EGUSPHERE-2025-2505 | Research article

Ammonia exchange flux over a tropical dry deciduous forest in the dry season in Thailand

Xu et al. (2025)

Response to Referee's Comments

RC1: 'Comment on egusphere-2025-2505', Anonymous Referee #1, 09 Jul 2025

https://doi.org/10.5194/egusphere-2025-2505-RC1

The manuscript entitled "Ammonia exchange flux over a tropical dry deciduous forest in the dry season in Thailand", written by Xu et al., presents a unique study to NH3 exchange between forest and atmosphere in the tropics. It is, to my knowledge, the first to present such data and is valuable to the general NH3 community. They systematically discuss the drivers of both NH3 concentration at different levels and the flux and suggest future studies that could answer questions raised in their own study. Their methodology, however, lacks clarity. As this could potentially affect a major part of their analysis, I recommend major revision. In addition, I have some specific comments and suggestions that could further improve their study.

We sincerely appreciate the Referee #1 for the constructive comments on our manuscript, as well as for recognizing the novelty and value of this study for NH_3 community. We acknowledge the concerns about the clarity of methodology for flux calculation, as this is the essential part of this study. In response, we have expanded the Methodology section to ensure transparency and reproducibility in the revised manuscript. We have also extended discussions particularly focused on the uncertainties in the measured fluxes.

[Comments on methodology]

1. Section 2.2: Flux calculation
    1. As the authors deployed instrument at 26 and 34 m above a forest with mean canopy height of 20 m, they are measuring inside the roughness sublayer (RSL). As they do not mention this, and refer to a paper that is not listed in the bibliography, I cannot verify whether they included this in their calculations and thus they could potentially underestimate their fluxes. (see e.g. Harman and Finnigan (2008), de Ridder (2010) or Duyzer et al. (1994))

    We fully agree with this comment regarding flux measurement conducted within the RSL. The aerodynamic gradient method (AGM) based on the Monin-Obukhov similarity theory (MOST), which we used in this study, is valid only within the inertial sublayer. Since MOST will not hold within the RSL, where the fluxes are affected by tall canopies, several studies have proposed a height-dependent correction factor (α) derived from measurement to correct the deviations from MOST (Duyzer et al., 1992). Physical based corrections using models have also been performed by Harman and Finnigan (2007, 2008). More recently, Melman et al. (2024) evaluated these methods and found that both measurement and physical based approaches were suitable for flux correction. According to Foken (2008), the RSL has a large temporal variation and extend up to 3 times canopy height. In this study, NH_3 concentrations were

measured at 34 m and 26 m above a canopy with a height of 20 m, suggesting that both measurement heights are likely within the RSL and correction for measured fluxes is necessary. Unfortunately, the observation system in this study was unable to obtain vertical profiles of wind speed. The recorded air temperature data only to the nearest 0.1°C. Therefore, flux correction using the above-mentioned approaches could not be performed.

Nonetheless, to evaluate the underestimation of measured fluxes, we corrected the fluxes using a tentative α of 0.75 following Matsuda et al. (2015). The value of 0.75 was decided considering values in previous studies (Duyzer et al., 1992; Bosveld, 1997; Wyers and Duyzer, 1997; Melman et al., 2024) as shown in Table R1. As a result, mean flux increased approximately 32% during both the leafy (from 0.140 to 0.185 μg m^{-2} s^{-1}) and leafless (from 0.158 to 0.208 μg m^{-2} s^{-1}) periods. For the new term of "leafy" and "leafless" periods, please see our response to Referee #1's Specific Comment (3). The details of this evaluation and the corrected flux are now included in the revised manuscript (please see Line 138-141, 495-499 in the Author's tracked changes version).

Additionally, we expanded the description of flux calculation methodology for more clarity (Line 112-124).

Table R1. The correction factor α for flux profile function in previous studies.

Reference	α for each height interval
Duyzer et al. (1992)	36-31 m: 0.95
	31-24 m: 0.80
Bosveld (1997)	36-30 m: 0.90
	30-24 m: 0.75
Wyers and Duyzer (1997)	34-26 m: 0.9
	26-22 m: 0.73
Melman et al. (2024)	0.84*

***: Height interval could not be identified from the original paper.**

- **Duyzer et al. (1992) https://doi.org/10.1016/0269-7491(92)90050-K**
- **Harman and Finnigan (2007) https://doi.org/10.1007/s10546-006-9145-6**
- **Harman and Finnigan (2008) https://doi.org/10.1007/s10546-008-9328-4**
- **Melman et al. (2024) https://doi.org/10.1016/j.agrformet.2024.110107**
- **Foken (2008) https://doi.org/10.1007/978-3-540-74666-9**
- **Bosveld (1997) https://doi.org/10.1023/A:1000453629876**
- **Wyers and Duyzer (1997) https://doi.org/10.1016/S1352-2310(96)00188-4**

2. At what time interval do the authors calculate the transfer velocity D, and how does that compare to the long measurement interval of dNH3? If possible, flux measurements should be done at 30 minute intervals, as turbulence and gradients are highly variable. I understand that this was not feasible with the given set-

up, however, long averages of gradients multiplied with a long average of the transfer velocity will lead to the 'Schmidt paradox' (Moene and van Dam, 2014), if there is not accounted for the 'cross-term' (see also chapter 2 of https://dx.doi.org/10.21945/RIVM-2022-0202). As I did not see a mention of this, I do not know whether the authors accounted for this. Moreover, the authors should discuss the potential error due to such a long measurement interval, including the effect of averaging over different stability regimes.

We fully agree that fluxes should be ideally calculated at shorter intervals than this study according to the variability in turbulence and concentration gradients (ΔC). However, high-resolution concentration data were not available in the system of this study, and concentrations were measured via manually changed filter-packs with sampling durations of around 4 hours at the daytime and 16 hours at the nighttime. Therefore, we calculated D at 10-min intervals using meteorological elements recorded by a 3D sonic anemometer (10 Hz) and averaged 10-min values of D over each sampling time for concentration measurement. And then averaged D was multiplied by ΔC to calculate flux. However, filter-packs were manually changed, and it made strict adherence to fixed sampling windows (e.g. exactly 09:00-13:00) impossible. For example, if a sampling ran from 09:04 to 12:48, we averaged D over the closest matching interval (e.g. 09:10–12:50). Therefore, the mean value of concentration, D, and flux were weighted by the sampling time. We revised the section of flux calculation for more clarity (Line 123-124).

