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Discussions

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 #1

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This paper describes the results of a 3 year (authors say 4 in the abstract) forest N fertilization study conducted in China. The study focuses on growth of trees, saplings, shrubs and understory growth and mortality. The authors main conclusion is that N fertilization affects the various plant growth forms in different ways, with the smaller plants being most affected. Overall, this paper adds to the growing knowledge regarding N impacts on forest ecosystems, but suffers from many of the limitations that other fertilizer studies have to deal with 1) environmental relevance of the dosage amount and form, 2) short (3 year) period for assessment and 3) no data to support the mechanisms of the observed impact. Further, the study has low replication (3 20 x 20 m plots) per treatment. My suggestion is that in the revised paper - these limitations should be fully

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Discussion paper

addressed and evaluated with respect to the implications for the overall conclusions made by the authors.

Specific comments

1. Environmental relevance - The application rates of 50 and 100 kg/N/ha are very high and I suspect are found in a few locations in China, but not likely widespread. My experience is that such high dosages almost always produce some effect but 1 year of 50 kg/N/ha is not the same as 5 years at 10 kg/N/ha. The authors should read a very good paper by Lovett and Goodale (2011) Ecosystems - that discusses this issue. Further, if my math is correct the authors are applying 100 kg N in 12 dosages per year, each time in 15 L of water. This makes 8kg N per time - dissolved in 15L, which is about 440g/L. Given the reportedly greater impacts of the treatments on the ground species, I am wondering about the direct effects of this spray? This should be discussed/evaluated.

2. This is a short study (3.4 years - should be consistent throughout which it isn't at present) with relatively low replication. In both instances real changes may be occurring but statistically they are not different among treatments. Throughout the paper the authors refer to differences among treatments - when in fact they are not significant (e.g. Figure 3). Over time or with more replication it could be true - just as equally it may still be noise in the system. The authors are guilty of talking about differences when in fact they statistically the same.

3. The main argument for the difference in response among growth forms is shading. There is no evidence for this presented in the manuscript (not measured). Equally, there is no evidence for statistical differences in N content among treatments (supplementary info). Thus while the authors present a mechanistic reason behind the differences there is no real statistical evidence to support these claims. Changes in canopy cover were not assessed and N or P (nothing else shown) are not significant among treatments. Soil pH is lower, but Al or Mn are not measured. I found the discus-

sion section (4.3) very misleading for example - "total N content of soil was enhanced by N fertilization and P concentration in plant leaves and in fine roots showed that N concentration increased" - not only is this a poor sentence, it is factually incorrect - N content did not increase in the 50 Kg N treatment nor did N content significantly increase (Figures are actually labeled incorrectly). Similarly there is no evidence of P being lowered by the treatment (soil or plant). Why was nitrate or ammonium not measured?

4. The P fertilizer study added at the end reads just like an add on and does not help the paper and it should be deleted. Similarly the text on lines 243-249 could be deleted.

5. The data shown in Figure 2 - basal area changes over time by size class are self-evident and this could be deleted. I am much more interested in how size class distribution compared among the study plots at the beginning of the study period. With such low replication (~40 trees per plot = 120 trees per treatment, which then get broken down into smaller units - some of these comparisons may be being made on a very few trees).

As addressing these comments should alter the paper substantially I will not comment on editorial issues.

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