We appreciate the helpful comments from the reviewers and the editor that have helped to improve the quality of our revised manuscript. Our response to reviewer and editor comments is included below in RED

RC1: 'Comment on egusphere-2025-1167', Anonymous Referee #1, 03 Apr 2025

General comments:

The authors present a modelling study that investigates the impact of using low-resolution concentration data for computing the ammonia dry deposition flux in a forest ecosystem in the Rocky Mountains National Park. Among the results, a key finding is that using the low-resolution ammonia concentration data led to an underestimation of the dry deposition flux. Additionally, a correction factor is derived, which can be used to mitigate this bias. The case study could provide an interesting continuation of the work by Schrader et al. (2018) but omits relevant methodological details and additionally requires extra proofreading.

We appreciate the reviewer's assessment and respond to individual comments below.

Specific comments:

Paragraph at lines 18 – 19: The wording "[…] from more commonly available input data was evaluated" is unclear. It would be clearer to directly state which data have been used instead – in this case, the bi-weekly ammonia measurements and the ERA5 meteorological data.

The text was updated to specify that we evaluated the impact of biweekly NH_3 measurements and ERA5 meteorology.

Line 27: Perhaps "NH_x (NH_3 + NH_4^+) emissions" instead of "NH_3 emissions".

Primary emissions of NH_x are gas-phase NH_3. Particle ammonium is formed through phase partitioning involving salt formation with acids in the atmosphere. Here, we are specifically talking about the direct emissions that lead to reactive nitrogen deposition.

Line 33: Eutrophication is not limited to lakes only, so consider omitting the word 'lake'.

Good point. Lake effects have been one of the major indicators of excess N deposition impacts for RMNP. However, to make it more generally applicable, we have removed the word "lake" to include all eutrophication effects.

Figure 1: Given the relevant mountain-plains circulation taking place in the Rocky Mountain National Park and its relevance to the NH_3 concentrations, this figure would benefit from an elevation map. Additionally, a scale should be inserted.

Figure 1 has been updated to include an elevation profile and a scale bar.

Section 2.2.1: The leaf-area-index (LAI) is an important variable in the modeling of NH_3 atmosphere-biosphere exchange and should also be mentioned in this section.

LAI was estimated using the NEON LAI spectrometer mosaic product, which is derived from remote sensing data. The main text has been updated to indicate where the LAI came from. An additional figure has been added to the SI to illustrate the LAI selection and show the spatial variability of LAI values in this area. A sensitivity analysis has also been added to the SI to give the reader some insight into the effects on NH_3 fluxes from changing LAI.

Section 2.2.1: This section would improve by shortly characterizing the typical meteorological conditions at the NEON site (e.g., average temperature, relative humidity, amount of rain days, etc.) as well as the average NH_3 concentration. Moreover, the number of days with snow might be relevant here, as NH_3 exchange differs when there is snow present.

Due to its high elevation location, the meteorology at the NEON tower in RMNP is highly variable. Additional context has been added to this section to give the reader a general sense of the typical meteorology in RMNP.

Lines 112 – 114: What is the name of the instrument measuring the friction velocity, and what is the temporal resolution of this instrument?

NEON calculates friction velocity (u*) using 3D sonic anemometers, which have a resolution of 20 Hz.

Section 2.3.1: Please mention the number of bi-weekly NH_3 concentration measurements that have been collected.

The number of biweekly sampling periods (27) is now included in the text.

Section 2.3.2: I am currently missing information on the quality control of the measurements. For example, when the atmosphere is stable, stratification of the atmosphere occurs, which can hinder accurate NH_3 concentration measurements as the atmosphere is not well-mixed. Additionally, similarly to the comment for Section 2.3.1, please mention the number of half-hourly NH_3 measurements made with the AirSentry and provide information on how many observations have been filtered, if any.

For NH_3 concentration, we could see lower concentrations at the surface due to stratification of the atmosphere. However, the exchange model used only considers concentration at the reference height. During our campaign, we also measured NH_3 gradients on the NEON tower which will be used in a later work to directly determine NH_3 fluxes. The observed gradients may give insight into the effects when NH_3 is not well mixed in the atmosphere. Elevated NH_3 concentrations are brought to RMNP by upslope transport, where the winds would contribute to atmospheric mixing. For quality control of NH_3 measurements, the number of AirSentry and passive measurements is now included. Only NH_3 data missing due to power outages have been removed from the AirSentry dataset.