We also recognize the concern regarding "Schmidt paradox", where flux calculated by averaging D (<D>) and ΔC (<ΔC>) separately (F = <D><ΔC>) may differ from the real flux ($\langle F \rangle = \frac{\sum_{i=1}^{N}(D_i * \Delta C_i)}{N}$) and neglect the "cross-term". However, it is systematically impossible to evaluate the deviations of F from <F> in this study. While some modeling approaches can predict 1-h concentrations from long interval measurements (e.g. 1 month), as shown in the references presented by Referee #1, we do not have the short interval data at SERS which required as inputs for these models. Nevertheless, to evaluate the potential effects on fluxes, we conducted an analysis in which we assumed that ΔC varies sinusoidally during the sampling period, with a period of 1 hour and an amplitude of 1, such that the average of the sinusoid matched the observed ΔC. This allows us to simulate the variability of ΔC at 10-min intervals and calculate tentative <F> (Fig. R1). The results showed that the difference between <F> and F was less than 5%, even with large fluctuations in ΔC (±100% of the mean). The difference became larger with larger amplitudes, indicating that accurate quantification of ΔC at short intervals is essential for reliable flux calculation. While this is based on an assumed pattern of variation in ΔC, it provides a rough estimation of the potential error and indicates the uncertainty in our flux calculation. According to Rutledge-Jonker et al. (2023), the omission of the cross-term can lead to an underestimation of the flux up to approximately 15%. However, our approach is based on a rough assumption, we think it is not appropriate to include these results in the revised manuscript. Instead, we added a discussion in the revised manuscript to acknowledge this uncertainty and limitations of our flux calculation method (Line 500-505).

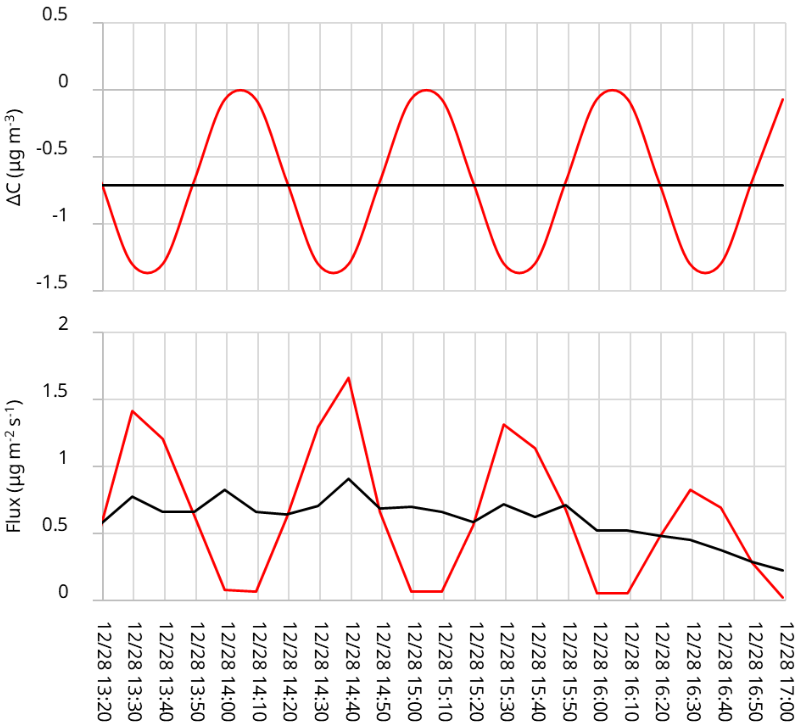

Fig. R1. Estimated fluxes of D2 on 28 Dec 2023 assuming ΔC varies at 10-min. The black lines represent the case where ΔC is constant, while the red lines represent a scenario in which ΔC fluctuates sinusoidally with a period of 1-h and an amplitude of 1. 10-min values of D was used for flux calculation.

Regarding the effect of averaging over different stability regimes, we calculated the percentage of unstable conditions in each sampling time. As shown in Table R2, the impact of mixed stability is likely negligible during the daytime as unstable conditions generally dominated. Because the sampling time at nighttime is relatively long, about 20% of unstable conditions (at the morning) mixed with neutral and stable conditions. Although we are currently unable to quantify how this condition affects the mean nighttime flux, it does not influence our overall conclusion that NH_3 emission predominantly occurs during the daytime. This discussion is now provided in the Uncertainty section of the revised manuscript (Line 505-509).

Table R2. Mean percentage of unstable conditions in each sampling time.

%-unstable	D1	D2	N
Leafy period	100	95.0	18.3
Leafless period	100	99.3	21.0

2. Section 2.3: The authors report the used meteorological instruments measure at an interval of 10 minutes. Does that mean that they sample only once every 10 minutes, or that there measurements are averaged/valid over that 10 minutes. Moreover, how do they obtain the meteorological input for flux calculation (by averaging?) and how do they obtain the meteorological conditions that they ultimately compare to the concentrations and fluxes?

The 3D sonic anemometer records meteorological elements at a frequency of 10 Hz, and the data logger processes the data into 10-min statistics (e.g. mean of wind speed, covariance between horizontal and vertical wind components). Friction velocity and Monin–Obukhov length that required for calculation of transfer velocity (D) were derived from the 10-min values. Air temperature and humidity were recorded as instantaneous values at 10-min intervals. When compared with fluxes and concentrations, all meteorological elements were averaged over the sampling time for concentration measurement, as described above for D. These details have been clarified and added to the revised manuscript (Line 144-147, 260-261).

[Specific comments]

1. Line 43: How did NOx change during recent years?

   We have added a brief explanation in the manuscript to clarify that NO_x emissions in Asia peaked in the early 2010s and have been declining since then (Line 45-46).

2. Line 80: Is the range the difference over space or over time? And how heterogeneous is the surroundings? What is the standard deviation on the LAI measurements.

   The LAI was measured only once, on 15 December 2023, at 4 locations within approximately 50 m of the observation tower (Fig. R2). The range is the difference over space. We revised the sentence in the manuscript for clarity and added the standard deviation (Line 81-84). Although formal assessment of heterogeneity could not be conducted, the surrounding area is uniform with dipterocarp species dominating as describe in Section 2.1.

