates for ____?

AUTHOR RESPONSE: The sentence should have been deleted while editing; the revised text will be changed accordingly.

45. Line 138: give average temperature range over the course of the study

AUTHOR RESPONSE: Mean annual temperature is provided in Table 1.

46. Line 161: change 'because of' to 'due to'

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

47. Line 172: provide volume of chamber

Printer-friendly version

Discussion paper

AUTHOR RESPONSE: The chamber volume was approximately 0.008m3 (8 L); the text will be revised accordingly.

48. Line 179: specify intervals

AUTHOR RESPONSE: Gas samples were collected at evenly spaced intervals over a 30 minute period; i.e. samples were collected 7.5 minutes apart. The text will be revised accordingly.

49. Line 187-192: were zeroes included?

AUTHOR RESPONSE: Yes. The revised text will be altered accordingly.

50. Line 227-230: provide more detail: soil samples were taken in the field, air-dried and then re-wetted to target WFPS?

AUTHOR RESPONSE: To clarify, the WFPS experiments were conducted with field-moist samples; i.e. the soil samples were collected from the field, shipped to Aberdeen, and subsequently distributed into glass jars without being fully air-dried. For incubations where the target WFPS was below the field moisture levels, the soils were allowed to partially air-dry until they reached a value 10 % below the target WFPS for the experiment, and then carefully re-wetted through the 15N tracer application to bring up the soil moisture up to the target levels. For treatments where the target WFPS was above field moisture levels, the soils were simply wetted to 10 % below the target WFPS and then the 15N tracer solution added to bring the soil up to the target moisture level. The text will be revised to describe this procedure in greater depth.

51. Line 231-233: needs clarification: 0-10 cm depth included the organic layer at all elevations, except in the upper montane forest where 0-10 cm depth included only mineral? If 0-10 sometimes included the organic layer, what was the thickness of the organic layer at those elevations? What was the thickness of the organic layer at the upper montane site; how deep did you go to access the 0-10 mineral sample? explain reasoning behind this sampling decision; could this have affected your results?

AUTHOR RESPONSE: For premontane forest, lower montane forest, and montane grassland, the organic matter in the upper 10 cm soil layer is intermixed with the mineral phase, and does not constitute a distinct mineral-free horizon. Thus, we simply sampled from the 0-10 cm depth because there was no practical means of separating the organic matter from the mineral soil in these habitats. In contrast, upper montane forest soil shows a very different pattern of vertical stratification compared to the other habitats. In this habitat, the mineral soil is overlain by a thick (up to 17 cm deep) mineral-free organic layer, consisting of poorly decomposed leaves, roots, and humic materials; very akin to low density peat. To sample the mineral soil in this habitat, we went below this distinct organic horizon to a depth of approximately 17 cm.

With respect to the WFPS experiment; we decided to collect mineralÂăsoil from below the organic horizon in the upper montaneÂăforest because thereÂăwas no mineral material found in this layer, making it difficult to compareÂăresults between habitats (given thatÂăthe other habitats contain mineral material in the upperÂă10 cm of their soil profiles). At the time, we did not consider sampling theÂăorganic layer asÂăwell. This was an oversight on our part, which weÂătried to partially correct in our N addition experiments, by including theÂăorganic layer in those subsequent experiments. In theÂărevised form of theÂămanuscript, we will try to acknowledge this shortcoming in our laboratoryÂăexperiments.

52. Line 297-307: clearly distinguish between ‘soil core’ and ‘soil sample’; “core” implies that the soil is still intact – once it has been mixed and added to the jars, the soil samples are no longer soil cores

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

53. Line 300-301: unclear; the five cores were mixed and then split into four equal parts? was the subsample and WFPS adjustment done on the cores or on the mixed soil in the jars?

AUTHOR RESPONSE: Each of the cores was split into four equal parts. The text will be re-written to clarify this point.

