We would like to thank Referee #3 for their thorough review of all materials and the data repository. We appreciate their insights and have responded to each concern. Our response to the Referee is included in RED.

We have also added the following section to the results to better address the editor's comments.

"In this analysis, we simulated the bidirectional exchange of NH_3 above a forest ecosystem using the model proposed in Massad et al. (2010). However, there are other bidirectional exchange models (e.g., Zhang et al., 2010; Pleim et al., 2013) and their simulated fluxes may differ significantly from the model used here (Jongenelen et al., 2025). In the bidirectional exchange model used here, we observe that the selected inputs for NH_3 concentration and meteorological data may introduce biases into the simulated NH_3 fluxes. This may also be true for the other models when simulating NH_3 bidirectional exchange, a good topic for future research."

Jongenelen, T., M. van Zanten, E. Dammers, R. Wichink Kruit, A. Hensen, L. Geers, and J. W. Erisman (2025), Validation and uncertainty quantification of three state-of-the-art ammonia surface exchange schemes using NH3 flux measurements in a dune ecosystem, *Atmos. Chem. Phys.*, *25*(9), 4943-4963, doi:10.5194/acp-25-4943-2025.

EGUSPHERE-2025-1167 | Research article

Sensitivity of Simulated Ammonia Fluxes in Rocky Mountain National Park to Measurement Time Resolution and Meteorological Inputs

Naimie et al. (2025)

Anonymous Referee #3

[General comment]

The authors measured NH3 concentrations with a high time resolution of 30-sec and estimated NH3 flux using a bidirectional exchange model above a subalpine forest, covering a one-year period from September 2021 to August 2022. They proposed that the underestimation of NH3 dry deposition inferred from model using biweekly basis concentrations can be improved by (i) applying a correction factor derived from the relationship with fluxes inferred using 30-min concentration, or (2) estimating the diurnal variation of the biweekly basis concentration.

Observational data on NH3 in forest ecosystems are scarce, and long-term measurements of NH3 concentrations with high temporal resolution are of great value. In addition, given the limited application of bidirectional exchange models in forests compared to grasslands and croplands, this study not only provides valuable information for forests but also proposes a potentially practical approach for improving dry deposition estimation with higher accuracy by utilizing the data of monitoring network such as AMoN.

The concept and overall approach in this study are excellent, and I commend the authors for their efforts for treating a large amount of observation dataset and handling long-term modeling. However, it seems that there is still room for further discussion regarding the specific aspects of the methodology and analysis of both observations and models that support the results and conclusions of this study.

[Major concerns]

QA & QC for measured NH3 concentration

Upon reviewing the dataset referenced by the authors ("Rocky Mountain National Park Ammonia Data"), I have several concerns regarding how the observational data, which is the foundation of this study, were processed and quality controlled.

1 For the biweekly NH3 measurements obtained using passive samplers, the authors mention there were 27 sampling periods, with samples collected in duplicate. From 23 August 2021 to 9 November 2021, four samples were collected per period. However, the number was reduced to two after this period. I think the authors used mean values for each period for analysis, but it is samples when averaging. Were the sample sizes standardized or weighted in any way?

We apologize for the confusion in regard to this sampling. From Aug. 23 to Nov. 9, two samples were deployed at the tower top, and two were deployed lower on the tower. After this period, passive samples were only deployed at the tower top. Only the tower top data was used for this analysis. You are correct that we used a mean value for each period. This sentence has been added to the passive measurement methods section: "Due to site access issues, some samples had durations longer than 2 weeks. To create a consistent dataset, all data were aggregated to a 2-week average. In the case where two samples overlapped a 2-week period, they were combined using a weighted average. One sample was below the detection limit and was removed from this analysis."

2 Additionally, there is only one valid passive sample for the period from 30 March 2022 to 14 April 2022. Moreover, the sampling period from 26 May 2022 to 21 June 2022 is approximately one month. There are also cases of overlapping sampling periods (e.g., 23 August to 6 September, and 29 August to 13 September 2021). However, the manuscript does not mention how these irregularities were addressed.

Thank you for taking such a detailed look at the raw data used. We have added text to explain how the overlapping periods were handled (see above). We have also added the following text to make other sampling discrepancies clearer to the reader: "During the sampling period, some sampling periods were longer than others due to issues accessing the site. To compare periods of a consistent length, all data was taken to a 2-week average using a weighted mean of the data available during that period. One sample was below the detection limit and removed from this analysis." This text was added to lines 154-156.

3 Regarding the high temporal resolution NH_3 measurements of AirSentry, the detection limit is 0.070 ppbv. According to the raw data (RMNP_AirSentry_NH3_2021_2022.csv) from 9 July 2021 to 13 July 2022, approximately 18.6% of the total values fall below the detection limit. Surprisingly, there is even data with a concentration of "0" (approximately 10.8%). It is highly likely that a substantial portion of the data are also below the quantification limit. I am seriously wondering how the authors handled such data, as the authors state that "Only NH3 data missing due to power outages have been removed from the AirSentry dataset".

Thank you for the detailed look at the data repository. There are a few updates we can make to help the reader understand the data processing. First, the data with a concentration of "0" were removed in our analysis, but were not replaced for the data upload. This error has been fixed, and the data repository does not contain any values of zero. Second, what is currently included in the manuscript for a detection limit (70 ppt) is for the 30-second measurements. The data used is a 30-minute average of these measurements. Averaging 60 data points increases the signal-to-noise ratio and lowers our detection limit. The following text has been added to the methods section in regards to the AirSentry data (lines 168-172): "The limit of detection for 30-second measurements 70 pptv. For this data analysis, NH_3 concentration data was averaged to 30-minute mean values. Averaging data points increases the signal-to-noise ratio. We approximate that the signal-to-noise ratio increases proportionally to the square root of the number of samples (n = 60) (Dempster, 2001). In this case, the signal-to-noise ratio increases by a factor of 7.7, reducing the limit of detection to 9 pptv for 30-minute mean NH_3 concentrations. Across the year of data collection, 101 points fell below the detection limit." A description of this detection limit should be helpful to the reader. Later in the text (lines 190-192), we have added the following line to explain why those 101 points were allowed to remain in the dataset: "The 101 30-minute average NH_3 concentration values below the AirSentry detection limit, representing 0.5% of the total measurement period, were assumed to represent a random distribution below the detection limit and retained for post-process scaling from the passive observations." Additionally, the mean of values below the detection limit is 0.005 ppbv NH_3, which is approximately ½ of the MDL.

4 While missing data are understandable in long-term field observations, it is essential for the authors to clearly describe their QA/QC procedures: how sub-detection-limit values were treated, how much of the dataset exceeded the detection and quantification limits, and what criteria were used to ensure the reliability of the measurements. Without such information, the credibility of the dataset and the study's conclusions may be undermined.

