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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 #2

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Review of Biogeosciences discussion 9, 9759-9790, 2012, by Wang et al., Interactions between leaf nitrogen status and longevity in relation to N cycling in three contrasting European forest canopies

General comments

This paper reports interesting results on how three tree species (one deciduous, two conifers) differ in terms of functional acclimatization of tree internal N conservation, and how this may affect nitrogen cycling in these forest stands. The authors go well beyond simple measurements of leaf chlorophyll and nitrogen in different leaf age classes but extrapolate their data to forest canopy N dynamics. This allows deciphering the two

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major controls of tree internal N conservation, i.e. foliage longevity and canopy N retranslocation, with trade-offs with peak standing canopy N and N transfer to the forest floor via leaf litter fall. The paper therefore makes a significant contribution to the field of forest ecology (biogeochemistry and N cycling) and the physiological ecology of trees. The scientific approach is in most topics valid (but see comments on bulk tissue measurements of ammonium and H+ below). The discussion concise, sets the data in relation to current knowledge, and opens up new interesting avenues to study and model tree trade-offs concerning tree internal versus ecosystem N cycling. The results are concisely presented and conclusions are clear. and therefore shall be accepted for publication after minor revision. 1. Does the paper address relevant scientific questions within the scope of BG? Yes, increasing atmospheric deposition of reactive N and its effects on forest N cycling is of major importance in the industrialized world. Understanding the mechanisms of tree internal versus ecosystem N cycling is therefore a pre-requisite for future silviculture and to model the future behavior of tree and forest health. 2. Does the paper present novel concepts, ideas, tools, or data? The data are novel, linking tree physiology (e.g. foliage longevity) with ecosystem level processes such as internal N recycling and litterfall. 3. Are substantial conclusions reached? Yes, see above. 4. Are the scientific methods and assumptions valid and clearly outlined? Yes. 5. Are the results sufficient to support the interpretations and conclusions? Yes, but it would be highly interesting to see how foliage longevity scales with the ratio of tree internal versus ecosystem N cycling. Is there a trade-off between both? 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes. I only could not trace how the ratio between the fraction of tree internal N cycling and ecosystem internal N cycling was calculated based on data presented in Table 2. This should be clarified, either in the text or the numbers given in the table with a footnote on how the authors performed this estimation. 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes. 8. Does the title clearly reflect the contents of the paper? Yes. 9. Does the abstract provide a concise and complete summary? Yes. 10. Is the overall presentation well structured and clear? Yes. 11. Is the language fluent and precise? Yes. 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes. 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See point 6. 14. Are the number and quality of references appropriate? Yes. 15. Is the amount and quality of supplementary material appropriate? Not applicable.

Specific comments

p. 9761, line 8: It is not only atmospheric deposition of NH3 but also of NOx and wet deposition that caused the critical N loads to be exceeded in many European forests. Values for total N deposition (wet and dry deposition) should also be presented in Table 1. p. 9762, line 13: what does it mean that "deciduous species will distribute N more closely...". The meaning is not clear. p. 9762, line 16: the hypothesis that N pollution may lead to an opening of the N cycle is impossible to be tested with the approach and study design. Atmospheric NH3 concentrations varied across sites but with that as well tree species and major climate parameters changed. The hypothesis could only be tested based on the same species across an N deposition gradient with largely the same climate and soil type. p. 9763, lines 10-12: total N deposition estimates should also be presented here and in Table 1. p. 9772, lines 1-end: it is a pity that (bulk) É¿bulk were not compared to (apoplastic) É¿apo in the same material though the former is easier to measure, or that no measurements of foliar NH3 exchange were performed to set É¿bulk in relation to NH3 exchange. Moreover, measuring bulk tissue NH4+ and H+ leaves the question open of the subcellular localization of both. The authors should shortly discuss this. I guess that the major part of both, NH4+ and H+, is stored in vacuoles, e.g. Wang et al. (1993) reporting 72-92% of root NH4 in rice being located in the vacuole, Lee and Ratcliffe (1991) showing 5-10 mM NH4 in cytosol of maize roots and 15 mM vacuolar, and finally Kronzucker et al. (1995) demonstrating 0.05-8 mM NH4 in the apoplast, and 2-33 mM in the cytoplasm. Given

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the co-localization of both would be advantageous for using this proxy. p. 9774, line 8: canopy enrichment probably refers to throughfall N enrichment , but sounds strange. Please change. p. 9774, line 23: I would not term NO losses as volatilization but rather as gaseous losses, and include N2O and N2 as well which are more important quantitatively as gaseous N losses. chapter 4.4: The paper would further increase in significance given that the authors could demonstrate a trade-off between leaf longevity and the partitioning between N re-translocation (tree internal N cycling) and litterfall N (ecosystem internal N cycling), on basis of the ratio between the respective N rates (g N m-2 a-1). If possible the authors should compile data for this from the literature (only litterfall N data are missing) and present the relationship as Figure 5B. Conclusion: p.l 9778, line 3: How was soil fertility as mentioned here indicated. Probably not by N deposition, soil N cycling, soil N concentration, but by canopy Nc? The term "the most fertile stand" is not suitable since not mentioned or discussed or based on data. Please rewrite this part.

Technical corrections

p. 9761, line 25: please spell out nitrogen if at the beginning of a sentence. p. 9774, line 11-12: please rewrite to " at least püart of this enrichment can be accounted for by dry deposition".

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