54. Line 375: change ‘with’ to ‘and’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

55. Line 462: followed by topography

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

56. Line 473: change ‘is’ to ‘was’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

57. Line 474: define the fluctuation or refer to a table or figure where it is defined

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

58. Line 585: change ‘for’ to ‘from’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

59. Line 761: change semicolon to comma

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

60. Line 768: between soil temperature and ___?

AUTHOR RESPONSE: N2O

61. Line 779: change ‘as’ to ‘at’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

62. Line 782: change ‘are’ to ‘is’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

63. Line 836: remove ‘and’

AUTHOR RESPONSE: Editorial suggestion will be taken in the revised version of the text.

REFERENCES

Arnold, J., Corre, M. D., and Veldkamp, E.: Cold storage and laboratory incubation of intact soil cores do not reflect in-situ nitrogen cycling rates of tropical forest soils, Soil Biol. Biochem., 40, 2480-2483, 10.1016/j.soilbio.2008.06.001, 2008. Li, C. S.: Modeling trace gas emissions from agricultural ecosystems. , Nutrient Cycling in Agroecosystems, 58, 259-276, 2000. Sexstone, A. J., Revsbech, N. P., Parkin, T. B., and Tiedge, J. M.: Direct Measurement of Oxygen Profiles and Denitrification Rates in Soil Aggregates., Soil Sci. Soc. Am. J., 49, 645-651, 1985. Smith, P., Smith, J. U., Flynn, H., Killham, K., Rangel-Castro, I., Foereid, B., Aitkenhead, M., Chapman, S., Towers, W., Bell, J., Lumsdon, D., Milne, R., Thomson, A., Simmons, I., Skiba, U., Reynolds, B., Evans, C., Frogbrook, Z., Bradley, I., Whitmore, A., and Falloon, P.: ECOSSE: Estimating Carbon in Organic Soils - Sequestration and Emissions. Final Report., Scottish Executive Environment and Rural Affairs Department Report, 166 pp., 2007. Teh, Y. A., and Silver, W. L.: Effects of soil structure destruction on methane production and carbon partitioning between methanogenic pathways in tropical rain forest soils, Journal of Geophysical Research: Biogeosciences, 111, n/a-n/a, 10.1029/2005JG000020, 2006. Verchot, L. V.: Cold storage of a tropical soil decreases nitrification potential, Soil Sci. Soc. Am. J., 63, 1942-1944, 1999. Werner, C., Butterbach-Bahl, K., Haas, E., Hickler, T., and Kiese, R.: A global inventory of N2O emissions from tropical rainforest soils using a detailed biogeochemical model, Global Biogeochemical Cycles, 21, 1-18, Gb3010 10.1029/2006gb002909, 2007. Teh, Y. A., Diem, T., Jones, S., Huaraca Quispe, L. P., Baggs, E., Morley, N., Richards, M., Smith, P., and Meir, P.: Methane and nitrous oxide fluxes across an elevation gradient in the tropical Peruvian Andes, Biogeosciences, 11, 2325-2339, 10.5194/bg-11-2325-2014, 2014. Zimmermann, M., Meir, P., Bird, M., Malhi, Y., and Ccahuana, A.: Litter contribution to diurnal and annual soil respiration in a tropical montane cloud forest, Soil Biology and Biochemistry, 41, 1338-1340, 2009a. Zimmermann, M., Meir, P., Bird, M. I., Malhi, Y., and Ccahuana, A. J. Q.: Climate dependence of heterotrophic soil respiration from a soil-translocation experiment along a 3000 m tropical forest altitudinal gradient, European Journal of Soil Science, 60, 895-906, 10.1111/j.1365-2389.2009.01175.x, 2009b. Zimmermann, M., Meir, P., Bird, M. I., Malhi, Y., and Ccahuana, A. J. Q.: Temporal variation and climate dependence of soil respiration and its components along a 3000 m altitudinal tropical forest gradient, Global Biogeochemical Cycles, 24, GB4012, 4011-4013, 10.1029/2010GB003787, 2010. Zimmermann, M., Leifeld, J., Conen, F., Bird, M. I., and Meir, P.: Can composition and physical protection of soil organic matter explain soil respiration temperature sensitivity?, Biogeochemistry, 107, 423-436, 10.1007/s10533-010-9562-y, 2012.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-107, 2017.

Printer-friendly version

Discussion paper