We appreciate the feedback and encouragement to include more description of the QA/QC procedures performed on the dataset. We have updated the methods section, as described above, to include more discussion of QA/QC. We hope this will help the reader see the credibility of the datasets and our conclusions more clearly.

Uncertainty in the measured NH3 concentration

In my opinion, the overall uncertainty of this study depends heavily on the accuracy of the measured NH_3 concentrations. The authors also state that "increasing the NH3 concentration by 9% increased the annual deposition by 47%", highlighting the strong sensitivity of model outputs to measured concentrations. While other referees have suggested sensitivity analyses for various parameters of the bidirectional exchange model, the most critical first step is to clarify the uncertainties in the observational data. Addressing these uncertainties would not only enhance the credibility of the measurements but also improve the reliability and interpretability of the model-based analyses. Assuming proper QA/QC procedures were conducted, I offer the following specific suggestions:

This is an important point, and part of our hopeful takeaway for readers is that NH_3 concentration measurements have an important and highly variable impact on simulated NH_3 fluxes. In the sections above, we have addressed the QA/QC concerns discussed. This will hopefully aid the reader in understanding the reliability of our data.

- It is generally known that NH3 exhibits strong seasonal and diurnal variability in both flux and concentration, as is evident in Figure 2. Before scaling high-resolution AirSentry data to match passive sampler, a direct comparison of the two measurement techniques should be quantitatively presented. How well do the two measurements agree? If there are systematic differences, is there any evidence of seasonal bias? In my experience, passive samplers requiring multiple lab processing can be prone to contamination, particularly during warmer seasons.

A direct comparison of the mean biweekly passive NH_3 concentrations and AirSentry data has been added to Fig. S1 in the supplementary information. We have also added the following text: "AirSentry NH_3 concentration measurements at the nearby EPA shelter are compared with mean biweekly passive NH_3 data. The biweekly passive NH_3 values are a weighted average of all tower-top passive measurements made during the 2-week period. Note, the AirSentry NH_3 measurements were taken at the nearby EPA shelter, at a height of 2 m above a grassland ecosystem, and the passive NH_3 measurements were made at a height of 25 m above a forest ecosystem on the NEON tower." A linear fit to the datasets departs from the 1 to 1 line (slope = 1.07, intercept = 0.1 ppbv, R^2 = 0.71), but as noted, the site differences make it challenging to separate systematic biases from measurement techniques and potential concentration differences between the two sites. We do not observe a seasonal variation in the difference between measurement techniques.

- Although the authors state that "Passive NH3 sampling methods have been shown to have a low bias", the raw data reveal cases where concentrations differ by more than a factor of 2 between passive samplers during same sampling period (e.g., from 10 May to 26 May 2022; 5 July to 18 July 2022; and 18 July to 2 August 2022). Given such large discrepancies in measured concentration using passive samplers, is it truly necessary to scale high resolution data from AirSentry, which is capable of detecting NH_3 at levels as low as 0.070 ppbv, by passive sampler? It also seems that Referee 2's concerns regarding this issue have not yet been fully addressed.

Our reason for scaling the AirSentry data is to avoid differences between atmospheric NH_3 concentration above a grassland, where the AirSentry was placed, and NH_3 concentration above a forest ecosystem, where the other measurements were made. We also scale the measurements to ensure that the 2-week mean value is consistent for NH_3 flux simulations with high time resolution NH_3 concentrations and the biweekly NH_3 concentration data. Due to the different locations of these measurements, we did not directly assess any biases in the passive NH_3 data. We use previous works to estimate the potential low bias, and use it as a bound for how NH_3 fluxes could change if such a bias was observed. Given our data availability, we had no way to assess how the passive measurements would have compared with AirSentry NH_3 measurements made at the same location.

Seasonal variation in NH3 concentration and inferred flux

- Assuming the QA/QC procedures and the uncertainties in the observational data discussed above are adequately addressed, I offer the following suggestion concerning the seasonal variation of NH3 concentration and fluxes. As the author described, "In practice, fluxes can change quickly and even reverse direction with changing environmental conditions." However, the analysis and discussion about the seasonal variation in NH3 concentration and inferred flux appear to be insufficient.

We appreciate the discussion provided here by Referee #3. We have addressed the QA/QC concerns above, and here will address the direct comments on the seasonality of NH_3 and the simulated fluxes.

- Although direct measurements data were not presented at the forest of this study, recent long-term observations have reported that NH3 flux in forest forests generally show emission during warm seasons and deposition during other seasons (Melman et al., 2025 https://doi.org/10.1016/j.atmosenv.2024.120976). However, the estimated fluxes are quite small level than previous studies ranging from -5 to 5 ng N m-2 s-1, and no large seasonal variation can be seen (as shown in Figure 3). Furthermore, emission and deposition do not alternate in short-term, but in some cases show consistent deposition, particularly from late July through September in 2022. I can understand the state of "we focused on the impacts of measurement resolution to probe the impacts of time resolution and reanalysis meteorology," however, the lack of detailed discussion on the seasonal variation leaves a gap in the interpretation. As a fundamental step to enhance the methodological reliability of this study, the authors should analyze the seasonal patterns in NH_3 flux and concentration more thoroughly, and explore their potential causes, before addressing the annual deposition estimates and discussing the differences arising from the use of various concentration datasets and meteorological inputs.

Yes, we were also surprised to not have a more clear large seasonal variation in NH_3 fluxes. Previous work in RMNP (Pan et al., 2021) also observed deposition fluxes during the summer This conceptually makes sense, because NH_3 concentrations are typically higher in the warm seasons. Some previous works may have found deposition during cool seasons in part due to their handling of snow cover. Later in this document and in the general text, we have added a small analysis to understand how shutting off soil emissions in the winter time would have affected our simulated fluxes. We have added some additional discussion of the seasonality of meteorology, to help the reader understand the seasonal cycle in RMNP. We appreciate the referee's feedback that the text is lacking a seasonal interpretation. For an updated annual Nr deposition budget in RMNP and to understand bidirectional flux simulations of NH_3 this would be a great focus for future research.

- The compensation point, which determines both the direction and magnitude of the estimated NH3 flux strongly depends on meteorological factors such as temperature as well as ambient NH3 concentration. Despite a previous suggestion by Referee #1, the meteorological conditions at the study site remain largely undocumented. The revised manuscript includes only mean values for temperature and relative humidity, but it is not even clear over what period these values were calculated. In addition, only qualitative descriptions of rainfall and snowfall are provided. With such limited information, it is difficult to interpret the temporal dynamics of the NH_3 flux or to assess uncertainties in the annual deposition estimates. At a minimum, the authors should clearly present seasonal variations in temperature, relative humidity, and rainfall, as well as the snowfall periods during the observation period to support their analysis.

