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Biogeosciences Discussions

Interactive comment on "Input and output of dissolved organic and inorganicnitrogen in subtropical forests of South China under highair pollution" *by* Y. T. Fang et al.

Y. T. Fang et al.

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Authors answers to Anonymous Referee # 4

Answers to general comments:

1: First, I don't believe that sufficient measurements were taken to make such bold statements about the lack of importance of dry deposition at this site (e.g., abstract, p. 8, p. 12, p. 19). Dry deposition may make just a small additional contribution to bulk deposition compared to wet deposition alone, but bulk deposition does not capture all of dry deposition - especially NH3 gas inputs, the form likely to dominate in an agriculturally intensive area. Throughout the manuscript, please clearly indicate what sort of deposition is what (wet-only, bulk, or wet + dry). This concern does not

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take away from the significance of the manuscript, however, since total (wet + dry) deposition rates are likely even higher than reported here.

Answer. We agree that the statement is too bold. What we were thinking was that compared to European conditions dry deposition (e.g. Kristensen et al. 2004) is minor at this site. We will clarify as detailed in the following: Dry deposition, especially the gaseous forms of N, is the most difficulty to quantify in N biogeochemistry. Because of that, we tried to use both the wet-only (catch only wet deposition) and bulk collections (catch wet + particulate phase of the dry deposition). In addition, we have measured both precipitation N input and throughfall N input. One way of gaseous N deposition is through canopy uptake and subsequent leaching from the canopy, leading to higher throughfall N flux than the precipitation N flux (e.g., see Lovett et al. 2000). The net canopy uptake of N in younger forests suggested (i.e., throughfall flux less than precipitation flux) at least in the humid, high precipitation regions of tropics and subtropics, the deposition in precipitation (wet) is catching most of the "depositable N" in the air. Consequently, we think it is OK to conclude that "At the high humidity and frequent rainfalls in the wet season of this region wet deposition is likely to dominate the total atmospheric deposition (Ch 4.1)". We agree to take the sentence "Dry deposition was of minor importance at the site" out of the abstract, and to "tone down" in several other places where dry deposition is discussed.

2: Throughout the manuscript, please clarify what is meant by surface runoff from these plots. Is this overland flow? Streamflow?

Answer. Surface runoff is overland flow as described in Materials and Methods. We added that information in revision to clarify the issue.

3: I suggest some caution and clarification in the discussion comparing these N budget results with results from Nadelhoffer and others (1999), who used 15N tracers to track the fate of N in temperate systems. Simple mass balance and 15N studies can yield different estimates of where N is retained due to real time lags in N uptake and turnover.

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Answer. Yes we agree that there is a risk that the conclusion by Nadelhoffer et al. (1999) may rely on a short term effect and have changed the citation to be Nadelhoffer et al. 2004 (Forest Ecology and Management, 196: 89-107), where the N-15 fate was analysed after a decade, nevertheless organic soil was still shown to be the largest sink for 15N added seven years after the end of 15N additions (ranging from 34 to 83% of additions).

4: The old-growth stand may well be losing more N than it receives (p. 16, top). Some alternatives should be mentioned, too: inputs could be underestimated somewhat due to lack of dry deposition measurements, or if these drier-than-normal years provided less N in wet deposition than typical (i.e., the site could be in rough balance over a longer time period). Also, "exports" may be overestimated, since measurements here are just the flux below 20 cm, not total loss from the system. The case that the old-growth systems may be a source rather than a sink for N would be made stronger, should these minor concerns be addressed.

Answer. Our study includes data from two years. Thus we can of course only conclude that for this period the old growth seems to be a source. The seasonality and rainfall, however, differed over the two years, but still results showed the same trend. Thus we do not think underestimation of input will be enough to explain the negative N balance. The fact that we used 20 cm as the depth for the outflow balance may overestimate the leaching loss as explained by the reviewer. However, we did not find a major difference in soil solution N concentration between at 20 cm and at 40 cm for a period where both were available (addressed in the methods section) We therefore believe that it is safe to conclude that the old forest has been N-saturated. "It might result from 'mining' of the pre-existing organic N (McDowell et al. 2004) and/or reduced N uptake by vegetations (see below)" will be added in the text (also see the answers to referee #3). Based on the comments we have made several changes to the text and tried to be specific by several times mentioning the depth was 20 cm for leaching loss measurement. In the conclusion (p.20, line 2) we changed the wording to emphasize that there is "no

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retention" and an indication of a net loss.

5: Finally, I suggest caution and clarification when comparing DON export from below 20 cm with values from the literature derived from catchment-level estimates (p. 19-20). Significant quantities of dissolved organic matter are typically retained in soil below 20 cm depth through sorption or decomposition processes. Measurements from 20 cm depth may well overestimate total ecosystem DON loss.

Answer. It is right that significant quantities of dissolved organic matter could be retained in soil below 20 cm depth through sorption or decomposition processes. But we believe it may be minor at our sites, compared to the large DON fluxes we observed. However, in the revise version, we will add a sentence "although DON might be further adsorbed in underlying mineral soils and be processed near or in stream" to address reviewer's concern.

Answers to Specific/Technical Comments

All but the following, will be corrected to follow the reviewer's suggestions:

1 p. 5, middle. Basal areas of 26 m2/ha seems surprisingly low for an old-growth humid temperate forest, as does 14 m2/ha for a 60- to 70-year-old successional forests.

Answer. There are some big and old trees in the forest, but which are not included in our plots. This is because that our plots measured by 10 m * 20 m and it was not practical to include the very big trees since we trenched around the plot to isolated our plots. These could make basal area for the forest underestimated. We will add a sentence in the revision to address this issue: "Basal area in the old-growth forest might be underestimated because particular big trees were not included in our plots".

2 p. 9, line 7 and beyond: "precipitation" chemistry is from wet deposition or bulk deposition?

Answer. Since there was no significant difference between wet-only and bulk deposition, we have pooled data together.

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3 p. 9, mid-page. "Unusually" high NO3 after a long drought is perfectly reasonable as reflective of accumulated dry deposition. These values ought to be used in quantifying throughfall; this is not a good reason to use precipitation values instead of the measured throughfall values.

Answer. The possible contamination of throughfall samples in the dry season led us to decide using precipitation data as a precaution. We have addressed the possible underestimate of N deposition in the text.

Interactive comment on Biogeosciences Discuss., 4, 4135, 2007.