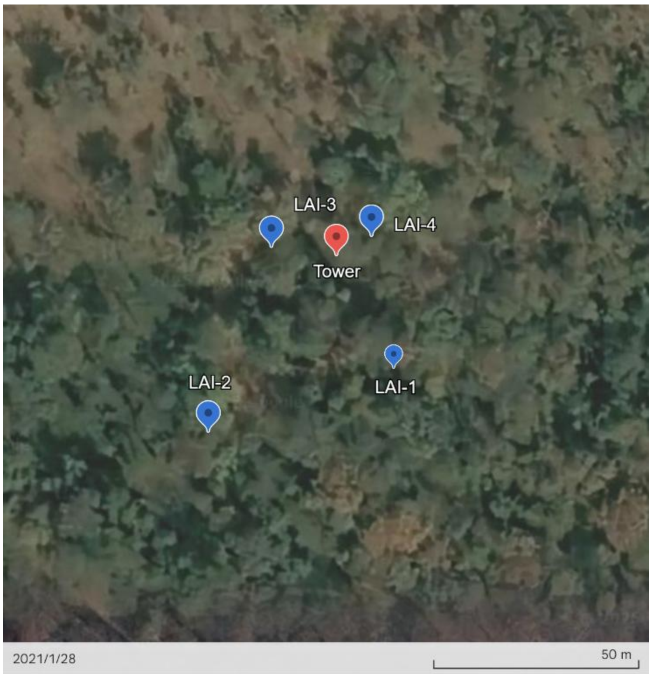

Fig. R2. Locations of LAI measurement on 15 December 2023. The map was made through ©Google Earth.

3. Line 99: first halve and second halve imply that the observation periods are consecutive. Therefore, I suggest to replace it with period 1 and period 2, or something alike.

   In this study, the observations were conducted under different canopy conditions: one with foliage and the other during leaf fall. Therefore, we prefer to use the terms "leafy period" and "leafless period" to emphasize the difference in canopy state, which is relevant to the analysis and interpretation of measured concentrations and fluxes (<u>Line 104-107</u>). We modified the entire manuscript.

4. Line 104: Matsuda et al. (2010) is not listed in the bibliography

   We apologize for our mistake and appreciate Referee #1 for pointing this out. We have now added Matsuda et al. (2010) to the References (<u>Line 751-753</u>).

5. Line 137: Is there any information on how these numbers compare to long term values of the site? Are they relatively high or low?

   To the best of our knowledge, this study presents the first NH_3 concentration measurements at the SERS site. Therefore, no long-term data are available for comparison. Instead, we compared our results with data from the nearest EANET station as described in the manuscript. Since the EANET station closed in 2020, continuous observations at the SERS site are necessary to further discuss concentration levels.

6. Line 171: Please specify what is meant by 'a high category'

   We used the term "a high category" to indicate that the observed NH_3 concentrations at SERS fall in the upper range when roughly compared with previous studies conducted in forest sites. We could not show direct comparisons in the manuscript because the data are not fully compatible; in some studies, only rough ranges are available from figures, and specific values are not reported. Nonetheless, to avoid any misunderstanding, we have revised the wording to "relatively high" (<u>Line 200</u>).

7. Figure 4 and Figure A1. the WD seems to have a diurnal cycle during a distinct amount of time, with different WD during night and daytime. I wonder what could cause such conditions and whether there are some submesoscale processes that could explain this.

   Generally, during the dry season in Thailand, the prevailing wind is known to be northeasterly due to the northeast monsoon from China and Mongolia. However, in this observation, southwesterly winds were also observed for several periods during the nighttime. We also wonder whether this might be a unique phenomenon related to the local environment at the SERS site. Therefore, we examined the wind data for the same period from the nearest meteorological station to SERS operated by Thailand. The Chok Chai Hydrometeorlogical Station (14.73945, 102.16472), located about 35 km northeast of SERS, is in a flat terrain surrounded by croplands and residential areas. As shown in the Fig. R3, during the leafy period, there were also periods when westerly winds were dominant (the available data lack measurement time to distinguish daytime and nighttime). This suggests that the southwesterly wind is not unique at SERS.

When averaged over the entire leafy period (15 Dec 2023 to 30 Dec 2023), the westerly winds at the Chok Chai Hydrometeorlogical Station are were not prominent, same as in our results at SERS. At this stage, we think the southwesterly wind at nighttime is just a short-term fluctuation rather than the result of some submesoscale process.

For the leafless period (22 Jan 2024 to 7 Feb 2024), we revealed that the change in wind direction occurred not only at SERS but also in almost all part of Thailand in another paper under review. This change was possibly due to a transition in monsoon, which is likely to occur from mid-February until the late May. However, this is central part for that paper, and it is not possible to show the detailed data here. Instead, we added more explanation regarding this change in the wind direction (Line 220-224).