We appreciate the Reviewer's feedback on increasing the meteorological information included in the manuscript. We have added seasonal mean values for both RH and temperature to help the reader understand the meteorological conditions at the NEON tower in RMNP. The following text has been added: "Mean values were calculated from September

2021 to September 2022. Snowfall typically occurs between October and May. The seasonal mean temperatures (relative humidities) are as follows: winter (December, January, and February) mean is -3 °C (30%), spring (March, April, and May) mean is 2 °C (44%), the summer (June, July, and August) mean is 15 °C (49%), and the fall (September, October, and November) mean is 8 °C (37%). Precipitation is measured at 1-minute resolution by a Belfort AEPG II 600M weighing gauge. Precipitation events were defined as periods of rainfall separated by at least one hour without precipitation. During our study period, there were 27 precipitation events in the winter, 62 in the spring, 63 in the summer, and 26 in the fall."

- While the authors have conducted sensitivity analyses on parameters such as LAI and TAN in response to referee comments, I think these are relatively minor issues at this stage. The primary parameters that determine the compensation point of ($\chi z0$ in the model used at this study) are temperature and emission potential of stomata and soil. In addition to temperature, the emission potential, especially for stomata, also varies seasonally (Flechard et al., 2013 https://doi.org/10.5194/bg-10-5183-2013). It is reasonable to assume a constant emission potential within a given season; however, for a one-year study such as this, it would be more appropriate to account for seasonal variation. For example, assigning higher values in summer and lower values in winter would better reflect the expected physiological and environmental dynamics. Moreover, as Referee #1 pointed out, the stomatal emission potential used in this study appear significantly lower than those reported in Zhang et al. (2010 https://doi.org/10.1029/2009JD013589) or Massad et al. (2010). Sensitivity analysis of the emission potential is a critical step when applying the bidirectional exchange model and should be prioritized. Even if the influence of emission potential on the $\chi z0$ is limited under low temperature due to the high-altitude site, this feature itself could be an important characteristic worth highlighting. Accordingly, seasonal variation in temperature should be explicitly shown and discussed in this context.

We tested the sensitivity to TAN, LAI, and NH3 concentration at the suggestion of other referee comments, as noted here. For TAN and LAI, we had good data to make estimates of the reasonable bounds and found the NH3 fluxes to be sensitive to both parameters. Although fluxes are sensitive to temperature, we have temperature data from the NEON tower with very low uncertainty. The equations used do not include an emission potential of soil. Instead, in Eq. (12), the ground compensation point is simulated using TAN. In effect, conducting a sensitivity analysis on TAN determined the sensitivity of the ground compensation point and the relative role of the ground (including soil) on NH_3 fluxes. For stomatal emission potential, we used direct measurements of foliage around the NEON tower. The collection process and calculation are described in section S7 of the supplementary materials. This value is lower than those reported by Zhang et al. (2010) and Massad et al. (2010), however, it is directly reflective of our measurements at the site. The stomatal emission potentials summarized in Zhang et al. (2010), and Massad et al. (2010) are highly variable, and there is very limited direct data for seasonal changes above this land surface type. Foliage samples were collected across seasons; however we did not observe a seasonal pattern due to the high sample variability. We probed this sensitivity by running simulations with different values for stomatal emission potential. Doubling the stomatal emission potential decreased the annual deposition flux by 14%. Halving the stomatal emission potential increased the annual flux by 7%.

- While there are still many uncertainties regarding soil processes, and I understand the difficulty in handling soil emissions in bidirectional exchange models, existing model such as Zhang et al. (2010) provide useful guidance. For example, the soil emission potential is set to zero during periods with snow cover in this model. I recommend referring to such practices in your analysis and considering whether a similar treatment may be appropriate for your study.

For this work, we chose to focus on one bidirectional exchange model (Massad et al. 2010) and deeply investigate the impacts of changing the time resolution of meteorological and NH_3 concentrations.

We conducted another sensitivity analysis to probe the impact of shutting off soil emissions. For this test, we set χ_g equal to zero during the winter (December, January, and February). This changed the wintertime net fluxes from emission to deposition. This sentence was added to the main text "To probe the impact of snow cover, a sensitivity test was conducted setting χ_g equal to zero during the winter (December, January, and February), which increased annual deposition by 0.06 kg N ha^{-1} yr^{-1}. However, this analysis does not take into account how the surface differences may change NH_3 fluxes above snow. " This text was added to the supplementary information "Lastly, we tested the sensitivity of flux simulations to the ground compensation point during the winter (December, January, and February), to probe the potential impact of snow cover. In the winter, we set χ_g to zero to stop ground emissions. Setting χ_g to zero during winter changed the net wintertime flux from emission to deposition and increased the annual NH_3 deposition by 0.06 kg N ha^{-1}."

[Minor and technical comments]

1. Next time, please clearly indicate which parts of the manuscript have been revised in response to the referee's comments. For example, you could write: "We revised the sentence (Line: xx-yy in the Author's tracked changes version)."Additionally, **there are many typographical errors throughout the manuscript. Please perform a thorough proofreading before submitting your revised version.**

In the future, and for responses to this review, we will include line-by-line notation for all changes made. We apologize for missing the noted typographical errors noted here and appreciate the reviewer for catching them.

2. Figure 1: The phrase "at 7.5-arc-second spatial resolution" is difficult to immediately understand. Pleased convert it to "225 m" for clarity.

Although 225 m is approximately correct, the USGS outputs this data in arc-seconds because it is generated using latitude and longitude, which are not a consistent distance across the globe. We have changed the text to "at 7.5-arc-second spatial resolution, or approximately 225 meters".

3. Site location: According to the aerial photo at the provided coordinates (40.275903, -105.54596), there appear to be several buildings within approximately 100 meters of the NEON tower. This suggests that the condition differs from that of a typical forest observation site. Furthermore, the spatial extent shown in Figure 1 is too large to understand the site-specific conditions. Please provide additional information, such as a photograph of the observation tower, a site map illustrating the surrounding environment, or a schematic diagram of the observation.

There are a few small measurement shelters used by NEON within 100 meters of the site, and one larger building ~150 m away. Supplementary Fig. S5 shows the site configuration in more detail, with the LAI values plotted. In Fig. S5 you can see the larger building mentioned, and its distance from the measurement site. The larger map is shown in Fig. 1 to give the reader a sense of the general surroundings and source regions near the site.

4. Line115 in Author's tracked changes version (same as below): Please correct the unit from "C" to "°C" and insert spaces before and after the "=" sign. Also, clarify the period over which the means of air temperature and relative humidity were calculated. At a minimum, the total precipitation and snowfall events during the observation period should be explicitly stated.

In line 115, "6 C" has been updated to "6 °C" and spaces have been added before and after the equals ("=") sign. The following text was added to line 116: "Mean values were calculated from September 2021 to September 2022." We have added the following text: "Precipitation is measured at 1-minute resolution by a Belfort AEPG II 600M weighing gauge. Precipitation events were defined as periods of rainfall separated by at least one hour without precipitation. During our study period, there were 27 precipitation events in the winter, 62 in the spring, 63 in the summer, and 26 in the fall" to describe the precipitation events observed at the RMNP NEON tower.