Lines 145 – 147: Please provide the full form of the abbreviation 'NPS' when it is first mentioned.

The abbreviation "NPS" for National Park Service is now defined in the text, where first mentioned.

Section 2.3.3: I think that this section can benefit from a table summarizing the specifications of each of the three datasets (e.g., location of measurement, sampling type, sample size, temporal resolution, nomenclature) to both summarize the three different datasets and to guide the reader through the differences between the datasets.

We are concerned that some of the information listed may be confusing to the reader. For example, including "location of measurement" for the 30-minute data product implies that the raw NH_3 concentration was taken from the AirSentry location. A bulleted list in this section summarizes the data products and nomenclature. For the key modeling understanding, it is most important that the reader understands the respective time resolution and therefore impacts of time resolution on model results.

Figure 2: The caption of Figure 2 repeats text from the main body and could, therefore be omitted.

The caption of Figure 2 has been edited to remove repeated information. The definitions of each dataset are now included in the main text only.

Lines 170 – 171: Have you considered that the diurnal NH_3 concentration cycle at the NEON site could be different compared to the diurnal cycle at the NPS shelter, related to differences such as the physical location of the measurements and the vegetation type at the two sites? Regarding the latter, deposition velocities can be lower above grasslands compared to forests, given the lower roughness length z_0 of grasslands, which can consequently lead to higher NH_3 concentrations above grasslands. This section or the discussion should at least contain a more critical evaluation concerning the systematic differences in the diurnal cycle above grasslands and forest sites.

Yes, we did consider how the diel pattern of NH_3 could vary between the grassland site and NEON site. From August 23, 2021, to October 4, 2021, we deployed University Research Glassware annual denuders to measure NH_3 on the NEON tower. These data are compared to the raw AirSentry data and the AirSentry data scaled to the passive measurements in Fig S1. We find that the daytime NH_3 concentrations agree well between the sites. Overnight URG samples generally have higher concentration than what we observe in the AirSentry data. Additional discussion has been added to the main text to explain the potential differences.

Lines 230 – 232: For the sake of completeness, it may be helpful to include at least Eq. (15) from Massad et al. (2010).

Eq. (15) has been added to the text. The wording of the section has also been improved to clarify the modelled resistances.

Additionally, the canopy height at which the wind speed is measured is 11 m, while the mean canopy height mentioned in Section 2.2.1 is 19 m. This difference should be addressed to avoid confusion.

Thank you for catching this! The wind speed was taken from the top of the tower, not the canopy height of 19 m. The noted height of 11 m was a typo. The text has been updated to reflect the proper height of wind speed measurements.

Lines 239 – 243: Massad et al. (2010) provide corrections for the temperature and leaf-area index when calculating the cuticular resistance R_w, based on the findings by Flechard et al. (2010) and Zhang et al. (2003). For example, Schrader et al. (2016) also incorporate these effects in the R_w parameterization as shown in Eq. (5) in their paper. If you choose to omit the LAI and temperature coefficient from the R_w parameterization, that decision should be justified.

There are two subsections in Massad et al. (2010) that discuss Rw. In Section 2.2, Equations (3 – 4) include the T and LAI effects in Rw(corr). In Section 4.6, they proposed a generalized formulation for Rw(corr) (Eq. (24)), where the T and LAI effects are removed. We followed this generalized formulation, which does not include T and LAI. However, upon reviewing Massad et al. (2010) again, we found that Table 8 contradicted Equation (24). It appears that Rw(corr) from Equation (24) might be Rw, meaning it would still require T and LAI corrections. Since we do not have the original data used for the generalization, we cannot investigate which form is correct, and correcting the Rw parameterization is beyond our scope.

Line 250-251: Given the importance of the stomatal emission potential, it would be appropriate to introduce what emission potentials are. Moreover, please discuss how and from which equation the emission potential of 4 has been derived. This value does seem rather low.

The text has been updated to include a brief conceptual introduction for emission potential. The value of 4 for stomatal emission potential was used initially to match measurements from other regions with very low annual ecosystem N input. We have updated this value based on foliage measurements taken around the NEON site in RMNP. The updated stomatal emission potential is a weighted average of the species-specific emission potential and average land coverage at the site. An explanation of the sample collection is provided in the supplementary information. The stomatal emission potential now used for all simulations is 29.

