Reply to Reviewer 1

The reviewer comments are in magenta, the replies from the authors in black and text in manuscript in italics.

General Comments:

This is a thorough and well-conceived paper describing efforts to use experimental observations to validate a terrestrial biosphere model, with the goal of evaluating how effectively this model describes ecosystem retention of excess nitrogen. The authors use results from a ^{15}N tracer experiment, where plant and soil N retention was measured three times over ten years, to evaluate the QUINCY model's ability to describe the movement of N through a temperate deciduous forest ecosystem.

The authors are quite thorough in their discussion of the model and how it relates to field observations over time, and do an effective job at explaining where mismatches between simulated and observed N pools occur. The science presented here is sound, and I do not see any critical issues in the methods or interpretation of results in this paper. The work described here is a solid contribution to the literature and should be of interest to BG readers.

My primary critiques are with the contextualization of the study and the results. While the bulk of the paper is spent discussing the model behavior in relation to field observations, there needs to be more attention given to the implications of this work, particularly in the Discussion.

We thank the reviewer for their positive and insightful feedback on the paper. We will do our best to improve the manuscript in line with the provided suggestions and pay closer attention to the implications of this work.

Specific Comments by section:

Introduction

- Lines 29-43: Authors should acknowledge long-term decreasing trend in N deposition in certain temperate regions due to air pollution legislation, particularly in Europe and North America. This is particularly relevant given the location of the study in the Northeast U.S. See Mason et al. (2022; https://doi.org/10.1126/science.abh3767) and related literature, including Craine et al. (2018; https://doi.org/10.1038/s41559-018-0694-0) and Groffman et al. (2018; https://doi.org/1007/s10533-018-0445-y).

This is indeed a very relevant point and we will address it in the revised version of the manuscript, using the references that the reviewer kindly provided.

- A little more background on the QUINCY model is needed in the introduction. We need to understand more about why this model is chosen. Is it the only model that

Some studies have developed a ^{15}N model to e.g. study tracer experiments (e.g. Müller et al, 2004; Liu et al., 2026). QUINCY is a relatively new model that has been developed with a particular focus on the nutrient cycles, and it can be run at multiple different scales. We will highlight these points in the revised text.

This is a good point. The background deposition at the site was previously included in Table 1, but we have now moved this information to the Methods section, as suggested also by the other reviewer. In addition, we made a plot of the N deposition input used in the model in the supplementary material.

There is a US Environmental Protection Agency long-term monitoring site located approximately 14 km north of the study site. N deposition has indeed decreased by about one third between 2000 and 2020, from 13.4 to 8.9 kg N ha^{-1} yr^{-1}. Compared to areas experiencing high levels of N deposition (e.g. the critical load for temperate forests has been estimated to be 10–15 kg N ha^{-1} yr^{-1}; Xie et al., 2024) , this level of deposition can be considered as changing from high load to moderate.

We will add these points to the Methods section.

This is indeed a good idea. Previous experience with Copernicus journals did not permit colors in tables, but if the journal guidelines now allow this, we will follow the suggestion.

We apologize for the unclear figure. To remedy this, we will increase the size of the panels and extend the lowest one to fill the two columns.

Yes, we will do this, if the journal's guidelines permit.

We completely agree with the reviewer and are very grateful for this suggestion. Section 4.1 was indeed lengthy and dense. For now, we have split it into two sections: temporal dynamics of the tracer signal in vegetation and soil.

- Line 297-298: Do you have a sense for why the model underestimated these C and N pools? If so, that should be stated.

The sentence in lines 297-298 was:

"*It is important to note that the model underestimated the C and N pool size of fine roots and C pool size of the leaves, and this might have some influence on the occurring dynamics.*"

The simulated size of leaf C pool is not largely underestimated (14 %), the underestimation of C pool is much more severe for the fine roots. The leaf C:N ratio is 23, which is closer to the lower end of the allowed C:N ratio for this plant functional type in QUINCY. The allowed range for the C:N ratio in QUINCY for this plant functional type has been estimated from the TRY database (Kattge et al., 2011) and is 13.7 to 38.7 (Miinalainen et al., 2025). In N limited conditions the plants could allocate more biomass to fine roots in order to increase N uptake from the soil, but even though the simulated leaf C:N ratio is lower than the observed, it is not close to the lower threshold and the influence in the allocation has not been noticeable.