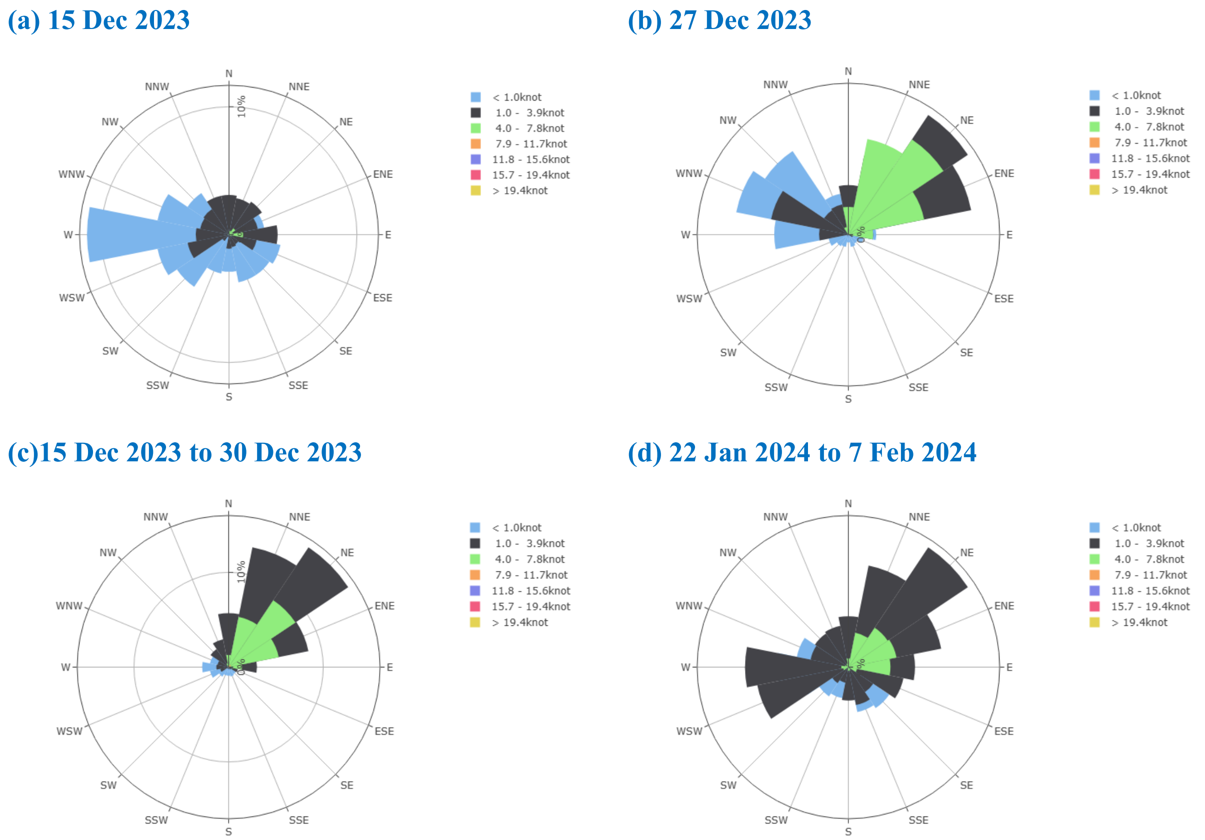

Fig. R3. Windrose at Chok Chai Hydrometeorlogical Station. The figures were automatically made by the Automatic Weather System of Thai Meteorological Department. Time series of wind direction could not be obtained from the system.

8. Section 3.2: There are more factors that could control the NH3 concentration, such as boundary layer height and entrainment, see also Schulte et al., 2020 (https://doi.org/10.1016/j.atmosenv.2020.118153).

We appreciate the Referee #1's suggestion to consider additional factors controlling NH_3 concentration, such as boundary layer dynamics and entrainment, as unraveled by Schulte et al. (2020). In a grass field in the Netherlands, they have revealed that the boundary layer dynamic controls concentration in the morning, and advection controls concentration in the afternoon. However, due to the limitations of the

measurement system, we do not have high temporal resolution concentration data necessary to conduct such analysis at the SERS using boundary layer models. Therefore, our analysis mainly focused on near-surface meteorological elements (e.g. air temperature, relative humidity, wind speed and direction) and canopy conditions (LAI). These variables were directly observed at the site and tightly associated with the observed NH_3 concentration. We have added a brief note in the revised manuscript to acknowledge this limitation and to suggest that future studies focused on such processes would be valuable for advancing the understanding of NH_3 dynamics (Line 311-315).

9. Figures 5 and 6: The different relation between the two different levels implicitly say something about the footprint of the two levels. It would be nice if the authors could add a discussion on this.
As also suggested by Referee #2, we conducted a simple footprint prediction using an analytical model. Please see our detailed response to Referee #2's Specific comments (1).

10. Line 226: Is there any scientific argument to exclude 1st February? Why are the concentration on this day so different?
As shown in the revised manuscript and our response to Referee #1's Specific comments (7), the wind direction at SERS (and in most parts of Thailand) shifted from the northeasterly to southwesterly from 1 February possibly due to a transition in monsoon. This change typically induces higher temperature throughout Thailand. At this time, the southwesterly wind might have brought a different air mass with low NH_3 concentrations. The increase in air temperature and low concentration at SERS during the end of the leafless period led to the negative correlations. Therefore, we tried to exclude the data after the wind direction changed to ensure consistency. We removed not only the data from 1 February, but also the data after that (2 February). As shown in Fig. R4, the low NH_3 concentration during this period caused the inverse correlation. We have now added a scientific argument in the manuscript for the data exclusion (Line 267-270).

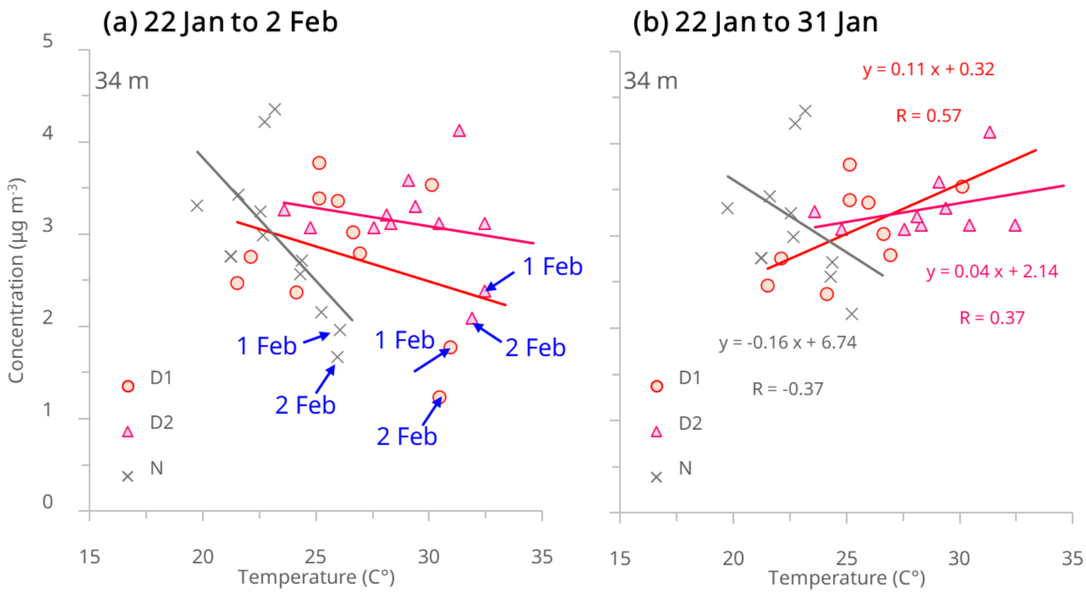

Fig. R4. Relationship between NH_3 concentration at 34 m ((a) all data, (b) excluding data after 1 Feb) and air temperature during the leafless period.