Line 253 – 259: The parameterization by Massad et al. (2010) does not originally calculate the soil compensation point or the soil resistance ($r_g + r_{ac}$). Often, the exchange of gases with the soil is not taken into account in dry deposition schemes due to the overlying canopy, which will (re)capture NH_3. Moreover, Massad et al. (2010) state that very few data is available regarding ground layer emissions. Thus, please elaborate why NH_3 exchange with the soil is modeled here.

In Massad et al. (2010), χ_g is called the ground compensation point (section 2.4) and is included in the Eq. (12) calculation of χ_c. The text has been updated to make the nomenclature consistent with that described in Massad et al. (2010) to avoid confusion. R_g, R_{ac} and χ_g can be found in Fig. 1, schematic in Massad et al. (2010). You raise a good point that soil emissions could be recaptured by the canopy above. In Massad et al (2010), they suggest using a soil emission potential of zero in unmanaged ecosystems. However, this has more to do with the lack of soil measurements. In RMNP, we are fortunate to have measurements to base our soil emission potential on. Due to this, we have included the effects of soil in our analysis.

Line 262: z_0 is not the reference height but the roughness length. This mislabeling occurs more often, both in text and in figures.

The text has been updated to be consistent throughout and properly labels the reference height (z) and roughness length (z0).

Line 265: χ_a is mentioned here for the first time, so it requires a brief explanation.

An explanation of χ_a is now included in the revised manuscript.

Line 269: Connected to the comment at line 265, here, the ammonia concentration is denoted as [NH_3] instead of χ_a. For the sake of consistency, use a single notation for atmospheric ammonia throughout the manuscript.

The text and figures have been updated to use consistent notation for atmospheric ammonia concentration. In all locations, the atmospheric ammonia concentration is denoted as χ_a.

Moreover, the denominator contains the term "$\cdot 10^3$" which is not included in the original parameterization by Massad et al. (2010). Please specify why this term is included.

The term "10^3" was erroneously included in the text based on a necessary unit conversion for the model simulation results. It has been removed from the text to be consistent with Massad et al. (2010).

Lines 300-303: Can you be certain that the morning increase in NH_3 concentration at the site is mainly due to NH_3 evaporation from cuticular dew layers, and not also influenced by either the diel mountain-plains circulation transporting polluted air with NH_3 or NH_3 emission from the stomata?

Previous work from Wentworth et al. (2016) found that the timing of the early morning NH_3 emission pulse was temporally correlated with dew evaporation, not transport. The diel pattern of the mountain-plains circulation is typically later in the day than the observed dew emission. Additionally, the early morning NH_3 emission pulse was not observed on mornings without dew or during precipitation.

Section 3.2: I am confused here to what extent the same method of Schrader et al. (2018) is applied here. The method by Schrader et al. (2018) proposes a true average NH_3 flux formula (Eq. 9 in Schrader et al., 2018) when long-term average NH_3 concentrations have been used as input in a dry deposition scheme. Additionally, they provide a method to calculate this true flux, which requires the covariance between the exchange velocity v_{ex} and the atmospheric concentration χ_a to be calculated. If I understood your method correctly, you have run the dry deposition scheme with the 30-minute concentration data and the bi-weekly sample data and afterwards compared the slope, intercept, and the R^2 of the two different flux outputs – which is ultimately used to correct the fluxes. While both your methodology and that of Schrader et al. (2018) aim to correct NH_3 flux calculations based on low-temporal-resolution NH_3 data, the approaches themselves differ substantially. I recommend rephrasing this, as it currently gives the impression that you applied the exact same methodology, aside from the three exceptions noted in lines 344 – 346.

Thank you for pointing this out. You understood correctly; we aim to correct NH_3 fluxes from low-temporal resolution NH_3 data as did Schrader et al. (2018). However, we use the Massad et al. (2010) model applied at high time resolution, instead of an average NH_3 flux formula proposed by Schrader et al. (2018). The text has been updated to better reflect the similarities and differences between the two methods.

Finally, the average 30-minute concentrations from the AirSentry have been scaled to match the bi-weekly passive NH_3 concentration. There is a high chance that this will improve the R^2 and also affect the slope and intercept used for correcting the fluxes. Have you considered the effect this has on the efficacy of your method?

