

Interactive comment on “Soil trace gas fluxes along orthogonal precipitation and soil fertility gradients in tropical lowland forests of Panama” by Amanda L. Matson et al.

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Received and published: 27 January 2017

GENERAL COMMENTS

This is an elegantly written paper, that nicely frames the research within the wider theoretical context of soil trace gas fluxes in the tropics, and succinctly reviews our current understanding and identifies major knowledge gaps. Importantly, the author correctly identifies the fact that while existing theory about the controls on soil trace gas exchange in the tropics are underpinned by theoretical expectations, there have been relatively few tests of this theory across natural rainfall or fertility gradients in the tropics. The work presented here tests and further develops our theoretical understanding by employing these natural gradients to tease-out the synergistic effects of

rainfall, nutrients, and other environmental drivers on modulating soil-atmosphere trace gas exchange. The data presented here are thus ideal not only for assessing theory, but could also for testing, developing and parameterising bottom-up process models, such as DNDC or DAYCENT.

In addition to the general contributions that this paper makes, I believe it also helps to enhance our understanding of specific biogeochemical cycles. For example, this paper helps to develop our understanding of the multiple constraints placed on methanotrophy in tropical soil. Prior studies have tended to focus on a smaller sets of control variables (e.g. moisture and temperature only), whereas this study takes a more comprehensive look at the role of environmental variables and seasonality on methane uptake.

Likewise, this paper enhances our understanding of less well-studied gases such as nitric oxide (NO), for which we know far less than it's more "popular" sister compound nitrous oxide (N₂O). The overall trend towards net NO uptake across this area is intriguing, given that past studies in CSA has emphasised the role of these systems as regional/global NO emission sources. It would be useful if the authors could develop or speculate on the wider implications of NO uptake for local and regional atmospheric chemistry, as I do not believe that our wider community has fully engaged with the notion of soils as NO sinks, given the past emphasis on soils as NO emission sources.

While I did not have major criticisms of this paper, I do have a few suggestions for improvement. First, it would be useful if the authors could make more use of multiple regression or the mixed effects models to determine the hierarchy (i.e. relative importance) of environmental drivers for different trace gases (i.e. which are the dominant and which are the lesser environmental controls?). While the authors have outlined the dominant role of soil moisture, it would be interesting to see a clearer description of the relative importance of the other drivers. Does the hierarchy of drivers vary among sites? Do the hierarchy of drivers vary among seasons? This would help develop not only a more "global" understanding of which drivers are dominant in this region, but

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also help us understand how individual study sites differ from each other at different times of year.

Second, in the section on soil CO₂ flux, I think it would be useful if the authors could revise the text to incorporate a slightly expanded discussion of how root respiration could be influencing variations in soil CO₂ fluxes (see point 10 below). For example, could the differences in respiration between this study site and others be attributed to differences in belowground biomass or root/shoot allocation? Do data exist on belowground biomass in these sites? If so, do those data help explain patterns in soil respiration?

Third, in section 4.2 of the Discussion, the authors have identified separate sets of controls on CH₄ uptake that appear to be operating on different time scales; i.e., daily fluxes of CH₄ appear to be more strongly linked to soil moisture, whereas soil fertility was a stronger constraint on annual CH₄ fluxes. This is an important and interesting finding, as it highlights the scale-dependency of different environmental controls, and suggests that different environmental factors may be controlling different aspects/components of trace gas cycling; e.g. in the short-term, soil moisture may be regulating transport and supply of CH₄ to methanotrophs (hence, regulating instantaneous fluxes), whereas in the long-term, site fertility may be influencing the total amount of methanotrophic biomass or the overall methanotrophic potential of these soils. It would be useful if the authors could consider a way of revising the current text to better highlight this important finding, as it has wider implications for upscaling these results or incorporating these findings into process-based models.

Other minor specific comments are provided below.

SPECIFIC COMMENTS

1. Lines 158-165: It would be useful know the precision of the analysis; i.e. what was the coefficient of variation for the standards?
2. Lines 173-174: Ibid.

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3. Lines 175-177: Were any fluxes non-linear? How were these data treated? Under more saturated soil moisture conditions, was there any evidence of ebullition? If so, how were these data treated?
4. Lines 205-207: Are there any potential limitations associated with using this ^{15}N natural abundance technique?
5. Lines 254-256: Have the authors considered using Box-Cox transformations to normalise the data? If successful, this would enable the authors to use parametric statistics (e.g. linear regression, multiple regression) rather than Spearman's Rank correlation. Moreover, even if the data do not fully meet the assumptions for parametric analyses, it may be useful/instructive to analyse the data using multiple regression techniques to evaluate the relative hierarchy of environmental drivers.
6. Lines 271-286: It is worthwhile reporting the seasonal trends (or, lack of trends in NH_4^+) here as well. Does NH_4^+ show wet or dry season differences? I had assumed not given that this wasn't stated explicitly. This is interesting because if there are no significant differences, which could be interpreted that rates of net ammonification/ NH_4^+ mineralisation are relatively invariant (although of course you cannot infer whether this is due to invariance in N mineralisation or DNRA rates).
7. Lines 274-275: Do you have complementary measurements of net or gross N cycling processes to help interpret these field patterns? It's possible that the reduction in NO_3^- during the wet season may be linked to reduced nitrification (with a growth of anoxic microsites), or an increase in NO_3^- reduction (e.g. DNRA or denitrification).
8. Lines 293-297: Were these data from bivariate regressions or from a multiple regression model? If the second, it would be useful to indicate, based on the sum of squares, which variables accounted for a larger proportion of the variance and which variables accounted for less, in order to clearly establish the hierarchy of drivers.
9. Lines 317-319: Increased evidence for nutrient limitation of methanotrophy? What

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are the implications of this for process models (could be discussed in the Discussion)?

10. Lines 339-341: Evidence for very active nitrifiers? Perhaps this could be explored further in the discussion.

11. Lines 362-363: To what extent is inter-annual variability modified/affected by differences in belowground allocation and variations in root-rhizosphere respiration? Do data exist on the belowground biomass across your gradient or differences in root/shoot allocation? If so, this may help tease-out the extent to which differences in total soil respiration are affected by differences in the fluxes from individual respiration components.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-393, 2016.

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