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Biogeosciences Discussions

Interactive comment on "Interactions between leaf nitrogen status and longevity in relation to N cycling in three contrasting European forest canopies" *by* L. Wang et al.

Anonymous Referee #1

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This paper addresses the issue of interactions between leaf nitrogen status and longevity in relation to within canopy and within ecosystem N cycling in three different forest types. The authors set out to test three hypotheses: - Deciduous forests will distribute N in relation to photosynthetic activity more than long lived coniferous forests - Deciduous forests will re-translocate a larger proportion of their internal N than long lived coniferous forests - N pollution leads to an opening of the N-cycle.

These objectives are in the scope of biogeoscience and fit well within the special issue "Nitrogen and Global change". I raise however some questions that needs to be addressed.

The paper is concise; however some parts are a little difficult to follow especially the description of the calculation of canopy N content and N re-translocation from senescing leaves and needles in the Materials and Methods (page 9764). Perhaps a presentation in formulas on in a table format might be more helpful for the reader to follow. In addition, it is not very clear in these calculations which parameters were derived from literature, which were deduced from measurements done within those experiments and which are directly measured in this experiment. I would suggest explicating this for all variables presented in table 2 page 9785

To apply those calculations, the authors made the assumption that the forest canopy is divided in two homogenous layers (top and base). This is in contradiction with one of the original hypotheses stating that forests will distribute N in relation to photosynthetic activity and therefore having it divided in two homogenous layers is an over simplification. I acknowledge that this simplification might be necessary for such calculations. Some clarifications are however needed: how was the limit set between the two layers? How was the LAI calculated/measured for each layer? Is it a dynamic variable (changes with time) or a constant value is chosen for the whole calculation? Since the canopy N content is the product of LAI and the N content per unit leaf area, the LAI therefore modules the calculated canopy N content and it would be useful to have the values shown and commented if those were dynamic.

The authors evaluate the N pollution at the different sites by giving the average NH3 concentrations (page 9763 lines 9-14); a much better indicator of N pollution is total N deposition which would account for wet/dry and oxidized/reduced forms of N pollution. The authors state that bulk tissue gamma was calculated as an indicator for comparison of NH3 exchange potential among different tree species (page 9767 lines 8-10). Can you cite any references on that? I don't believe there is much evidence in the data given to link the bulk tissue gamma to the potential NH3 exchange or to atmospheric N pollution. It would have been necessary to have more information on atmospheric N pollution on the site and on the NH3 exchange of the exchange either via stomatal

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gamma measurements or NH3 exchange flux measurements. It is however an interesting variable to look at in terms N partitioning within the canopy.

The measurements and results related to chlorophyll a and b concentrations are a little outside the main objectives of the paper, they are poorly discussed especially in relation to N measurements and the 3 hypothesis posed in the introduction. I would suggest discussing them in relation to that if possible or removing them.

The paragraph on N re-translocation efficiency page 9769 should be moved to the discussion.

Interactive comment on Biogeosciences Discuss., 9, 9759, 2012.