Yes, we did consider the additional impact that could be observed if the mean biweekly concentration was different between sampling techniques and locations. In particular, the observed difference in NH_3 concentrations above grassland and forest sites leads us to normalize the values to match what was observed above the forest. For this project, we wanted to specifically understand the impact of changing the time resolution and therefore decided to remove the additional complication of sampling technique variation in concentration.

Lines 363 – 366: Do you have an explanation for why the total NH_3 deposition is significantly lower using the 30-minute NH_3 concentration data compared to the unidirectional framework? Is this only caused by the inclusion of compensation points or, for example, by differences between the NH_3 and HNO_3 concentrations at the RMNP?

The total NH_3 deposition is significantly lower using the 30-minute NH_3 concentration data and bidirectional model compared to the unidirectional framework because of the inclusion of compensation points. For the unidirectional deposition velocities, we are using a fraction of the modeled HNO_3 deposition velocity, so the relative concentration of NH_3 and HNO_3 would not affect the relative deposition.

Line 395 – 397: "[…] but overestimate the annual NH_3 deposition flux by 59%". Please indicate which NH_3 deposition calculation is used as a reference here (i.e., either the HNO_3-based calculation of the unidirectional model or the total NH_3 deposition based on the 30-minute NH_3 concentration data).

The text now reads: "30-minute NH_3 simulations run with reanalysis data inputs are well correlated (R^2 = 0.77) with 30-minute NH_3 simulations run with in situ data inputs (see Fig. 11) but overestimate the annual NH_3 deposition flux", to indicate that the difference is based on the 30-minute NH_3 data and bidirectional simulations.

Additionally, while line 397 states an "overestimation" of the NH_3 flux when using the ERA5 meteorological data, I think this is supposed to be an underestimation of the NH_3 flux, as the deposition strength decreases caused by the higher R_a.

This is an important distinction, which we have made more clear in the text. Between the two meteorological input simulations, R_a differences reduce the magnitude of ERA5 simulations. However, when we consider the annual net effect, the change to negative fluxes is smaller than the change to positive fluxes. Therefore, the annual NH_3 dry deposition is overestimated by ERA5.

Technical corrections:

All headers: Titles should only contain capitalization for the first word and proper nouns.

All headers have been updated to remove erroneous capitalization.

Line 105 – 108: Ammonia should be written with a "3" in subscript (i.e., NH_3 instead of NH3).

The abbreviation NH_3 for ammonia has been updated to include the "3" in subscript for these lines.

Line 250: Replace "equation 10" with Eq. (10)

"Equation 10" has been replaced with Eq. (10).

Line 262: Replace "equation 15" with Eq. (15)

"Equation 15" has been replaced with Eq. (15).

Line 266: Replace "equation 16" with Eq. (16)

"Equation 16" has been replaced with Eq. (16).

Line 297: Fig. 11 does not have a subfigure 'a'.

Thank you. In the previous version, this figure contained 2 subplots. The 'a' has been removed when referencing Fig. 11 for clarity.

Lines 415-417: The phrase "Maximum R_a values from the reanalysis simulations are greater than an order of magnitude larger […]" could benefit from improved sentence structure.

That sentence has been reworded and divided into two sentences to make it clearer.

References:

Flechard, C. R., Spirig, C., Neftel, A., and Ammann, C.: The annual ammonia budget of fertilised cut grassland - Part 2: Seasonal variations and compensation point modeling, Biogeosciences, 7, 537–556, https://doi.org/10.5194/bg-7-537-2010, 2010.

Massad, R. S., Nemitz, E., and Sutton, M. A.: Review and parameterisation of bi-directional ammonia exchange between vegetation and the atmosphere, Atmospheric Chem. Phys., 10, 10359–10386, https://doi.org/10.5194/acp-10-10359-2010, 2010.

Schrader, F., Brümmer, C., Flechard, C. R., Kruit, R. J. W., Van Zanten, M. C., Zöll, U., Hensen, A., and Erisman, J. W.: Non-stomatal exchange in ammonia dry deposition models: Comparison of two state-of-the-art approaches, Atmospheric Chem. Phys., 16, 13417–13430, https://doi.org/10.5194/acp-16-13417-2016, 2016.

