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Biogeosciences Discussions

Interactive comment on "Long-term nitrogen addition decreases carbon leaching in nitrogen-rich forest ecosystems" *by* X. Lu et al.

X. Lu et al.

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Response to Anonymous Referee #2

We would like to thank the Anonymous Referee #2 for the comments on our manuscript. Accordingly, we have revised the manuscript. Details please see the following responses to the Referee's comments and the revised manuscript attached with the highlight (on the base of Response to Anonymous Referee #1).

General comments: This study provides solid evidence for an largely-ignored mechanism, reduction in soil DOC efflux, leading to soil organic carbon accretion in old-growth subtropical/tropical forests. I give a high applause to the authors for their contribution in proving this mechanism.

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Interactive Discussion

Discussion Paper

[Response: We deeply appreciate these kind and positive comments.]

Specific comments: However, I do not understand why the High-N treatment would lead to high soil water down-ward efflux as compared with the Low-and Medium-N plots. The authors did not present the zero-tension lysimeter water data. But the reduced DOC efflux in the High-N plot combined with the lowest DOC concentration in the High-N plot suggests an increased water efflux in the High-N plots as compared with the Low- and Medium-N plots. I would like to see an explanation in discussion by the authors.

[Response: We have noted the higher water down-ward efflux in the High-N treatment plots than that of the Low-and Medium-N plots, and the lowest DOC concentration in the High-N plot. However, this did not mean that High-N treatments increased water efflux. As described in the manuscript, we established the field research site in this (sub)tropical forest, with plots and treatments randomly selected. Considering the natural conditions, it was unavoidable that there existed some variations among treatment plots. However, there was no statistically significant difference for water effluxes among treatments ($P>0.1$, Tukey's HSD test). Importantly, N-addition-induced changes in water efflux are a long-term hydrological process, which merits us to further study in the future. It may be hasty to draw a conclusion that High-N treatments increased water efflux in a short period like this study.

Meanwhile, we have found that N-treatments significantly decreased DOC concentration, especially in high-N plots, compared to that of controls. Hence, we primarily attributed the decrease of DOC efflux to the decreased DOC concentration. The aim of this study is to explore how experimental N addition affects DOC dynamics. Hence, we would like to compare the differences between N-treatment plots and the Controls.

We have added the above information in discussion. Also please see Page 16, lines 399-402 in the revised manuscript.]

I am not convinced that many tropical forests are N rich ecosystems. The citations

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listed by authors usually do not show higher N content in plant tissue or in soil total N. Instead, the cited studies generally refer to that the productivity of tropical forests is relatively more P limited than N limited as shown in temperate forests-By no means this is suggesting that tropical forests are N rich systems as a generalization. I guess that the authors can modify the statement as “many tropical forests are relatively N rich ecosystems as compared with P availability”. In fact, tropical moist forests with high precipitation can have high N leaching rate, and consequently have low N availability when N fixing plants are missing from the top canopy. Unless someone identify a common source of N input, I am not ready to accept the generalization that tropical forests are rich in N. In this study, the authors showed unchanged C/N ratio in soil WDOC, suggesting no signs of luxurious consumption of N in the N fertilized plots, consequently that forest is not over-dosed with N yet.

[Response: Thanks for these good suggestions. We agree with your statement that “many tropical forests are relatively N rich ecosystems as compared with P availability” and have added this information in the revised manuscript (e.g. Page 4, Line 86).

In our study, although soil C/N ratios were unchanged under N treatments, there were increasing trends in both total soil N and C contents with elevated N addition, and planned contrast analysis showed that N treatments significantly increased soil N and C contents, compared to that of Controls. Hence, unchanged C/N ratio should be led by the coinstantaneous increases of N and C. This does not mean that the studied forest is not over-dosed with N yet. On the contrary, our former studies in the same forest showed that N additions greatly inhibited litter decomposition (Mo et al., 2006, Plant and Soil, 282:135-151), reduced soil respiration (Mo et al., 2008,Global Change Biology, 14:403-412) and enhanced both organic and inorganic N leaching (Fang et al., 2009, Ecosystems, 12: 33-45). Also, we found that exceptionally high rates of N addition (50-150kg N ha-1yr-1) significantly decreased plant diverity, and none of the species present in the understory took any advantage from being fed with additional N, which offered experimental proof that plants in this studied forest are not N-limited

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(Lu et al., 2010, Global Change Biology, 16: 2688–2700). In additional, the region surrounding DBR has been experiencing high rates of atmospheric N deposition (21-38 kg N ha-1 yr-1 as inorganic N in bulk precipitation) at least since 1990's (Huang et al., 1994; Zhou and Yan, 2001; Fang et al., 2008). Hence, we are sure that our studied forest is typical N-rich ecosystem.]

Technical corrections Can Oe materials pass through the 2 mm sieve? Or what is your operational definition of Oe?

[Response: Yes, we have sieved soils at 0-20cm to pass a 2-mm screen, including Oe materials. We have pointed out this in the revised manuscript "Field soil sampling and laboratory analysis"]

Do you have N data on atmospheric dry deposition?

[Response: We are very sorry that we did not determine atmospheric dry deposition because of technical difficulties. However, we have supported the N data on atmospheric wet deposition.]

Why increased DOC efflux in the High N plot as compared with the Low and Medium N plots?

[Response: Pleases see the responses above to "Specific comments".]

Remove the regression line and equation in Fig. 5a because there is no significant correlation.

[Response: As a comparison to Fig. 5b and to make it clear, we would like to keep the regression line and equation in Fig. 5a]

In the legend of Fig 2, delete "by" before "using".

[Response: Thanks. We have deleted "by" from the text.]

P/L

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1455/6 . . . for the protection of . . .

1455/9 . . . per year . . .

1455/18 According to 14C measurement of plant living tissues

1456/11 . . . deionized water . . .

1456/25 . . . date of collection. . .

1457/2 delete “and”

1458/20 . . . test for differences. . .

1460/8 . . . study period increased, but not significant, in the N-treatment plots. . .

1462/16 . . . 2009). Gundersen. . .

1462/18 . . . ecosystems. Liu. . .

[Response: Thanks. We have changed the above as suggested.]

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C1051/2013/bgd-10-C1051-2013-supplement.pdf

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