Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-179-RC2, 2017

Biogeosciences
Discussions

Interactive comment on "Ammonium and nitrate additions differentially affect soil microbial biomass of different communities and enzyme activities in slash pine plantation in subtropical China" *by* Chuang Zhang et al.

Anonymous Referee #2

Received and published: 21 June 2017

This manuscript by Zhang et al examines the impact of 4 years of simulated atmospheric N deposition in ∼30 year old pine-dominated forest plots. The approach used by the authors is novel in that it differentiates between type of N applied (NH4 vs NO3), which is particularly interesting due to the global stabilization/decline in N deposition as NO3 and the increase in N deposition as NH4.

General Comments:

1) In general, the data are much more complex across time than the authors present.

It would be nice if the results are as clean as suggested in the topic sentence of each discussion paragraph, but it is simply not the case because many of the results are time-dependent. For this reason, the authors need to greatly expand the interpretation and discussion of the treatment x time interaction that is presented in Table 2. Further, to help the reader reason through the data, I think it would be beneficial to collapse the data to not include the 3 sampling times for those factors that do not exhibit a significant treatment x time interaction. For example, Fig 2(i) can be reduced to three bars for control, ammonium N, and nitrate N because there was not a significant interaction.

2) More can be done with soil enzyme data to forward the authors main hypotheses and ideas that are introduced in line 119. For example, enzyme data can be presented as ratios of C acquiring/N acquiring and/or C acquiring/P acquiring. Such analysis will provide a clearer avenue to draw conclusions about whether microbes can alter how resources are allocated to scavenge for nutrients under different conditions.

3) Is there any ecological rationale for the March/June/October time points? What is the climatic variation across these times? Also, I assume soil moisture was measured, and if it was, those data should be presented and included in all analyses (including the RDA). Soil moisture has been shown to be a major driver of microbial community composition.

4) The manuscript needs to be edited for grammar, flow, and word choice. The writing is poor and must be improved significantly in order to be publishable.

Minor comments:

Line 169: Is there rationale for the dose of N applied? Any relation to predictions for future N deposition in the region?

Line 229: How were the 3 sampling times considered for the RDA? Was the RDA ran on data from one of the three sampling times? Or from average data across the sampling times? Given the treatment by time interaction, this point needs clarification.

Line 315: Others have shown that N addition disproportionately effects soil fungi and may stimulate soil bacteria (for example, see doi 10.3389/fmicb.2016.00259 and 10.1128/AEM.01224-14). This dynamic may also help explain the increase in DOC observed with N addition.

Table 2: I think P-values can be removed from this table. Given that significant values are bolded, P-values are redundant and make the table busy for the reader.

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