Zhang, L., Brook, J. R., and Vet, R.: A revised parameterization for gaseous dry deposition in air-quality models, Atmospheric Chem. Phys., 3, 2067–2082, https://doi.org/10.5194/acp-3-2067-2003, 2003.

RC2: 'Comment on egusphere-2025-1167', Anonymous Referee #2, 26 Apr 2025

While filling 5.8% of missing data using average diel patterns is pragmatic, this approach assumes temporal homogeneity in NH_3 behavior. The authors should quantify the potential error introduced by this method, especially during episodic events (e.g., wildfire plumes or synoptic transport), which may not follow average patterns.

We repeated the bidirectional flux simulations using the maximum and minimum diel pattern to fill the data and found that across the full year of data, it impacted the annual deposition by less than 5%. This indicates that the error introduced by an average diel pattern is relatively small when considering the annual deposition. This is in part due to the correction factor we apply to ensure that the 2-week mean concentration matches that recorded in the passive measurements. As stated here, this could still miss episodic events. However, events with the potential to profoundly impact the annual deposition would be captured in the passive measurements.

The use of Radiello passive samplers, which have a documented low bias, raises questions about the accuracy of biweekly NH_3 concentrations. Scaling high-resolution AirSentry data to match passive sampler averages may obscure short-term variability critical for flux simulations. A sensitivity analysis on the scaling method's impact would strengthen confidence.

We scaled the AirSentry data to make sure that the differences were from the effects of measurement time resolution, without mean NH_3 changing the comparison. Additionally, there are likely some concentration differences above forest and grassland ecosystems where these two measurements were taken. Puchalski et al. (2011) found a low bias with MRSE of 9% when comparing Radiello passive samplers to other sampling techniques. This suggests that our calculated annual NH_3 dry deposition is a lower bound. A sensitivity analysis of NH_3 concentration on NH_3 fluxes is now included to give readers a sense of the changes associated with reasonable NH_3 concentration. Although the passive low bias is only 9%, we find that simulations with the mean value increased by 9% result in an annual deposition that is 47% larger than previously estimated. This illustrates how sensitive NH_3 flux simulations are to concentration inputs since the relevant driver is the difference between ambient concentration and compensation point. This additional discussion has been added to the supplementary information. For this paper, we are focused on the specific impacts of time resolution and place it in the context of N deposition in RMNP. We have added more discussion about the impacts of NH_3 concentration and specifically discussion of the passive measurement low bias to the manuscript. In addition to this, an upcoming paper will improve our understanding of true annual deposition in RMNP, by using a gradient method to compare with flux results and update model parameterization for this ecosystem.

The exclusion of snow cover effects on surface exchange is a significant oversight, particularly for winter fluxes where snow alters surface-atmosphere interactions. This omission may explain discrepancies in winter emission estimates.

For areas that have snow cover, the omission of snow cover as a parameter for flux simulations is a large limitation. The effects of snow are poorly understood for NH_3 fluxes and are not included in the bidirectional model used in this work. The generation of an equation that would capture these effects, to include in the model, is outside of our capabilities using this dataset. Future works should investigate these impacts and directly measure fluxes above snow cover. It could be especially important for ecosystem impacts in regions that experience heavy snowmelt. We have added discussion of the potential impacts of snow cover and potential biases introduced to the conclusions section.

The soil compensation point (χ_9) relies on estimated total ammoniacal nitrogen (TAN = 9.6 mg kg^{-1}). No justification or sensitivity analysis for this value is provided, yet it directly influences χ_9 and flux calculations.

The citation was inadvertently left out of the original manuscript. The text has been updated to include a citation to Stratton et al. (2018), who conducted soil analysis in RMNP and reported measurements of soil nitrogen and specifically ammoniacal nitrogen. We have also included a sensitivity test for TAN value in the manuscript and supplementary information.

While a one-month dataset sufficed to derive a diel correction factor in RMNP, this may not hold for regions with stronger seasonal variability (e.g., monsoon-influenced areas). The authors should acknowledge this limitation and recommend longer sampling periods for less-studied ecosystems.