11. Line 254: Do the authors have any information on the amount of emission from the source area during the measurement campaign?
Unfortunately, we do not have any data on emissions from agricultural fields and residential areas in the surrounding area during the observation period. We acknowledge that activities such as the application of chemical fertilizers can significantly increase ambient NH_3 concentrations. In the Nakhon Ratchasima province, the main agricultural products are rice and sugarcane. Our observation period (from December to February) coincides with the harvest season of both products, and not with the growing season (autumn). Therefore, we assume that NH_3 emissions from nearby agricultural fields due to application of fertilizer were not extensive during our observation. However, we did not include this discussion in the manuscript because of lack of available data.

12. Line 276-277: please specify what you mean with 'roughly patterned'.
We revised the sentence for more clarity (Line 328-330).

13. Lines 277 – 279. I assume that the authors note on possible measurement errors due to these causes. Should be replaced to discussion on errors.
This comment is related to the next one, and our response is shown below.

14. Line 280: A paired t-test is usually used to compare different groups. Does that mean that you are here comparing e.g. all night samples at 26m to all night samples at 34 m? And if so, why did the authors do that? A difference could be significant at group level, but still be insignificant on a single instance. I think it would be better here to judge each gradient measurement individually by comparing the uncertainty/error on the measurements, unless the authors are only interested in an average flux (but the rest of the article addresses individual measurements). Also, how does this relate to the previous sentence? I.e., if the two levels are significantly different, how would that relate to the presence of emission sources or meteorological elements? I don't think I fully understand the intention of this sentence.
Before analyzing the individual possible measurement errors, we first performed a paired t-test to confirm whether a consistent difference in concentration existed between the two heights (34 m and 26 m). This was because NH_3 concentrations are not only subject to uncertainties in measurement but also strongly influenced by site-specific environmental and meteorological conditions. Although the paired t-test does not directly evaluate the uncertainty of individual measurements, it supports that significant vertical gradients exist above the DDF and provides a justification for using the obtained ΔC in flux calculations (Line 332-337).
We fully agree with the Referee #1 that the errors of individual ΔC should be discussed. Since parallel or duplicate sampling as in previous studies (e.g. Duyzer et al., 1992; Wolff et al., 2010; Ramsay et al., 2020)

could not be conducted at each height, we were unable to quantify the statistical error of ΔC. Instead, we estimated random errors of ΔC by propagating the analytical error of the ion chromatograph and the standard error of travel blank samples (n = 5), which were used to calculate concentrations. As a result, we confirmed that approximately 83% of the samples had a ΔC values exceeded the random errors (n = 66/80). There was no bias in the random error of ΔC between the leafy and leafless periods. We have now added a discussion in the revise manuscript (Line 338-343).

To further evaluate the reliability of individual ΔC values, we examined the ratio of each ΔC to the estimated random error (ΔC/σ). Over approximately 67% of the samples had ΔC values exceeded two times the estimated error (2σ), as shown in Table R3. We acknowledge that this approach is simpler than previous studies and plan to evaluate the error more systematically in future work by conducting side by side measurements.

Table R3. Percentage of ΔC exceeding the estimated random error.

(%)	D1	D2	N
>σ	76.9	81.5	89
>2σ	69.2	66.7	74
>3σ	42.3	33.3	44

15. Lines 298-299 I don't understand this. Figure 9 shows a quite large difference between D1 and D2, and N for all panels (including the transfer velocity)?

    Our intention was to explain that the difference in mean and median values of ΔC between daytime and nighttime was smaller than that of the transfer velocity. We revised the sentence for more clarity (Line 360).

16. Lines 329-349: The authors extensively discuss the stomatal conductance gs, and use measurements of August 2020 and from 1996 to explain their strongest emissions during D1. However, gs is ultimately controlled by ecosystem health and meteorological conditions, which might have changed over this time, especially when comparing over such a long period or over different seasons. The authors could make a much stronger case here by calculating the gs themselves using e.g. Emberson or an A-gs model (https://doi.org/10.1016/j.envpol.2006.04.007), which they could train on the CO2/H2O fluxes (if available) or otherwise on the previous measurements of gs. If the authors decide against this, they should better discuss the validity of their comparison.

    We extensively discuss g_s in this study because previous works have speculated that daytime NH_3 emissions originate primarily from stomata although most of them lacked direct measurements of plant physiological parameters or the discussion on the relationship between measured fluxes. While we did not measure stomatal conductance in this study, we discussed the relationship based on previous studies conducted at the same site. To our knowledge, this is one of the first detailed discussions on the relationship between measured NH_3 fluxes and g_s dynamics.

The g_s data from Tanaka et al. (2023) were not cited to support measurement at SERS, but to interpret previous flux observation by Xu et al. (2023) at the same temperate deciduous forest in Japan (FM Tama). Although we cannot show the data here, we found that the inferred NH_3 flux using the bi-directional exchange model of Zhang et al. (2010) was highly sensitive to the stomatal resistance (R_{st}), which is the reciprocal of g_s, in another paper under preparation. We also successfully reproduced the observed NH_3 flux by relaxed eddy accumulation method over the forest of FM Tama using the Zhang model, which formula for R_{st} were modified according to the parameters presented by Tanaka et al. (2023) for g_s. Without modification, the R_{st} calculated by the default formula of Zhang model will suddenly increase around noon, causing a sharp decrease in NH_3 emission flux. On the other hand, R_{st} calculated by the modified formula did not show such a sudden increase and show a good agreement with observed values. This was because the g_s of Tanaka et al. (2023) increased rapidly in the morning and remained almost plateau until the evening (please see their Fig. 3(d)). This case supports the comparison of flux and diurnal variation in g_s at the same site, and we consider it is more important to interpret the field measurement considering on-site information.

