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Comment

Interactive comment on “Responses of CH₄ uptake to the experimental N and P additions in an old-growth tropical forest, Southern China” by T. Zhang et al.

Anonymous Referee #2

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General comments: Zhang et al. have carried out a N and P fertilization study in a mature tropical forest and characterized monthly soil-level CH₄, CO₂, and N₂O fluxes as well as pre and post fertilization (33 months) soils analyses. I feel that there is a substantial amount of interesting work that went in to this manuscript and that the overall questions the experiments carried out can answer are important. I generally also think that the data presentation in figures 3-6 represents a useful way of combining data to illustrate key patterns. I do have some major reservations about the manuscript as it stands however. There are some misunderstandings about the ecology of CH₄ oxidizing bacteria. It would be important for the authors to read about methanotrophs in detail and amend the introduction and discussion appropriately before this paper

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is publishable in my opinion. The introduction in general has too much extraneous information, but not important framing of methanotroph ecology in forest/upland soils and the influence of increased deposition and fertilizers on their activity. There are also parts of the methods and results that don't really fit well in to the paper. The title and discussion only (or at least mostly) focus on CH₄ uptake, yet much more work was done- either other results should be deleted or the title and discussion changed- though my feeling is that the N₂O fluxes and soil respiration should be kept. Based on the data presented in the study, I also don't really see the evidence for N saturation that the authors claim in this site (with reported low soil N:P ratios, small inorganic soil N pools, slow N₂O efflux- even with ammonium nitrate fertilizer), though admittedly I do not have direct experience working in tropical forests. The authors should also justify why the addition rates of N and P were chosen (150kg per year for 33 mo).

Specific comments:

Introduction: Delete most of the first paragraph and rephrase in 2 key sentences at the most; cite only the 2007 IPCC W.G. I document for the impact of CH₄ in the atmosphere over xx years.

Note that at line 9: Wetland soils are also the largest collective global source of atmospheric CH₄, so overall soils contribute to, not reduce atmospheric CH₄ concentrations.

The 2nd paragraph does not really tell the story it should in my opinion and could be re-written: it should be clear from reading this that methanotrophs require C and redox substrate from air (not soil organic C as repeatedly mentioned throughout the paper) and hence diffusion is important. There are too many sweeping statements here about controls on CH₄ fluxes that as written are not clear- for example why would vegetation influence CH₄ oxidation? The explanation for ammonium inhibition of CH₄ monooxygenase is also not clear, nor are implied linkages between soil P and CH₄ oxidation, plant roots and CH₄ oxidation, or P, "litter decomposing fauna", and methanotroph ac-

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tivity.

The 3rd paragraph focussing on broader P limitation in tropical forests to me does not fit well with what was characterized in this study.

P 4956 At lines 18-21 the authors make the claim that reduced ecosystem productivity would lead to reduced microbial activity and therefore influence methane-oxidizing bacteria. First, it is important for the authors to understand that methane-oxidizing bacteria in upland soils are generally not utilizing C derived from SOM. They also likely make up only a very small proportion of the total soil microbial community. I do not understand the linkages that the authors are trying to make here. Also- is productivity and decomposition really reduced here with N deposition? These soils are already highly acidic (the pH would need to be 5.5 to 6 before phosphate is not already largely bound as insoluble Al/Fe precipitates).

P 4956 Line 25: What evidence is there for N saturation? Do the authors more appropriately mean that the forest is no longer N-limited? The cited paper (Mo et al 2006) actually did not measure N deposition or "N saturation", but it appears to have studied litter decomposition in the same site; I don't see this citation as appropriate justification that the site is N saturated (nor is this really corroborated with the data in the present study, at least compared to soil N values in temperate systems I am familiar with).

Objectives: As above, the bulk of the data in this paper are not CH₄ fluxes; either expand the objectives (and elsewhere) or delete the extraneous (CO₂/N₂O) data. The stated hypotheses are more just predictions- they should be substantiated. Typo "...33 months study"

P 4957 Line 11: Were all 3 forests used in this study? I don't see a pine species listed below. Rewrite the methods section only focussing on what is relevant to this study.

P 4957 Line 18: The cited Fang et al 2006 paper that I found seemed to focus only on soil N and N fertilization effect (and as I can tell did not measure N deposition).

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P4957 weather/climate information/ Figure 1: How important is this to the study? Is mean monthly temperature/ precipitation really a key driver of a gas flux made over a short period on only 1 day per month?