5. Line 117: Please specify the height at which these meteorological parameters (e.g., temperature, humidity) were measured. Also, indicate the temporal resolution of the measurements (e.g., 10-minute averages, hourly, etc.).

The following sentences have been added to line 117: "The meteorological observations used from the NEON tower are 30-minute mean values. Direct measurements of wind vectors, air temperature, short wave radiation, relative humidity, air density, and air pressure were used from the tower-top measurements (25 m-agl). 3D wind vectors were measured at 20 Hz using the CSAT-3 sonic anemometer (Campbell Scientific Inc., Logan, Utah, USA)."

6. Line 119: Please clarify which specific product was used to determine the LAI value. A value of LAI = 0.8 is quite low for a forest site, which may be due to the coarse spatial resolution of 1km grid that includes bare land, and bulling in addition to vegetation. As the study develops, the authors should consider measuring LAI directly around the tower using a canopy analyzer or similar instrument. LAI is a critical parameter for modeling NH_3 emissions and deposition. For example, in the model by Zhang et al. (2010), the stomatal emission potential is set to zero when LAI < 0.5. Additionally, LAI is used to parameterize both in-canopy aerodynamic resistance and cuticular resistance. This is also a key issue for the authors' planned model inter-comparison paper and should be addressed carefully.

Thank you for catching this. The LAI product from NEON, detailed in the supplementary information, is a spectroscopic measurement around the NEON site. The following text: "Leaf area index (LAI) is estimated at the site using remotely sensed data at 1 km resolution" has been updated to "Leaf area index (LAI) is estimated at the site using remotely sensed data". The data is not at 1 km resolution, as you can see in Fig. S5. The tiles are output for square km areas. The spatial area shown to generate 0.8 for an LAI is shown in Fig. S5. The area was specifically selected to not include the nearby building or cleared land area. This data was also used to generate the minimum and maximum LAI values for sensitivity testing.

We appreciate the reviewer's input for upcoming papers and will seek to carefully address the impacts of LAI on intermodal comparison and parameterizations.

7. Line 121: Please indicate specifically which section or figure of the supplementary information readers should refer to. This comment applies to other instances of vague referencing throughout the manuscript as well.

To more clearly reference the supplementary information, the following text: "The square kilometer of leaf area index values surrounding the tower site is shown in the supplementary information" has been updated to "The square kilometer of leaf area index values surrounding the tower site is shown in Fig. S5". The following sentence: "The sensitivity to LAI can also be found in the supplementary information", has also been updated to: "The sensitivity to LAI can also be found in section 5 of the supplementary information".

8. Line 136: If I understand correctly, the Monin-Obukhov length (L) also must be calculated in the NEON site, and the methods for calculating L differ between Sections 2.2.1 and 2.2.2. At the NEON site, it seems that L was estimated using the sonic virtual temperature, the covariance of vertical wind and sonic virtual temperature, and the friction velocity derived from the 3D wind components measured by a sonic anemometer (Please specify the manufacturer name and model number of the instrument. Young ? or Gill?).
   In contrast, the method for calculating L from ERA5 data is unclear in the manuscript. How did you derive the surface buoyancy flux ($\overline{w'\theta v'}$)s from ERA5? This typically requires both sensible and latent heat fluxes, along with air density. Note that ERA5 provides both "surface sensible heat flux" and "instantaneous surface sensible heat flux"; please specify which was used. To enhance transparency and allow others to reproduce your method, I strongly recommend including the equations used (e.g., Lsonic = …, and LERA5 = …), and a table listing the specific variables used in each case. This would also help readers understand the discrepancies between Lsonic and LERA5 that are discussed in the supplemental information. Furthermore, since ERA5 has a 1-h temporal resolution, how was this data used with 30-min concentration data for flux calculation?

Yes, you are correct, NEON uses the Monin-Obukhov similarity theory to calculate L using friction velocity (from 3D sonic anemometer), air temperature, and Eddy covariance of wind and temperature. The 3D wind component instrument is now included in the NEON description with this text: "3D wind vectors were measured at 20 Hz using the CSAT-3 sonic anemometer (Campbell Scientific Inc., Logan, Utah, USA)."

For the ERA5 calculation, "instantaneous" fluxes were used. This sentence "Obukhov Length is the characteristic length scale of the atmosphere and is calculated from ERA5 data using surface sensible heat and moisture fluxes." Has been changed to this: "Obukhov Length is the characteristic length scale of the atmosphere and is calculated from ERA5 data using instantaneous surface sensible heat and moisture fluxes". We directly followed the suggested data download and equations from ECMWF. To make it easier for readers to repeat this process, we have included a citation to this resource from ECMWF and added this text "fluxes based on the suggested calculation from the European Centre for Medium-Range Weather Forecasts (Gusti, 2024)" to line 142.

9. The phrase "Eq. (5.7c) from Stull (1988)" is difficult to follow. I suggest rephrasing it as: "… was calculated using Eq. (1) following Stull (1988)".

Line 136 has been updated from "Eq. (5.7c) from Stull (1988)" to "Eq. (1) following Stull (1988)", as suggested.

10. Line 146: Please provide the manufacturer name and model number instead of listing only a URL. The same request applies to other instruments mentioned in the manuscript, including the 3D sonic anemometer, ion chromatography system, and AirSentry.

Lines 146 to 148 were updated to the following: "Biweekly NH_3 ambient air concentration was measured using Radiello passive diffusion samplers purchased from Sigma Aldrich. The Radiello sampling system includes a diffusive body (part number: RAD1201) and adsorbing cartridge (part number: RAD168)", to include part numbers and manufacturer name for the passive sampling. For the ion chromatography system in line 150, the text "using ion chromatography (IC)" has been updated to: "analyzed on a cation IC using a 20 mM methanesulfonic acid eluent (0.5 mL min−1) on a Dionex CS12A ion exchange column with a CSRS ULTRA II suppressor and Dionex conductivity detector(Li et al., 2016)." The 3D sonic anemometer used was a CSAT-3 3-D Sonic Anemometer from Campbell Scientific. This description has been added to the NEON data section.

The AirSentry model (AirSentry II) and manufacturer name (Particle Measuring Systems) are both listed in lines 156-157 with the text: "The instrument used was the AirSentry II Point-of-Use IMS from Particle Measuring Systems located in Boulder, CO." The text has been updated to the following: "The instrument used was the AirSentry II Point-of-Use IMS (Particle Measuring Systems, Niwot, CO)", to avoid confusion.

11. Line 151-152: There are several types of denuders (e.g., annular, multi-channel, honeycomb), but I am not familiar with the term "University Research Glassware Denuders". Is this a proper noun or brand name? Please clarify or revise how this is described in the manuscript. And I do not agree with that the passive sampler have a low bias based on your low data.