We appreciate Referee #1 for providing the model developed in Europe for calculating g_s. However, some input data required for the g_s calculations at the SERS (e.g. maximum g_s, species-specific parameters, soil moisture) was insufficient at this stage, and there was a high possibility that applying models and observation results from completely different climate zones and tree species would not reflect the characteristics of SERS site and induce furtherer uncertainties. Therefore, we cited Pitman (1996) because there were no other on-site observations at SERS to refer to, although the literature is old. As a next step, we plan to use a bi-directional exchange model to estimate the flux in SERS and verify our hypothesis on the relationship between flux and g_s.

・Tanaka et al. (2023) https://doi.org/10.1016/j.scitotenv.2023.164005

17. Lines 340-341: I assume the authors refer to different timing of emissions in Xu et al., 2023 and this study, please clarify the timings in the main text.

    While the sampling time was already described in the manuscript, we have now revised the text to further highlight the timing of the measurements to ensure clarity for the reader (Line 404-405, 408).

18. Lines 373-375: But then why the negative correlation with temperature during the second halve (especially during nighttime). Wouldn't you expect it the other way around?

    We have already addressed this point about the negative correlation between concentration and air temperature in our responses to Referee #1's Specific Comments (10). While we did not measure concentrations near the forest floor in this study, Xu et al. (2024) reported a positive relationship between air temperature and concentration at the forest floor in the FM Tama site during the leafy period possibly due to the litter decomposition (see their Fig. 6). We have recently conducted concentration measurements on the forest floor at SERS, and the forthcoming results will provide further insights.

    ・Xu et al. (2024) https://doi.org/10.1007/s44273-024-00042-z

19. Line 396: what do you mean with ‘obviously’

    We revised the sentence for more clarity (465-466).

20. Figure 10: Please consider to use constant binsizes for the two different observation periods, as this would allow for an easier comparison.

    We agree that constant bin sizes can make comparison easier and tried this at first. However, using the same bin sizes caused uneven data distribution in certain bins of relative humidity and friction velocity. To avoid this bias, we decided to use the current bin sizes to keep the number of samples even as possible.

21. Lines 434 – 459: This reads more as a results and discussion section. Please consider moving it to Section 3.5

    Section 3.5 of the manuscript is for discussions of uncertainty. We discussed a process that may affect the interpretation of flux although direct evidence remains limited, as well as measurement error. The dynamic exchange we describe is mainly based on previous studies, meteorological data, and the anomalously large emission flux at the SERS. While these findings are suggestive, they are not conclusive enough to confirm the presence of such a mechanism. Therefore, we chose to present this discussion as a potential source of uncertainty, distinguishing it from the more robust results presented in Sections 3.3 and 3.4. We believe that keeping these discussions separate helps to better clarify the main result and conclusion of this study.

[Technical comments]

1. Line 15: (…) we present the first observations of NH3 exchange (…)

   We revised the sentence (Line 15-16).

2. Line 44: therefor >> Therefore

   We corrected the mistake (Line 46).

3. Figure 1: Please use a white font for more clarity

   We revised the figures for more clarity (Line 89, 597). The revision of Fig. 1 also follow the Referee #2’s Specific comments (2).

4. Line 80: (…) at the beginning of the observation period (…)

   We revised the sentence (Line 83).

5. Line 275: (…) dC, because (…)

   We revised the sentence (Line 327).

6. Line 295: please rephrase “about four times”

We revised the sentence (Line 356-357).

7. Line 307: (…) acted as a source of NH3 (…)

   We revised the sentence (Line 370).

8. Figure 9: The authors could consider boxplots for more clarity

   We revised the figure as boxplots for more clarity (Line 374-379).

9. Lines 314-319. The authors could consider to uniform the units of all previous studies (i.e. all to ug/m2/s), as that would allow for an easier comparison.

   We agree that using a uniform unit is better for comparison. However, we used the original units of Ramsay et al. (2020) in the manuscript because converting ng m^{-2} s^{-1} to μg m^{-2} s^{-1} results in a number that is difficult to interpret intuitively (e.g., 0.00283 μg m^{-2} s^{-1}) and may decrease rather than improve clarity.

10. Line 336: please rephrase "And there was no change during this"

    We revised the sentence following the original expression in Tanaka et al. (2023) (Line 403-405).

11. Line 364: closed >> close

    We revised the mistake (Line 433).

    Line 442 & 456: Wentwortth >> Wentworth

    We revised the mistakes (Line 536, 550).

12. Line 456: (…) conditions. Notably (…)

    This was a mistake in the first manuscript and has already been corrected in the preprint version.

RC2: 'Comment on egusphere-2025-2505', Anonymous Referee #2, 21 Jul 2025

https://doi.org/10.5194/egusphere-2025-2505-RC2

[General comment]

This study presents a description and analysis of NH3 flux measurements conducted in a tropical deciduous forest in Thailand. Given the novelty of NH3 flux measurements in this ecosystem type, and the fact that most measurement campaigns have been conducted in temperate climates in Europe or North America, this study provides valuable new insights into the biosphere-atmosphere exchange occurring in tropical ecosystems. The authors discuss which exchange dynamics could be most relevant and identify the possible sources of emission fluxes. However, the presentation of the results, both in the text and in the figures, could be improved to enhance clarity and readability. In particular, some of the wording is vague or imprecise. Additionally, the manuscript would benefit from a more thorough and critical evaluation of the uncertainties associated with the NH3 flux measurements. Addressing these issues would significantly improve the manuscript and better highlight its important contributions to understanding NH3 biosphere-atmosphere exchange in tropical ecosystems.

We are grateful to the Referee #2 for the comments on our manuscript, as well as for evaluating the novelty and value of this study for NH_3 biosphere-atmosphere exchange in tropical ecosystems. In the revised manuscript, we have extended discussion especially focused on the uncertainties in the measured fluxes.

[Specific comments]

1. Line 81 – 82: The agricultural fields and residential areas are 400 m away from the observation tower. Have you performed further analysis (e.g., with the tool by Kljun et al. (2015)) on the footprint to ensure that anthropogenic activities do not interfere with your measurements?

