

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2022-72-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2022-72

Anonymous Referee #1

Referee comment on "Forest-atmosphere exchange of reactive nitrogen in a remote region – Part II: Modeling annual budgets " by Pascal Wintjen et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-72-RC1>, 2022

This study provides dry deposition estimates of total reactive nitrogen at a mixed forest site using four different measurement and modeling methods. Annual and seasonal concentrations, dry deposition velocities and fluxes were discussed and compared between different methods. The study provides a useful dataset and some useful findings in terms of modeling uncertainties using different approaches. The presentation quality needs improvement as detailed below.

Specific comments:

The second paragraph of the Abstract needs to better summarize major findings.

Lines 20-21: this sentence is not clear, hard to tell which flux number is from which method. You mentioned DEPAC-1D method in this sentence, and the next sentence provides a flux number again using this method.

Lines 24-27: while v_d parameterization certainly needs some improvements, partitioning among the different Nr species in the total Nr budget might be the dominant factor for the temperature-dependent v_d in this case. This issue can be discussed more in section 3.2, but this statement may not be needed in the Abstract because such a statement provides little useful information.

Lines 31: It is better to first present the dominant N species in the measured concentrations (and Nr flux if available) before discussing modeling results.

Lines 36-37: this single sentence should not be a separate paragraph; it can be presented in the first paragraph of the Abstract or at the end of the Introduction.

Section 2.1 and line 131: I assume most of the materials in this section were already reported in your previous study. If this is the case, this section can be simplified since the manuscript is very long. The same recommendation applies to other sections where applicable.

Line 136: explicitly show which species (only NH₃?) uses the bi-directional approach.

Line 138: "as" should be "and".

Line 139: this part is not clear.

Line 169: why choose 2.5 m knowing that the measurement is at 30 m? In most regional scale CTMs, dry deposition is typically calculated at the mid-layer height of the first model layer (typically at 10-40 m) while some model may also use 10 m.

Line 170: most CTMs would have many more NO_y species than listed here (PAN, MPAN, etc.). Are these species not available in this model?

Line 182, Are the resistance formulas in DEPAC-1D (Section 2.2.3) the same as those in DEPAC (Section 2.2.1)?

Lines 171 and 187: This is a size-resolved vd model. What size distributions were used for particle nitrate and ammonium when calculating their vd?

Simplify Section 2.2 if possible since the manuscript is very long and the description here is a bit too long.

Section 2.3: Is direct DON wet deposition considered in this budget? You may also want to briefly mention the important contribution of the dry deposition of organic N in the total N_r dry deposition budget somewhere in the manuscript.

Section 3.1: With too many details (such as seasonal high and low values), but lacking of a summary of the big picture, makes the section difficult to read. I assume you can take the measured concentrations as the benchmark, and then evaluate model performance. Then you should first present the annual (and seasonal where needed) average concentrations for each N species from the measurements (this way the dominant species and their relative contributions to the total Nr can be easily observed), then mention the model-measurement differences.

Figure 1: You may just show statistics in the main body of the paper and move the time series of the data to SI to avoid a too-crowded picture. The same comment applies to other similar figures.

Section 3.2-3.4: vd is compared in two sections while Nr fluxes are compared in three sections. Why not use one section focusing on vd comparisons and another section focusing on Nr flux comparison? Vd comparison can be very brief (knowing that vd modeling has larger uncertainties from literature, e.g., Flechard et al, 2001 cited in this study).

My comments above on section 3.1 also apply to other sections below. A general impression I have is that: there are too many details in the results and discussion, but a good summary of the major findings is lacking.

Sections 4.2 and 4.3: Discussions on methodology uncertainties are important in any studies, but detailed discussions on modeling uncertainties (and without firm conclusions) like the ones presented here seem to be out of the scope of the present study.

Liens 819-824: No need to repeat what you have done in the conclusion section.

In summary, the methods used in this study are valid, data analysis results are scientifically sound, but presentation quality should be improved for smooth reading.