University Research Glassware is the company that made the annular denuders used in the cited paper. We have updated the text in line 151 from "University Research Glassware Denuders" to read "annular denuders", so it will be clearer to the reader. In this section, we are citing other works that have considered the biases present in passive sampling techniques. Due to the lack of data overlaps, we were not able to directly compare passive measurements to another collocated data source. We are using other works that indicate a low bias to assess what the impact of those low biases would be on simulated NH_3 fluxes.

12. Line 157: "Boulder, CO" may refer only to a location and does not specify the manufacturer or model. The correct company name is "Particle Measuring Systems." Please revise accordingly. What does "1/4""means? And please correct the unit from "C" to "°C".

The manufacturer and company name is already given ("Particle Measuring Systems"). The model name is also given ("AirSentry II"). Boulder, CO, is the location of purchase. The text has been updated to the following: "The instrument used was the AirSentry II Point-of-Use IMS (Particle Measuring Systems, Niwot, CO)", to avoid confusion. Thank you for catching the ¼", this means 0.25 inches. The text has been updated from imperial (¼") to metric (0.635 cm). A degree symbol has been inserted.

13. Line 159: Did you perform any tests to assess potential NH3 losses within the sampling tube? Given that NH3 is highly soluble in water, it may be adsorbed onto the tube walls under high humidity conditions.

The efforts taken to avoid NH_3 loss to the inlet are described in lines 159-161: "The sampling inlet was 0.635 cm Teflon tubing, heated to 40 °C to reduce NH_3 loss to the sampling tube. Inlet length was kept as short as possible to further prevent NH_3 loss." We did not directly test NH3 losses to humidity, but we would not expect condensation onto the inlet tubing walls given the coating and heat maintained. The average seasonal temperature and RH is now included in the text: "The seasonal mean temperatures (relative humidities) are as follows: winter (December, January, and February) mean is -3 °C (30%), spring (March, April, and May) mean is 2 °C (44%), the summer (June, July, and August) mean is 15 °C (49%), and the fall (September, October, and November) mean is 8 °C (37%)." Given the low temperatures, heating the inlet to 40 °C is much hotter than the ambient and the typical RH values are below 50% in all seasons.

14. Line: 168: Please specify "the effect of NH3 …" on what? The sentence is currently ambiguous.

The line "To investigate the effect of NH_3 (g) sampling time resolution" has been changed to "To investigate the effect of NH_3 (g) sampling time resolution on simulated fluxes" to explain that we are trying to understand how NH_3 sampling time resolution will affect simulated fluxes.

15. Line: 169: The expression "30-min frequency" is potentially misleading. Since AirSentry's time resolution is already expressed as measured by "30-sec frequency", the 30-minute value generated here should be rephrase.

The text was updated from "30-minute frequency" to "30-minute resolution."

16. Figure 2: The labels "2021" and "2022" on the x-axis only need to appear once at the beginning of each year. Instead, it would be more helpful for readers to better illustrate seasonal trends if the figure included month labels across the full period.

Every other month is marked in the current version of Fig. 2. We attempted to include a label for every month; however, this made it challenging to read the labels on the x-axis.

17. Line 180: If my understanding is correct, the data count would exceed 3,000 if values below the detection limit or those reported as zero are also included.

We have added more explanation in the AirSentry methods, as discussed above, to address the missing values.

18. Line 190: Please correct the formatting of "Fig (S1)" to "Fig. S1".

The text in line 190 has been updated from "Fig (S1)" to "Fig. S1".

19. Line 202: Please specify what type of wet deposition data were used.

The text in line 202 was updated from "Wet deposition" to "Weekly precipitation wet deposition", to better reflect the collection methods used by the NADP NTN.

20. Line 203: Please use the same format for latitude and longitude notation as used for other sites to ensure consistency throughout the manuscript.

Thank you for catching this. All latitudes and longitudes have been updated to this format "40.3639, -105.5810". The text in line 203 was changed from "40.3639˚N, -105.5810˚E" to "40.3639, -105.5810".

21. Line 210: The phrase "dry deposition is generated" sounds unnatural. Dry deposition is typically inferred or estimated, not "generated." Please revise this expression accordingly. The same applies to the use of "generation" in reference to Vd.

The word "generated" has been changed to "estimated" when referring to V_d values.

22. Line 214: The abbreviation "(Vd)" should be introduced at Line 211 when "deposition velocity" is first mentioned.

The abbreviation for deposition velocity, "(V_d)", was added to line 211 and removed from line 214.

23. Line 215: Why "dry deposition velocity" is being used at here instead of Vd.

In lines 214 through 216, the terms "deposition velocity" and "dry deposition velocity" has been replaced with V_d.

24. Line 217: Although I understand this is based on previously studies, please note that the Vd of NH3 is not so easily defined. This may partially explain the discrepancy in annual deposition amount discussed at Line 365-368.

Yes, we are just basing this on a few previous studies. We agree with the referee that this is a simplified and not perfect definition for $V_{d(NH3)}$.

25. Line 219: The bidirectional flux model by Massad et al. (2010) is more complex than other models used in previous studies, and the calculation of the χz0 is difficult. This is why the authors are required to conduct various sensitivity analyses. It would be better to describe the advantages behind selecting this model.

Thank you for this feedback, we have added the following line: "This model was selected because it estimates both emissions and deposition of NH3, uses a compensation point framework to capture these complex dynamics, and takes into account rapidly changing micrometeorology." This should better explain to the reader why we have selected this model and increase their understanding.

26. Line 222: If I understand correctly, the authors appear to have misunderstood the framework of the model of Massad et al. (2010). In this model, the direction of total flux is determined by the difference between atmospheric concentration (χa) and χz0. The canopy compensation point (χc) is an intermediate parameter for determining χz0, not the determinant of direction of total flux. This distinction is important because the model differs conceptually from simpler models. Please revise the explanation to ensure accuracy.

Thank you for noting this. As shown in Eq. (17), the overall flux is determined by χ_a and χ_{z0}, as you correctly indicate here. The text has been updated to give the proper description.

This line (226) has been deleted: "Canopy compensation point depends on the stomata resistance, cuticle resistance, and stomata compensation point."

The following text: "The model determines if the flux will be negative (deposition) or positive (emission) based on the relationship between the atmospheric concentration (χ_a) at a given reference height (z) and the canopy compensation point (χ_c).” has been replaced with: “The model determines if the flux will be negative (deposition) or positive (emission) based on the relationship between the atmospheric concentration (χ_a) at a given reference height (z) and the compensation point (χ_{z0}) at a defined distance (d) above the roughness length (z_0).”

27. Figure 3: The term “surface compensation point” is incorrect; as far as I know, this terminology is not used in previous literature. χz0 is the compensation point at height of (d + z0). Please correct “Stomata” to “Stomatal”, and “cuticle” to “cuticular”. Also, “laminar” is no need for Rbg.