   As we did not measure CO_2 fluxes, we could not conduct footprint analysis using such data in this study. Therefore, we are grateful to the Referee #2 for providing information on a simple model for flux footprint prediction. First, it should be noted that Kljun et al. (2015) recommend the use of their model to measurement heights (z_m) above the RSL. As discussed in our responses to Referee #1's Comments on methodology (1), measurement heights at this study are likely within the RSL. And the model returned an error when z_m was less than 20 times the roughness length (z0). Although the model still could provide footprint estimates within RSL using wind speed at z_m instead of z_0, the result becomes unreliable. Therefore, the analysis below was not included in the revised manuscript. To assess the potential source areas, we applied the model to two days in both leafy and leafless periods as examples. The estimated footprints on 28 Dec 2023 and 25 Jan 2024 are shown in Fig. R5. The result showed that the areas contributing to 80% of the flux are within the DDF in both periods. According to Kljun et al. (2015), 80% is sufficient to estimate the area of the main impact to the measurement. Regarding agricultural activities, please also see our responses to Referee #1's Specific Comment (11).

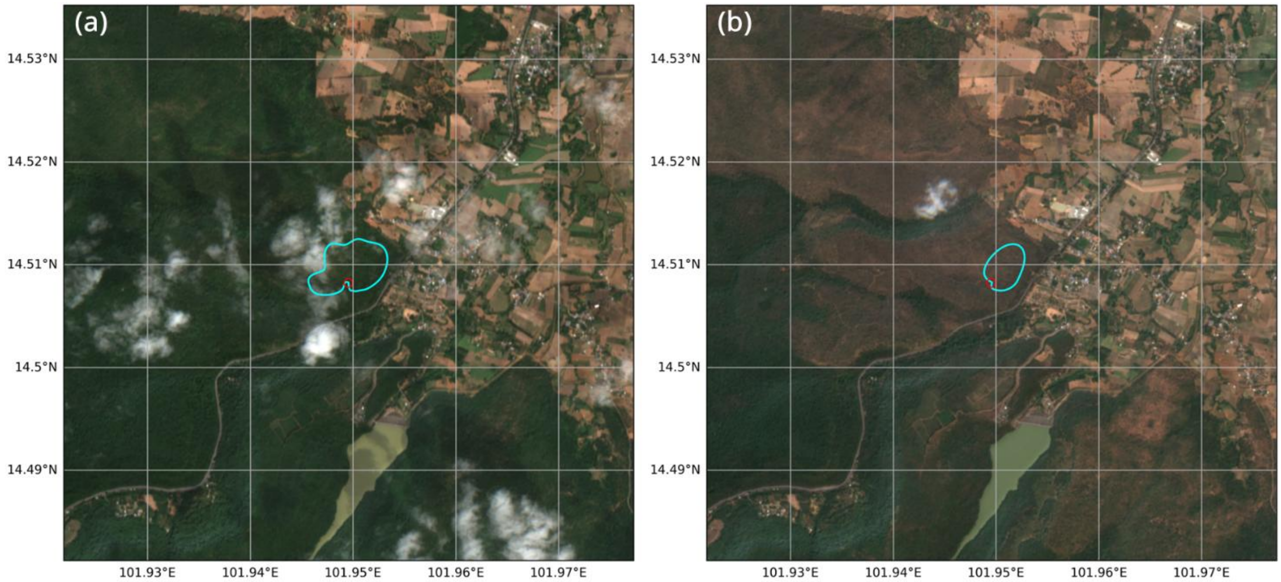

Fig. R5. The estimated footprints on 28 Dec 2023 (a) and 25 Jan 2024 (b). Circles indicate the location of tower. Footprint contour lines are shown for fraction of 80%.

2. Figure 1: Given the novelty of the flux measurements in this ecosystem type, I would consider adding one of the pictures of Figure A2 (or even merging these two figures). Moreover, the last sentence of the figure caption states that the data for the product has been filtered to include only those with a cloud cover percentage of less than 20%. I believe this refers to the satellite-derived LAI, but it is not clear in this capture. Please specify this.
As the Referee #2 suggested, we revised Fig. 1 by merging with Fig. A2 (Line 88-95, 588-590).
Regarding the caption of Fig. 1, we would like to clarify that Fig. 1 only shows satellite imagery retrieved from Sentinel-2 satellite operated by the European Space Agency, and it is not related to satellite-derived LAI (a product of the MODIS sensor onboard the NASA's Terra and Aqua satellites). The sentence referring to the cloud cover filtering simply describes how suitable satellite imageries were selected for generating the base map, as high cloud cover is not appropriate for visualization. Since this part does not involve any LAI description, we are unsure where the misunderstanding by Referee #2 might have arisen from. If there is a specific part of the caption that seems ambiguous, we would be happy to revise it.

3. Line 103: Can you be more descriptive regarding the physics behind the transfer velocity, or at least include the unit?
To avoid duplication with previous studies, we briefly describe the AGM theory in the manuscript. We have now expanded the Methodology (Section 2.2) also following the suggestion of Referee #1. More information can be found about transfer velocity in the revised manuscript (Line 112-124).

4. Line 179 – 186: I would recommend putting these statistics in a table, making it easier for the reader to compare the meteorological circumstances between the first and second phases of the study.

Although we think Fig. 4 is sufficient for easy comparison, we have added a summary table in the revised manuscript (Table A1) to accommodate the Referee #2's request (Line 227-228, 604-606).

5. Figure 3: For the interpretation of the NH3 concentration during the two observation periods, it would be useful to also add the measurement error of the concentrations to this figure as error bars or shaded uncertainty ranges. Now it is hard to judge how meaningful the patterns and differences between the measured concentrations are. Moreover, I think the readability of the figure would improve if you add gridlines. The latter also applies to Figures 4-6, 8-10.
We agree that adding error bars will give further information for the measured concentration in some cases. However, error bars are not included in Fig. 3 because the random error in concentration difference, which were estimated from the error propagation, is smaller than the observed concentration differences in most cases. Details on the measurement error are discussed in our response to Referee #1's Specific Comment (14). The main purpose of Fig. 3 is to illustrate the trend of temporal variation and concentration differences between the two sampling heights. Adding error bars reduce the overall clarity. We added gridlines for Figures 3-6, 8-10 for better readability in the revise manuscript.