It was not our intent to indicate that this length of sampling would necessarily be effective for all regions, although it tells us something interesting about the seasonality of the diel pattern of NH_3 in RMNP. We have added the sentence "Other locations may have larger and/or more complex variability in NH_3 diel pattern and may require longer periods of data collection to establish an effective NH_3 diel pattern." To address the limitation of only having data from one site. We also added additional description to the conclusion encouraging future studies to collect data for a full year to establish effective diel patterns.

The 31-km resolution of ERA5 likely smooths local topographic effects, critical in mountainous regions like RMNP. While the overestimation of deposition is noted, the paper lacks a quantitative assessment of how terrain complexity biases reanalysis inputs (e.g., friction velocity, Obukhov length).

To complement our analysis of aerodynamic resistance from ERA5 and NEON meteorology, we ran two case studies to directly look at the impact of friction velocity and Obukhov length. This was done by repeating the NH_3 flux simulations using the ERA5 meteorology but replacing friction velocity with the value from NEON. This was repeated for Obukhov length. These case studies are now included in the supplementary information. We see the largest impact from our simulation using the NEON Obukhov Length, however neither simulation entirely corrects for the observed differences.

The bidirectional model's annual NH_3 deposition (0.17 kg N ha^{-1} yr^{-1}) is 74% lower than earlier unidirectional estimates (0.66 kg N ha^{-1} yr^{-1}; Benedict et al., 2013b). However, the paper does not reconcile this stark difference with field measurements or independent validation (e.g., eddy covariance data).

The difference between the previous unidirectional estimate and our bidirectional model estimate is large. For this work, we focused on the impacts of measurement resolution to probe the impacts of time resolution and reanalysis meteorology. We will have another paper published shortly which looks at fluxes simulated using the gradient method in RMNP. Additionally, these gradient fluxes will be compared with bidirectional model simulations.

Critical Load Implications: The 6% NH_3 contribution to total N deposition is framed as minor, but RMNP's critical load (1.5 kg N ha^{-1} yr^{-1}) is still exceeded by current deposition (3.4 kg N ha^{-1} yr^{-1}). The policy relevance of these findings—particularly for targeting emission reductions—deserves deeper discussion.

Critical Loads were developed using only wet deposition of N species. While they are helpful for putting NH_3 dry deposition into a greater context in RMNP, it is challenging to place dry deposition into a policy context given the exclusion of dry deposition in Critical Loads. Notably, the source regions that are important for wet deposition are the same as those important for dry deposition. The policy implications are discussed in the context of where elevated concentrations are transported from and the importance of source regions in the CO Front Range to the east of RMNP. Notably, the highest NH_3 concentrations are observed during upslope transport from source regions in the CO Front Range. These source regions disproportionately contribute to NH_3 dry deposition because the difference between atmospheric concentration and compensation points drives the sign and magnitude of the NH_3 flux.

The study focuses on a subalpine forest, but bidirectional flux behavior may differ in grasslands or agricultural areas. The conclusion's recommendation for multi-site validation is appropriate but underdeveloped.

We have added an additional discussion of suggested multi-site validation to the conclusions to better outline how future researchers could employ these techniques to improve bidirectional modeling of NH_3 fluxes.

The linear correction for biweekly data (slope = 1.07, R^2 = 0.89) works well in RMNP but may fail in regions with frequent emission-dominated periods. A discussion of how site-specific factors (e.g., land use, climate) affect correction efficacy would enhance practical utility.

We have updated the discussion in the section considering the site specific correction factor to:

"As noted above, RMNP has few two-week periods of net NH_3 emission, and the efficacy of this method should be confirmed at a location with more extensive periods of net NH_3 emission. In particular, NH_3 fluxes above managed agricultural land could differ significantly from the pattern observed in RMNP. This study also focused on fluxes above a forest canopy, and results could differ for grassland ecosystems, which also occur in RMNP. To determine the efficacy in other locations, future investigations should select several sites with different land surface types and NH_3 concentrations to make biweekly and high-time resolution measurements for a year"

Key figures (e.g., Figure 7) lack clarity in distinguishing reduced vs. oxidized N species in grayscale. Colorblind-friendly palettes or pattern fills would improve readability.

Figure 7 has been updated to a more colorblind-friendly palette and hatching has been added to make the columns distinguishable even in grayscale. Thank you for catching this! We have checked the rest of the figures again to ensure they are colorblind-friendly.

Sections on resistance parameterizations (e.g., Equations 3–8) are dense and could benefit from schematic summaries or appendices to aid non-specialist readers.