Yes, Massad et al. (2010) describe χ_{z0} as the compensation point at height (d+z0). We called it the surface compensation point here to encompass all compensation points to deal with all surface relationships. In Massad et al. (2010), it is not given an intuitive term. We appreciate the reviewer’s feedback that calling it the “surface compensation point” adds confusion. We have removed the instances of “surface compensation point” and replaced them with “χ_{z0}” throughout the manuscript.

“Stomata” and “cuticle” have been updated to “stomatal” and “cuticular”, respectively.

The word “laminar” was removed from the description of R_{bg} in Fig. 3.

28. Line 235: Is Figure 3 showing "relationship" rather than a conceptual diagram? Please clarify and revise the description accordingly.

Good point, referring to Fig. 3 as a conceptual diagram is a better description. We have updated this sentence in line 235: “The relationship between resistances and compensation points is shown in Fig. 3” to: “A conceptual diagram of resistances and compensation points is shown in Fig. 3”.

29. Line 239: Obukhov length, displacement and roughness length have already been defined as L, d, and z0.

The sentence including displacement height and roughness length has been updated to read: “R_a was calculated according to Thom (1975), where z is 25.35 m, d is 7.15 m, and the roughness length is 1.65 m”. The sentence now does not redefine the shorthand, but still includes the numeric values. On line 244, “Obukhov length” was replaced with “L”.

30. Line 245: Please delete “captures the aerodynamic resistance from within the canopy layer and”. While this may apply to Rac, it is incorrect in the context of Rg.

The phrase “captures the aerodynamic resistance from within the canopy layer” was deleted from line 245.

31. Line 247: The phrase “using Eq. 16 and Eq. 17 from equations of Massad et al. (2010).” may confuse readers, especially since the present manuscript also contains Eq.(16) and Eq.(17).

The equation numbers are included to help a reader reference where in Massad et al. (2010) they should look to calculate the α parameter. We understand the referee’s concern here. However, we have decided to keep the equation numbers for easier referencing to the original material.

32. Line 252: Could you elaborate on how the parameter Rbg was determined? This parameter are not widely compiled, and your approach would provide valuable information for future studies.

The Rbg parameter equation has been updated to more clearly follow the description in Nemitz et al. (2001). The following text has been added to our description to explain how the parameter was calculated (lines 271-273): "Ground boundary layer resistance (R_{bg}) is based on Nemitz et al. (2001), where u_g is the wind speed at the ground, which we approximate as 5% of the wind speed at tower top (25 m), and z_l is the upper bound height of the logarithmic wind profile above the ground, which we approximate as 10% of the canopy height (Nemitz et al., 2001)"

33. Line 257: According to Table 1 in Zhang et al. (2003 https://doi.org/10.5194/acp-3-2067-2003), I could not find a rstmin value of "225" for any land use category. Is the mistake of 250?

This is not a mistake, we used a combination of the values for the two fauna types in RMNP, evergreen needleleaf trees and deciduous broadleaf trees + shrubs. Using land surface types from the NEON database, we assumed 75% evergreen needleaf trees (Rst min = 250) and 25% deciduous broadleaf trees + shrubs (Rst min = 150). This sentence: "The minimum value for R_{st} (225 s m^{-1}) was determined using Table 1 of Zhang et al. (2003)" was updated to: "The minimum value for R_{st} (225 s m^{-1}) was determined using Table 1 of Zhang et al. (2003), assuming 75% of the land surface was evergreen needleaf trees and 25% was deciduous broadleaf trees and shrubs."

34. Also, did you assign Rst at nighttime to be infinite value considering the stomatal closure following Zhang et al. (2003)? This is a critical assumption in modeling bidirectional exchange of NH3.

No, we used the equation as listed in Table 2 of Nemitz et al. (2001).

35. Line 258: Based on your response to Referee #1, it seems there may be a misunderstanding regarding Rw. As I understand it, this is an empirical formulation that accounts for four different vegetation types, not only for "Douglas Fir." Moreover, the effect of LAI and Temperature is considered at this empirical formula. It is important to avoid applying such models blindly without a full understanding of their basis.

We see how, in this case, including the phrase "predominantly Douglas Fir" has lead to confusion about how Rw was parameterized. Yes, the empirical formula includes LAI and temperature affects, but does not use the equation suggested by Referee #1. The text has been updated to "Cuticular resistance (R_w) was calculated according to the proposed corrected parameterization as described in Massad et al. (2010), for a forest ecosystem." This is following equation 24 in Massad et al. (2010), using the parameter proposed for a forest ecosystem.

36. As an additional point, changing the cuticular resistance formulation can also significantly affect NH3 fluxes in addition to the emission potential. I recommend considering the case study of Xu et al. (2023 https://doi.org/10.1016/j.atmosenv.2023.120144) using a bidirectional exchange model in your uncertainty discussion or future sensitivity analysis.

For this work, we used the cuticular resistance described in equations 24 and 23 of Massad et al. (2010) directly. Xu et al. (2023) include this formula in their Table 2, as an option for calculating Rw. However, they found that the Rw formula from Sutton et al. (1998) was a better fit for their observations. This is interesting and may be important as we think about using our data for model parameterization efforts. However, for this paper, we use the Massad et al. (2010) equation as described. It will be interesting to see if this formula for Rw aligns with our flux measurements in the future.

37. Line 262: This is self-evident from Eq.(8), and Eq.(9) is unnecessary. Is there any case in which relative humidity exceeded 100% at the NEON site?

Eq (9) is necessary here, because if RH exceeds 100% the calculated Rw is different between Eq (8) and Eq (9). It is theoretically possible to achieve RH values that exceed 100%, so we have included Eq (9) to be consistent with Massad et al. (2010).

38. Line 263: Although the exclusion of HCl likely has minimal influence on the results, was there a specific reason it was not considered in the calculation? I suspect that the higher acidity ratio (AR) observed in winter may be due to extremely low NH3 concentrations. Is it possible that a higher AR facilitated NH_3 deposition in the site? According to the study by Xu et al. (2023), Rw can be highly sensitive to this factor.

Rw is sensitive to AR, it and RH drive the Rw Eqs. (8/9). Figure 7 of Massad et al. (2010) shows the dependence of Rw on AR when RH is held constant. HCl was excluded from this analysis for two reasons: 1. HCl gas is not measured by CASTNET at the site, and 2. Previous measurements of HCl at RMNP have had very low concentrations.

39. Line 269: The stomatal and ground compensation points have already been defined as χs and χg.

The text "stomatal and ground compensation points" has been changed to "χ_{st}" in accordance with this comment and comment 43.

40. Line 270: Why did you not use the formula of Massad et al. (2010), which calculates Γs for Un-managed site based on nitrogen input? The Γs value calculated by this formula is about 10 times larger than the value used in this study. Do you expect this have any impact on the flux calculations?

