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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.

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Authors' response to Anonymous Referee #1

We thank Referee #1 for the positive comments and constructive suggestions which we find useful for improvement of our paper. In the revision we have been able to address all the questions and to incorporate all the suggestions of Referee #1 as explained below:

Referee comment #1: 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 retranslocation from senescing leaves and needles in the Materials and Methods (page

9764). Perhaps a presentation in formulas or 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

Response: We included a more explicit description on how we calculated the canopy N content and retranslocation. The entire description, including some new Equations,is given in the supplementary pdf file and will also appear in the revised manuscript.

Referee comment #2: To apply the 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?

Response: We agree our approach to represent the average leaf N content by the average of the two sampled layers is an approximation and a source of uncertainty. This is now mentioned in the text: In these calculations it is assumed that the average leaf N contents were sufficiently well approximated by the two layers, which were investigated in this study. In summer 2005 the Nl concentration profile in the investigated beech forest was investigated in relation to cumulated leaf area above the sampled leaves. From this a more complete calculation of Nc was performed. The difference between the multilayer estimate and the one using the topmost and the lowest layer as predictor for the average Nl amounted to only 1%. The data of Meir et al (2002) that included vertical N distributions in canopies from 5 different forest tree species were with one exception, oak, linearly related with height. Unfortunately Meir et al. (2002) did not mention the vertical leaf area distribution and thus a similar quantitative test as above could not be performed.

Referee comment #3: 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.

Response: We used the peak LAI for these calculations. Recalling the equations above the leaf area of the individual layers was not estimated.

Referee comment #4: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.

Response: We agree with the reviewer and have revised the text by inserting dry N deposition estimates from the NitroEurope denuder network (Flechard et al., 2011). The dry deposition of total inorganic N that was deposited via gasses and particles was 30, 15 and 4 kg N ha-1 yr-1 for the forest sites in The Netherlands, Denmark and Finland, respectively.

Referee comment #5: 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 stomata gamma measurements or NH3 exchange ïňĆux measurements. It is however an interesting variable to look at in terms N partitioning within the canopy.

Response: In the absence of stomatal Γ values, bulk tissue Γ can be considered as a potential NH3 exchange indicator in grass species (Mattsson and Schjoerring, 2002; David et al., 2009; Mattsson et al., 2009) although the parameter obviously is not a

direct measure of the NH3 exchange between plant and atmosphere. The relevance of bulk tissue Γ values in in tree species has previously only been tested in one case (beech; Wang et al., 2011) and showed limited value as a bio-indicator for NH3 exchange. There are indications though, that bulk tissue values may be important bio-indicators for N turnover processes, particularly during incipient senescence where concentrations of NH4+ may increase in parallel in both leaf tissue and apoplastic solution (Mattsson and Schjoerring, 2003). There are is also evidence that bulk leaf litter NH4+ and H+ concentrations may be more important for the NH3 exchange than the corresponding apoplastic value (Nemitz et al., 2000). Bulk tissue values therefore still have some relevance for characterization of nitrogen pools and their relation to overall nitrogen dynamics during the growth season Comparison of tissue ĐŞ values across tree species is a novel aspect of the present work and the results (Fig. 4) actually indicate some quite substantial differences reflecting the differences in the N status of the vegetation. As also pointed out by the reviewer this observation is interesting in terms of the N dynamics and N partitioning in the different species.

Referee comment #6: 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.

Response: Chlorophyll is an essential nitrogen containing compound and a valuable indicator of the physiological activity and degree of senescence of plant tissues the developmental stage and leaf age of leaves. The ratio between chlorophyll b and a also provide information about the integrated sun-shade regime of leaves in different layers within the canopy. Since these are important aspect of our paper we would like to keep the results in in order to provide readers with information about the overall seasonal pattern. To address the comments of the reviewer we have made the relationship between chlorophyll contents and the main objectives of the paper clearer in the text.

Referee comment #7: The paragraph on N re-translocation efïňĄciency page 9769 should be moved to the discussion.

Response: We agree with the reviewer that lines 2 to 14 on page 9769 can be moved to the Discussion section. This will be done in the revised version of our paper.

References David, M., Loubet, B., Cellier, P., Mattsson, M., Schjoerring, J. K., Nemitz, E., Roche, R., Riedo, M., and Sutton, M. A.: Ammonia sources and sinks in an intensively managed grass-land canopy, Biogeosciences, 6, 1903–1915, 2009.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C6490/2012/bgd-9-C6490-2012-supplement.pdf

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