The schematic in Fig 3. now includes all of the resistances and compensation points. We have also updated the text to encourage readers to reference Fig. 3 while they are looking at the more dense equations. We hope this will assist readers comprehension of the equations used, which can be quite dense.

Include sensitivity analyses for key parameters (TAN, snow cover, passive sampler scaling).

We have added a sensitivity analysis for key parameters to the supplementary information. This includes NH_3 concentration, TAN, and LAI values. We are not able to probe the sensitivity of snow cover because of its lack of inclusion in the model used for simulations.

Validate model outputs against independent flux measurements or isotopic tracers.

For this dataset, we lack measurements of isotopic tracers and long periods of flux measurements. In a future work, we will look at fluxes using a gradient method and compare them with those simulated using bidirectional models.

Expand the discussion on policy implications, particularly for RMNP's nitrogen management.

For this work, we are focused on investigating the sensitivity of simulated fluxes to concentration and meteorology inputs. We have revised the text to further elaborate on how the bidirectional framework enhances the impact of source regions in the Colorado Front Range, as the highest NH_3 concentrations are transported from these areas. Additionally, we have added some sensitivity analysis which indicates that our annual NH_3 dry deposition is likely a low bound and may need to be updated for RMNP nitrogen management.

Clarify figures and technical sections to improve accessibility for interdisciplinary audiences.

The figures have been updated to verify accessibility. Notably, the colors in Fig 7. have been changed and hatching has been added to improve readability.

This paper makes a meaningful contribution to atmospheric deposition science but requires addressing methodological uncertainties and broadening the discussion to enhance its impact.

We have addressed the methodological uncertainties raised here and included additional sections where relevant to improve the clarity and impact.

EC1: 'Comment on egusphere-2025-1167', Leiming Zhang, 28 May 2025

I have the following comments for you to consider when revising your manuscript:

A recent study by Jongenelen et al. (https://doi.org/10.5194/acp-25-4943-2025) demonstrated very large uncertainties in the modeled ammonia flux between using three existing bi-directional exchange models, one of which is chosen in your study. Can the major findings presented in your study be generalized if a different bi-directional flux exchange model is used?

Our findings about the impact of using ERA5 on aerodynamic resistance should impact the other bi-directional flux models used in a similar fashion. Although the other modeled resistances may be impacted differently. We previously considered modeling results from the other two bidirectional NH_3 exchange models, however we deemed the inter-model comparison worthy of its own publication. A future paper will consider the differences between these models, compare with NH_3 fluxes derived using concentration gradient measurements, and improve the parameterization of each model above a forest ecosystem.

Although using a bidirectional air-surface exchange scheme is more theoretically correct than using a traditional big-leaf dry deposition scheme, the former does not necessarily perform better than the latter in the simulated ammonia fluxes on seasonal to annual basis and in regional-scale air-quality modeling, as reported by several existing studies. This is because modelling the bi-directional flux requires additional model parameters such as the soil and canopy NH3 emission potentials, which may not be available at high spatial resolution on the reginal scale. Besides, more model parameters can introduce additional uncertainties. Can you provide any insights on this point with your data and some additional analysis?

We probed the sensitivity of our results to changes in several input parameters, including TAN, LAI and NH_3 concentrations. We found that our modelled NH_3 fluxes were very sensitive to TAN. The TAN sensitivity analysis is now included in the supplement. For this study, we had soil measurements to pull from. However, those values are not typically available and may be highly spatially variable, as they were in RMNP. Changing the TAN value by one standard deviation, as determined by Stratton et al. (2018), changed the mean NH_3 flux by ±0.9 ng N m^{-2} s^{-1}, a large deviation, given the size of typical NH_3 fluxes in RMNP. We also found that NH_3 fluxes are highly sensitive to NH_3 concentration value. In the sensitivity analysis now included in the supplement, increasing the NH_3 concentration by 9% increased the annual deposition by 47%. We chose a 9% increase because it is the RMSE determined between passive NH_3 measurements and other measurement types by Puchalski et al. (2011). From these results, we demonstrate that bidirectional fluxes are extremely sensitive to chosen parameters, and NH_3 concentrations, in addition to time resolution and meteorology datasets.**Citation**:

https://doi.org/10.5194/egusphere-2025-1167-EC1