We use a calculated value for the Γ_{st} because we made measurements of foliage at the site which allows us to directly calculate an estimated Γ_{st}. This value is different from values predicted by Massad et al. (2010). However, these emission potentials are highly variable from both Massad et al. (2010), and Zhang et al. (2010). This may have impacted flux calculations, and supports that increased measurements of foliage should be made to improve stomatal emission potential estimates.

41. Line 272: What exactly do you mean by "ratios" in this context?

Ratios here was just meant to indicate to the reader that they are unitless values. The text has been updated to "Emission potentials describe the potential for surface emission."

42. Line 275: Same comment with comment 39 for χs and χg. Also, "Eq. (3) and Eq. (5) of Stratton et al. (2018)" could confuse readers, as the equation numbers overlap with those in your manuscript.

"Ground compensation point" was replaced with "χ_g". As noted above, we appreciate the referee's feedback and understand how this may be confusing. However, we have decided to leave these equation numbers to aid the reader in referencing the original source material.

43. Line 281: The authors described "ground compensation points were calculated according to Massad et al. (2010)" in Line 269. However, they also described soil compensation point was calculated according to Stratton et al. (2018) in Line 275, and χg is not calculated follow the form of Eq. (11). If this is the mistake of Γg, I can understand. Which is correct?

Thank you for catching this. "Ground compensation point" should not be included in line 269. That section is describing the stomatal compensation point. Eq. (11) shows the calculation of $\chi_{st.}$ Eq. (12) shows the calculation of "χ_g", which follows the indicated equations from Stratton et al. (2018).

44. Line 286: Same comment with 40 for χc.

"Canopy compensation point" has been replaced with "χ_c".

45. Line 287: χa was already defined and explained earlier in Line 222.

The line "where χ_a is the atmospheric NH_3 concentration" has been deleted.

46. Line 291: Same comment with 43 for d and z0. Also, note that it is conventional to use lowercase z, not uppercase Z. Please revise accordingly.

Line 291 has been updated to directly use "d" and "z_0". Thank you for catching the typo of "Z_0" in this location.

47. Line 296: The explanation of inferred flux here is scientifically incorrect. It raises concerns about whether the authors even fully understand the resistance model framework. Also, "roughness height" is incorrect as already been pointed out by Referee #1.

Thank you for catching the accidental use of "roughness height" here. It would be helpful for the referee to explain what they take issue with in line 296. Line 296 read "Finally, the total flux was calculated following Eq. (17) (Massad et al., 2010)" at the time of this review. We have updated the text from: "Finally, the total flux was calculated following Eq. (17) (Massad et al., 2010). NH_3 flux is defined in this framework as a difference between the roughness height compensation point and the NH_3 concentration at that height, scaled by the aerodynamic resistance." To this: "Finally, the total flux was calculated following Eq. (17) (Massad et al., 2010). NH_3 flux is calculated in this framework as a difference between the χ_{zo} and χ_a, scaled by R_a." We see how use of the term "that height" is ambiguous and may confuse the reader.

48. Line 304: Please correct the section title to "Simulated bidirectional exchange flux of NH3".

We changed the section title in line 304 from: "Simulated bidirectional exchange of NH3" to "Simulated bidirectional exchange fluxes of NH3".

49. Line 307-309: This explanation has already been presented in the Methods section. And I cannot agree using the word of "relative magnitudes". Again, "Surface compensation point" is not an appropriate term; please revise all instances in the

manuscript. I suggest adding a plot of the "difference" between χa and χz0 in Figure 4. This "difference" directly determines the direction and magnitude of the flux and would provide valuable insight into seasonal variation.

We use the term "relative magnitudes" to capture that both the sign and size of χ_{z0} and χ_a matter. You are correct, using equation 17, the difference between χ_{z0} and χ_a determines the direction and magnitude of the simulated NH_3 flux. The sentence: "NH_3 flux direction is determined by the relative magnitudes of the NH_3 concentration and the surface compensation point (Fig. 4a.). " has been updated to "NH_3 flux direction is determined by the difference between χ_{z0} and χ_a (Fig. 4a.)."

50. Line 313: For clarity, please define the seasons as used in your study (e.g., spring = March to May). From Figure 4, it is not clear how "The largest periods of net emission occur in the spring" was concluded. It appears that the largest emissions occur from late June to July. Why are large depositions observed before and after this period (even in same summer)? What factors do you think are influencing these patterns?

The following text has been added to describe the seasons "winter (December, January, and February)", "summer (June, July, and August)" and "spring (March, April, and May)". Thank you for catching the error about net emissions. The largest net emissions do occur in the summer. It is an interesting question, this may be driven by the variability in transport patterns of NH_3 to the park, leading to highly variable NH_3 concentrations.

51. Line 321, 323: Same comment with 28.

"stomata" and "cuticle" have been replaced with "stomatal" and "cuticular"

52. Line 324: Based on Figure 5, it is difficult to support the claim that "Winter periods of net emission (see Fig. 4b) are driven by the ground flux." If these fluxes exhibit seasonal variation, boxplots may obscure such information, a time series plot would be more appropriate to reveal these dynamics because total flux is sum of these fluxes. Furthermore, as previously mentioned, some models assume no soil emission under snow-covered conditions. Then, why is soil emission estimated to be larger in winter when temperatures are low, and snow is present?

Considering the boxplot shown in Fig. 5, only the ground fluxes have large emissions. The stomatal and cuticular fluxes have a maximum value very close to zero. Therefore, emissions must be driven by fluxes from the ground. For this work, we did not incorporate a different parameterization for soil-emissions under snow cover conditions because of the large uncertainties in the effects of snow on NH_3 fluxes. This will be crucial future research to understand NH_3 fluxes in areas that have snow cover.

53. Line 325: Do you have any hypotheses or supporting information on how snow cover affects the flux?

We have added a small investigation of the effects of snow cover, by setting ground compensation point equal to zero in the wintertime. Some previous works suggest that snow cover would shut off ground emissions. However, there is limited evidence of fluxes directly to/from the snow surface. It would be a great focus of future research to understand how NH_3 is deposited to the snow surface.

54. Line 338: The term "surface exchange" may be more appropriate than "dry deposition" here.

We specifically use "dry deposition" because the discussion is around elevated NH_3 concentrations leading to deposition fluxes.

55. Line 346: The phrasing should be revised to “we also observe peak deposition fluxes” for clarity.

The phrase “we also observe peak fluxes” has been changed to “we also observe peak deposition fluxes”.

56. Figure 6: While the discussion of diurnal variation is valuable, analyzing the full annual dataset may obscure seasonal characteristics. Do the observed diurnal patterns hold true across all seasons, for example, during winter when concentrations and air temperature are lower? Are stronger daytime emissions observed during summer?

We previously looked at the diurnal pattern across seasons and found that the general shape of the diel profile was the same. Although the values differed, with the strongest daytime deposition observed in the summer and the strongest daytime observed in the winter.