4957-4958 and table 1: why is the stand characterization and litterfall important to this study? I would delete most of this. However interestingly the litterfall rates (if one assumes 50% C) match nicely with the soil respiration measurements (with litter decomposition representing about $\frac{1}{2}$ of total soil CO₂ efflux over the study); maybe something the authors could consider if they choose to keep (and bolster the discussion of) soil respiration.

P4958 Line 8/ Table 2: Why are there so many significant mean differences between treatment plots before fertilizer has been added? (This seems to be far more than the 5% that might be expected with acceptance of $P < 0.05$ as ‘significant’.)

P4958: Why were the specific rates of 150 kg N and P per ha per year and method and timing of application chosen?

P4958 Line 21: Do the authors mean “collars” instead of “chambers”

P4959 line 9: reword to “..by gas chromatography equipped with...”; also was there a methanizer attached to the GC-FID for CO₂ analyses?

P4959 line 26: “quantification of soil chemistry and biology” is awkward and not really appropriate. Please reword. Also, in general, what do these measurements add to the study? They are not really analyzed as potential controls on gas fluxes nor are they discussed.

P4960 line 10-11: cite the method for P extraction.

Section 3.1 (and parts of 3.2): Except for WFPS Are these results discussed or included in further analysis of controls on gas fluxes? Are they really important to the study at hand?

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P4961 line 24: is this increase surprising based on the amount of P that was added?

Sections 3.3 and 3.4: how were the time averaged fluxes and variability of these fluxes calculated? There is surprisingly little apparent variability in the fluxes (CVs of <3%).

P4964 Lines 5-8: These “GWP” calculations were not described in the methods section, and I think should be deleted or only discussed briefly with clear caveats in the discussion: One would have to be very careful in interpreting the total GHG burden or mitigation of the fertilization practices as only soil-level GHG fluxes were characterized, not also the NEE CO₂.

P496 Line 20: Why are these measurements better regarding diffusion rates? It seems that pore size and particle size in general would also influence CH₄ and O₂ diffusion (texture, as a proxy for pore size, has often been implicated as a key control on CH₄ oxidation in forest soils).

P4965: There are some key problems with this portion of the discussion, and I recommend restructuring and rewriting it. The context provided here has little to nothing to do with methane oxidizing bacteria. E.g. ecosystem productivity and microbial activity (and certainly not MOB) are not necessarily limited by the same resources; heterotrophic decomposers and AMF (obligate symbionts deriving all organic C from plants) are fundamentally different from MOB- the latter obtaining C and redox substrate from air, not SOM or plants, and having relatively high N demands when grown in culture. MOB represent a select few members of two bacterial phyla and given that ambient atmospheric/upland soil CH₄ concentrations are low, represent only a very very small portion of total soil microbial/bacterial biomass; therefore discussions about controls on soil microbes in general probably do not pertain to MOB. Also because of this, the linkage to increased root growth and activity supporting increased CH₄ oxidation does not make sense to me (and wouldn't long-term fertilization usually decrease root and AMF growth anyway?). This section needs to be replaced with more relevant discussion and cited literature exploring controls on MOB and CH₄ oxidation, including

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P and nutrient limitation and fertilization effects.

P4966 (top): I am more inclined to believe the explanation of increased rates of CH₄ and/or O₂ diffusion here as an explanation for increased rates of CH₄; below the discussion about NH₄⁺ inhibition of CH₄ oxidation should be made stronger with key mechanisms such as inactivation/preferential binding of CH₄ monoxygenases described (and cited), not just alluded to.

P4967 (top): as above in the results, I don't find the GWP calculations/ discussion to be very meaningful as complete full GHG/C budgets including NPP/ NEE are not available. The soil flux data by themselves might be misleading.

P4967 (Conclusions): This section is more of a summary than conclusion section. The same problems persist here as in the intro and discussion: e.g. nitrifying bacteria/archaea and MOB are phylogenetically different, the mechanism for CH₄ oxidation inhibition with NH₄⁺ is not increased activity by nitrifiers; it was not known if N₂O fluxes were derived from nitrification or denitrification and this received no discussion, so should be omitted here; issues of what is meant by N saturation and why the data (low N₂O, low soil N, low N:P) don't really fit; issues of treating MOB like other heterotrophic soil microbes that would respond to belowground NPP. Finally, how should issues of soil P be taken seriously under high N deposition? Context of why the P loading levels were chosen (e.g. are the authors advocating for P fertilization or liming that might increase P availability) could help here.

Tables 2 & 3 and Figure 1: It would seem more justified to include all of these data if other consideration of these as potentially controlling factors on gas fluxes were made (as was done for WFPS).

Figure 2: the small symbols make it a bit difficult to see specific patterns even where significant differences are indicated.

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