

Interactive comment on “Contrasting growth responses among plant growth forms to nitrogen fertilization in a subtropical forest in China” by Di Tian et al.

Anonymous Referee #2

Received and published: 16 January 2017

The paper describes how a subtropical forest system responded to three levels of N addition (0, 50 and 100 kg N ha⁻¹ year⁻¹). The authors describe the response for over and understory trees, saplings, shrubs and understory vegetation. In general the responsiveness of the system was limited, but larger overstory trees responded by increased growth (basal area), while other parts of the vegetation showed no increase in growth, or even showed suppressed growth. The main conclusion made is that different components of the vegetation respond differently to N addition and that this should be considered when effects of anthropogenic N deposition is evaluated.

Major comments

My opinion is that the text in large parts of the paper needs to be rephrased. The
C1

results needs to be much more carefully described and the authors should make an effort in making it more clear what differences that are statistically supported and what are not. Several of the main results discussed (e.g. that N addition stimulated growth of large trees and suppressed growth by small ones) is not supported by data. I agree that it is likely that the suppression of understory vegetation stems from increased light competition with a denser overstory, and that this was caused by N addition, But this is NOT reflected by any of the data collected by the authors. Perhaps, N addition increased leaf area or canopy cover of the overstory, and by this suppressed light conditions and the growth of the understory? Such effect would over time be expected to be reflected by increased basal area but the limited duration of the experiment (3 and not 4-year as claimed in the text) may have been too short the capture such response. If there are any data on canopy cover or light transmission to the ground level, such data would definitely be worth exploring as it may help explaining the results.

The addressed questions could easily be made a bit more sophisticated by asking for differences compared to the known response from other forest systems (e.g. temperate, tropical or boreal forests). This is partly related to how the available knowledge from other systems is described in the introduction (see comments further down).

The last part of the abstract can be misleading as the result presented only supports that small trees grow better under ambient N than elevated N and there is actually no support at all for higher growth of large trees under elevated N.

The last sentence of the abstract, i.e. the conclusion of the study/implication of the results is extremely vague as the reader is not provided with any clue to why it is important to consider more parts of the vegetation than just the trees. A hint may be given by the results presented, i.e. that large trees responded differently from other parts of the vegetation, but the authors never help the reader describing why this is problematic of what can happen if the response is just evaluated based on the trees (large trees).

I miss information on whether the growth in study system in general is N limited. In my opinion this is essential information when it comes to evaluating the response to the N addition. If not, or if the growth in the system is co-limited by other nutrients, a lack of N response should be interpreted a bit differently than if N is the solely or main limiting nutrient. I believe that this is important as the response to the N treatment in general was rather weak and most often non-significant. In fact the additional data presented on P addition (Fig. 6) might suggest that P is co-limiting nutrient.

L. 43-53. The authors seriously exaggerates the lack of knowledge, and I would go so far as saying that the content of this paragraph gives a false picture the available literature on N effects in forested systems. First, studies from boreal areas are not at all limited to tree response. In fact there has been much other work done, both on other plant groups and on other organisms than plants. For a quick overview see the summary paper by Bobbink et al 2010 (that is cited elsewhere in the ms). Second, the authors claim that the response of forest understory communities rarely have been studied, which is simply not true. Just a few examples are van Dobben et al. 1999 (For Ecol Manag 114, 83–95); Strengbom et al 2001 (Funct Ecol 15, 451–457); Gilliam 2006 (Journal of Ecology 94: 1176–1191), and there are many more.

L. 110-118. I can understand why you exclude trees that died, and understand why trees that had decreasing DBH were excluded (but not necessarily agree that they should be excluded, as you then only accept measuring errors in one direction but not the other), but how can you justify excluding trees that showed no change in DBH? I am very worried that by omitting trees that showed no change in DBH may have seriously have influenced the results of your study and risk exaggerating the positive response that the N addition may have had.

The authors should in general be much more careful when presenting non-significant differences. If these at all should be mentioned it should be absolutely clear to the reader that these are non-significant differences. Much of the discussion, and even parts of the major conclusions, deals with non-significant differences that are presented

C3

as if they were statistically supported (e.g. L. 204-205, 210-212, 252-256).

Specific comments

L. 22-23. There was no response at all of the larger trees! Avoid bringing up differences that are far from significant in the abstract. This is not just wrong, it is misleading!

There is no description on how, when and why P was added in some plots.

L. 139-140. Do you have pre-treatment measures supporting that the vegetation was homogenous among plots at the initiation of the experiment? If so present these in a simple form. If not you should describe how the homogeneity was assessed.

Table 1. It is not clear what the data represents. Are the numbers presented grand mean across all treatments? If so this should be clearly stated in the text explaining the table.

L. 157-158. I do not understand the results described here "The basal area and RGR of trees at the community level showed no significant response to N fertilization (Fig. 1); however, the increase rates of basal area were likely hindered by N fertilization (Fig. 1c)" What does this mean? As far as I can see from the statistical results presented and the data presented in Fig 1 there is just simply a lack of N response. Very unclear what you mean when saying that growth was hindered by N addition?

L. 161-163. Be more careful when presenting non-significant differences. There might be a tendency towards more dead biomass under N addition but the difference is far from significant.

L. 164-165. The text here is wrong here. This result has nothing to do with the N treatment. The test and the fig just describes that basal area and RGR differed depending on size among individual trees of this species. This is very important as it seems like part of the conclusion is based on that there is a N effect here.

L. 168-173. The text here is in most parts misleading. The only effects that are sup-

C4

ported by the data presented is that the smallest trees growing under no N addition had higher basal area and higher RGR than small trees growing under N addition. All other differences that may or may not be visible in the figure is far from statistically supported and should not be mentioned here in the results.

L. 175-179. Is the test result presented in the fig correct? According the test results N addition influences RGR and mortality, but from the post hoc test there seem to be difference among the groups. From inspecting the data presented in the fig I wonder if there is some error among the letters indicating the differences among the groups in panel c and d.

Results covering the data presented in fig 6 is missing from the result section

L. 192-194. What is the rationale for expecting a common positive response for all types of plants? To me this seems a bit naïve, given that forest plant communities often are size structured communities (see e.g. papers by Peter Grubb), and understory species than can be expected to be light rather than nutrient limited.

L. 204-205. The first part of the sentence (large trees) is NOT supported by the results.

Minor technical and language errors

The text is in need of some language edition. I just provide a few examples were the text need some re-phrasing. I have not paid that much attention to text editing as I believe that the paper need to be substantially revised before the paper can reach an acceptable standard.

L 21-22. ...the small trees with DBH (diameter at breast height) values of 5-10 cm were hindered by N fertilization...In what way was the small trees hindered?

L. 23-24 ... Small trees, saplings and particularly understory shrubs and ground-cover ferns suppressed seriously by increasing N fertilization... How are the suppressed? I am not very fond of the wording seriously as it is not a neutral wording. Better describe how large the difference was.

C5

L. 24-24. ...Proportion of mortality? Here it is better to write either the mortality of plants were... or The proportion of plants that died...

L 177. Avoid evaluating your results in the result section by using wording such as severely here. Save that type of wording for the discussion.

L. 180-185. There is no need to present mean values in text if these are shown in the figure 5. Do not present data twice, choose either to present then in text or in the fig.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-416, 2016.

C6