57. Line 365-368: Is it also possible that the applied bidirectional flux model underestimates the actual amount of dry deposition? This relates to the previous question: why is dry deposition substantially higher in May and August compared to other months? Could the low dry deposition in June and July be due to large daytime emissions? Since wet deposition also become lower during these months, did less rainfall favored NH3 emission?

It is possible that the applied bidirectional model underestimates dry deposition. We will hopefully have a better understanding of this when we compare our gradient method fluxes of NH_3 from measurements on the NEON tower. For the scope of this paper, we are unable to assess this directly. In the text we are careful to say that our annual value for NH_3 deposition is smaller than previously estimated, but not indicate that this necessarily means that the bidirectional flux model is the correct answer.

58. Line 379: According to Figure 7(b), the largest NH3 dry deposition appear in “May” and August. Please revise the text accordingly.

Yes, thank you. The two maximum months are given as “May and August” in the text now.

59. Line 380-382: Can you explain why the proportion of reduced nitrogen is so high in this area, in relation to the transport and sources mentioned above?

This could be due to the reduction in emissions of oxidized nitrogen, or the large CAFO emissions of NH3 in the CO Front Range. In Lines 353 to 360 we discuss the emissions and transport of NH_3 to the park.

60. Line 389: Please be consistent in terminology. Use either “time resolution” or “time-resolution” throughout the manuscript, but do not mix both styles.

Instances of “time resolution” have been replaced with “time-resolution”.

61. Line 406: Please consider expanding your discussion on the reasons for the differences in modeled flux. If I understand correctly, one key factor may be the diurnal variation in the flux: At high temporal resolution, models usually reproduce large daytime NH_3 emissions driven by increasing temperature. At night, stomatal closure and reduced turbulence lead to larger stomatal and in-canopy aerodynamic resistances, suppressing emissions from both stomata and ground. Simultaneously, elevated relative humidity and acidity ratio (AR) enhance deposition. Therefore, deposition at nighttime may largely contribute to the annual dry deposition in 30-min resolution. However, at lower temporal resolution, these diurnal dynamics are averaged out, potentially leading to overestimation of emission and underestimation of deposition.

Thank you for this contribution to the discussion. In RMNP, we observe the largest deposition fluxes during the day, largely driven by NH_3 concentrations. This can be observed in Fig. 6c. You are correct that at lower temporal resolution, these diurnal dynamics are averaged out, which leads to an improper estimation of emissions and deposition. We have added this text: "Simulated NH_3 fluxes have a strong diel pattern when simulated at 30-minute resolution (see Fig. 6c), due to changes in NH_3 concentration and meteorology. These complex dynamics are averaged out when an average NH_3 concentration is used, which leads to an underestimation in deposition." to line 415.

62. Figure 10: While the overall trends of deposition and emission appear roughly consistent, there are notable differences in the magnitude of deposition fluxes in some cases. Both fluxes seem to exhibit systematic bias. What could be causing this? It is also puzzling that the annual dry deposition totals are same despite these differences.

We are not sure what the referee means here by "both fluxes seem to exhibit systematic bias". As such, we have not made changes to the text.

63. Line 438: Does this mean that you applied a monthly diel pattern of each month? If so, this sentence is an insufficient explanation.

Yes. We have updated this sentence from: "Annual deposition from all flux simulations using a monthly diel pattern fell within 2% of the annual deposition using the annual average diel pattern." to "Annual deposition from all flux simulations using each different monthly diel pattern fell within 2% of the annual deposition using the annual average diel pattern." To indicate that the results were the same for the diel pattern of each month.

64. Line 443: Please revise "Dry deposition inferential" to "Bidirectional exchange".

"Dry deposition inferential" was replaced by "Bidirectional exchange."

65. Line 446: Again, how did you simulate 30-min fluxes using ERA5 reanalysis data with 1-hour temporal resolution?

We did this by allowing each hour to represent both 30-minute time steps it contained.

66. Line 451-452: This sentence is unclear and please rephrase. Figure 11 suggests that the ERA5-based flux show smaller emission compared to the NEON simulation, which could result in the higher dry deposition amount.

That is correct; however, the ERA5 flux simulations also have smaller deposition fluxes. When we consider the annual flux, both emission and deposition play a role.

67. Figure 11: The title appears redundant since it merely repeats the caption.

Thank you, we have removed the title here.

68. It may be more informative to plot by specifying season or by day/night. This could help identify reasons for the discrepancies more clearly.

We looked at the plots by season and by day/night. Ultimately, however, we felt this orientation best aided our discussion.

69. Line 460: Same comment with 44 for Ra and $\chi z0$.

The long form of each was replaced with the shorthand.

70. Line 472: Same comment with 66 for u* and L.

The long form of each was replaced with the shorthand.

71. Line 480: Same comment with 68 for L.

If the u* from ERA5 were corrected using NEON, would the simulation results become more consistent between the two datasets?

They become more consistent, but it does not fully account for the discrepancy. This is described in lines 501 to 505.

72. Line 481-482: Do the observed differences in Ra directly cause the discrepancies in dry deposition amount? If so, please describe. Also, it is unclear why χz0 values are consistent despite significant differences in Ra (in Line 461-462).

Yes, from our analysis, differences in R_a appear to drive differences in simulated NH_3 fluxes. The χ_{z0} values are likely consistent despite the difference in R_a because there are many factors contributing to its calculation.

73. Moreover, Figure S2(h.) shows a large discrepancy in RH (R2 = 0.34), and ERA5 values being substantially higher than NEON. As RH is a critical input for Rw, how does this discrepancy influence fluxes? Why was this not discussed in the main manuscript?

When we directly compared the R_w results from simulations with NEON and ERA5, we did not find a large discrepancy between them.

74. Line 485: Considering the editor's comment, the phrase "best simulated" may be inappropriate at this stage.

Good point. We have updated the conclusions in accordance with this comment and the editor's comments. The first sentence now reads "Fluxes of NH_3 (g) can be simulated using a bidirectional model".

75. Line 489: As noted previously, please use consistent terminology throughout the manuscript: either "bidirectional" or "bi-directional," but not both.

Use of "bi-directional" in the conclusion instead of "bidirectional" has been updated to be consistent with the rest of the document.

76. Line 510-514: This methodology is quite promising. If you could briefly describe its potential applicability to other sites or its utility for future research, this would enhance the academic contribution of this study.

Thank you for contributing to shaping this section in a way that will aid future research efforts. This sentence has been added: "Understanding how to correct biases introduced through the use of reanalysis data would allow improved modelling of NH_3 bidirectional fluxes in regions lacking high-time resolution measurements."

77. Figure S7: The meaning of "calculated fractional differences" is unclear. What can be seen from this figure is that the magnitude of flux in response to the χa scaling factor differ considerably by season, regardless of the two time periods.

We see where the confusion came from here and appreciate you pointing it out. We have changed the phrase "calculated fractional differences" to "relative changes". We specifically looked at the relative changes to understand how the flux magnitudes are changing given different initial fluxes.