The total plant C in observations is 11 140 gC m-2 and in the simulations 12 771 gC m-2, so they are relatively close to each other, with the simulations showing 15% overestimation. All the other plant C pools than the wood pool are underestimated by the model. This could be related to the fact that the simulated allometric relationships are not aligned with the observations.

Unfortunately, there was a small mistake in the original sentence: the model did not underestimate the N pool in the leaves (the observation was 4.0 gN m^{-2} and the simulated value was 5.3 gN m^{-2}). This has now been corrected. The N pool in fine roots was underestimated, though not by as much as the C pool in fine roots was underestimated.

We'll add discussion about this issue to the manuscript.

Lines 316-318: There should be some further explanation for this, as 2mm is typically the standard cutoff for fine roots. Do the authors believe that this impacted results at all? Either way, the effect of this discrepancy, or lack thereof, should be specified.

The reviewer is referring to the fact that the measurements had a cutoff of 1 mm and the simulation had a cutoff of 2 mm for the fine roots.

Both 1 mm and 2 mm are used as cutoff thresholds in the observations. The cutoff of 1 mm was chosen so that these observations would be consistent with earlier studies of the site (Goodale et al., 2015 and Goodale, 2017). Furthermore, it has been demonstrated (e.g. Frey et al., 2025) that <1 mm fine roots constitute the majority of fine root biomass and exhibit the greatest sensitivity to nutrient availability in this and similar forest types.

The discrepancy in the cutoff threshold between the model and observations could lead to the model overestimating the amount of fine roots and underestimating the amount of coarse roots. However, the model was underestimating the fine roots more considerably than the

coarse roots. (The ratio between coarse and fine root C was 3.0 in the observations and 9.3 in the simulations.)

The observations showed more modest turnover than the model simulations for the ^{15}N signal in the fine roots (Fig. 1b). In the observations the long-term behaviour between fine and coarse roots did not differ much, whereas in the simulations the dynamics in the fine and coarse roots are very different because of the model structure (Fig 1b,c). Because of this, we would not expect that the different cutoff threshold impacted strongly our results.

We will add these points to the revised manuscript.

- Lines 401-408: This presents the question of why you did not use the JSM rather than the basic soil model in QUINCY. The authors should provide a justification for not choosing JSM (benefits of simpler parameters, computational power, etc.)

JSM has been developed for studying detailed soil processes and while it can be a very valuable tool for understanding processes such as soil exudation (Schufft et al., 2025), using it requires detailed observations from the site. At this point we do not have enough data to evaluate the performance of JSM at the site (e.g. microbial biomass, microbial stoichiometry and CNP ratio at different depths of the soil profile). Also, it would be relevant to know the ^{15}N of microbial biomass and the mineral-associated organic carbon (MAOC) and dissolved organic carbon (DOC) pools in order to understand the processes and adjust the parameters.

We'll add this point to the text.

Conclusion

- This should be tightened up, as it reads too much like a restating of the discussion.

Thanks for this remark. We agree with the reviewer and will re-write the conclusion section by keeping this in mind.

- The paper ends abruptly and would benefit from a sentence or two summing up the primary findings, stating the implications of those findings, and suggesting future directions for this work (similar to what was stated in the abstract).

This is a very good point. We will add a couple sentences to end the manuscript with the content that the reviewer is suggesting.

Technical Corrections:

- Line 213: Typo, "The observations showed a smaller enrichment peak as the simulations", replace "as" with "than"

Thanks, we corrected this.

- Line 345 & 353: Typo, should be ^{15}N

Thanks for the remark. We have now corrected this.

- Line 346-348: Sentence is a bit confusing and contains a typo on line 347

The sentence that the reviewer is referring to is:

"*In the observations the top soil layers became more enriched from the incorporation of ^{15}N from the overlying litter layer, whereas in the simulated top soil was most enriched one year after the tracer application, and then became more depleted as the signal moved deeper to the soil and the vertical soil profile of the ^{15}N tracer started to get flatter (Fig. 3).*"

We apologize for the unclear formulation and typo. We have now modified the text to make clearer what was relevant for the observations and for the simulations.

"*In the observations the top soil layers became enriched because the ^{15}N from the overlying litter layer was incorporated into it. The top layers showed some depletion by 2018. The simulations showed larger changes in the top layers across the years and also the ^{15}N tracer started to move to deeper soil layers and the vertical soil profile of the ^{15}N tracer started to get flatter (Fig. 3).*"

References

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