6. Section 3.2: In this section, the factors influencing ammonia concentrations are discussed, with Figures 5 and 6 serving as the main supporting figures. Have you considered that many meteorological variables (e.g., temperature, wind speed, and relative humidity) are often strongly correlated, which can complicate the interpretation of individual relationships. Drawn conclusions could be misleading when potential multicollinearity is not accounted for. I would encourage the authors to explore the interrelationships between the variables and assess their potential impact on the analysis.
We acknowledge that meteorological variables such as air temperature, relative humidity, and wind speed can be correlated with each other. However, these variables influence NH_3 concentrations through different physical and chemical mechanisms. For example, an increase in temperature facilitate emission from leaf stomata and soil by increasing compensation point, while an increase in relative humidity enhances deposition. Therefore, it is more important to consider their individual roles separately rather than attributing observed correlations to the interrelationships.
To address this point, we showed the correlation coefficients among air temperature, relative humidity, wind speed, and solar radiation of each sampling time during the leafy period (Fig. R6). For example, temperature and wind speed were negatively correlated in the leafy period (both had strong correlations with NH_3 concentration). Following the logic of Referee #2, we could imply that temperature indirectly affects concentration through wind speed, or vice versa. However, this interpretation neglects the fact that temperature and wind speed affect concentration through entirely different mechanisms as discussed in the manuscript. While meteorological variables are often interrelated, it is also important to avoid misinterpretation due to spurious correlations.

D1	Temp	RH	WS	SR	D2	Temp	RH	WS	SR	N	Temp	RH	WS	SR
Temp	1.00	0.06	−0.74	0.35		1.00	0.07	−0.52	−0.10		1.00	0.62	−0.67	0.05
RH	0.06	1.00	−0.37	−0.35		0.07	1.00	−0.38	−0.22		0.62	1.00	−0.30	0.02
WS	−0.74	−0.37	1.00	0.20		−0.52	−0.38	1.00	0.68		−0.67	−0.30	1.00	0.06
SR	0.35	−0.35	0.20	1.00		−0.10	−0.22	0.68	1.00		0.05	0.02	0.06	1.00

Fig. R6. Correlation coefficients among air temperature, relative humidity, wind speed, and solar radiation of each sampling time during the leafy period.

7. Figure 4: I suggest moving the two column titles "1st half" and "2nd half" to the top of the figure (i.e., above 4a and 4e), as these column titles are now positioned between the tick labels and the caption of the figure, and get "lost" between the other elements. Moreover, have you considered also adding solar radiation and LAI data to this plot?

   We revised Fig. 4 and added the hourly variation in solar radiation (<u>Line 229-235</u>). We also added a brief explanation for the variation in solar radiation (<u>Line 224-225</u>).

   Given that the satellite-based LAI data are available only at 4-day intervals, incorporating LAI into the hourly plot of meteorological elements is not meaningful because only 4 LAI values are available for each observation period. More details can be seen in our responses to Referee #2's Specific Comment (11). Therefore, we chose to present LAI variation from Dec 2023 to Feb 2024 separately in the Fig. A3 in the previous manuscript (now Fig. A2), which offers a more meaningful information.

8. Line 206 – 207: "[…] found that daily NH3 concentrations have a strong relationship with the magnitude of temperature and may be affected by different processes during the daytime and nighttime". Could you briefly elaborate on the kind of processes referred to here to make this more informative?

   The sentence was originally quoted from Osada et al. (2020), in which the authors mentioned that "different nighttime and daytime processes" may affect NH_3 concentrations; however, we cannot find any description about the specific nighttime process. This statement in the original paper appears to be only a rough hypothesis. While Osada (2020) did not specify the nighttime processes, one possible explanation is that lower temperatures at nighttime may suppress emission from stomata, soil, or leaf litter, making temperature a less dominant factor. But this is just one hypothesis, and no direct data such as emission potential of stomata and soil are available to support.

9. Figure 5: I have several remarks about this figure. First, I would consider adding the y-axis label currently in subplot (c) to subplots (a) and (e) as well, to clarify that the y-axis concerns the NH3 concentration. Second, to justify the found correlations, I would recommend mentioning whether the regression functions here are statistically significant (e.g., by reporting the p-values, or if p-values are below 0.05). Finally, correlations have been shown for both at 36 and 24 meters in height, but the rationale for including the correlations at 24 meters is currently not explained. In other words, what was the reasoning for checking the relationships at height z1 as well?

We revised Fig. 5 and Fig. 6 accordingly (Line 255-261, 292-297). There were significant positive correlations between concentration and temperature at the daytime, and significant negative correlations between concentration and wind speed at daytime and nighttime during the leafy period (Line 239-240, 249). Correlations at both 34 m and 26 m are included to comprehensively understand how meteorological factors relate to NH_3 concentrations at these two measurement heights.

10. Line 222 – 229: This paragraph discusses how in the second half of the study campaign, the relationship between NH3 and the concentration was inverse at night time and little during D1 and D2. You discuss that omitting the data from February 1st changes the relationship again to a moderate correlation of 0.57 and 0.59. You mentioned earlier in the manuscript that the wind direction changes on this date. I suggest providing a (brief) explanation of why the correlations between temperature and NH3 concentration improve so significantly when omitting this data. Additionally, justify in this paragraph why you would omit this data.
We addressed this point about the negative correlation between concentration and air temperature. Please see our responses to Referee #1's Specific Comments (10).

11. Section 3.3: I was wondering if it was a deliberate choice not to include a time series of the measured fluxes as well. Such a plot can be a helpful way for readers to quickly understand the flux dynamics, such as day-night variations or temperature influences. Moreover, it would also be useful to display the (estimated) measurement uncertainty here per flux measurement. Finally, it would also be interesting to know whether the emission pulses increased over time in response to decreasing LAI, potentially due to enhanced leaf litter decomposition. Was there an observable increase in NH3 emission fluxes as more leaves fell from the canopy?
The temporal variation of measured fluxes with satellite-based LAI is now shown in Figure R7. The temporal variation in fluxes was not as pronounced as that of concentrations. The main feature observed was the difference between daytime and nighttime; however, this diurnal pattern is more clearly in the boxplot of Fig. 9. Moreover, Fig. 10 is better to show the distribution of fluxes in relation to each meteorological element. For this reason, we did not include this time-series plot of flux.
Regarding LAI, although the difference in mean values between the two periods (leafy and leafless) is substantial, the variation within each period is relatively small. At this stage, we cannot find direct evidence of the increase in NH_3 emissions associated with LAI decrease.
As for the measurement error in flux, we think it is better to discuss it in the Uncertainty section.

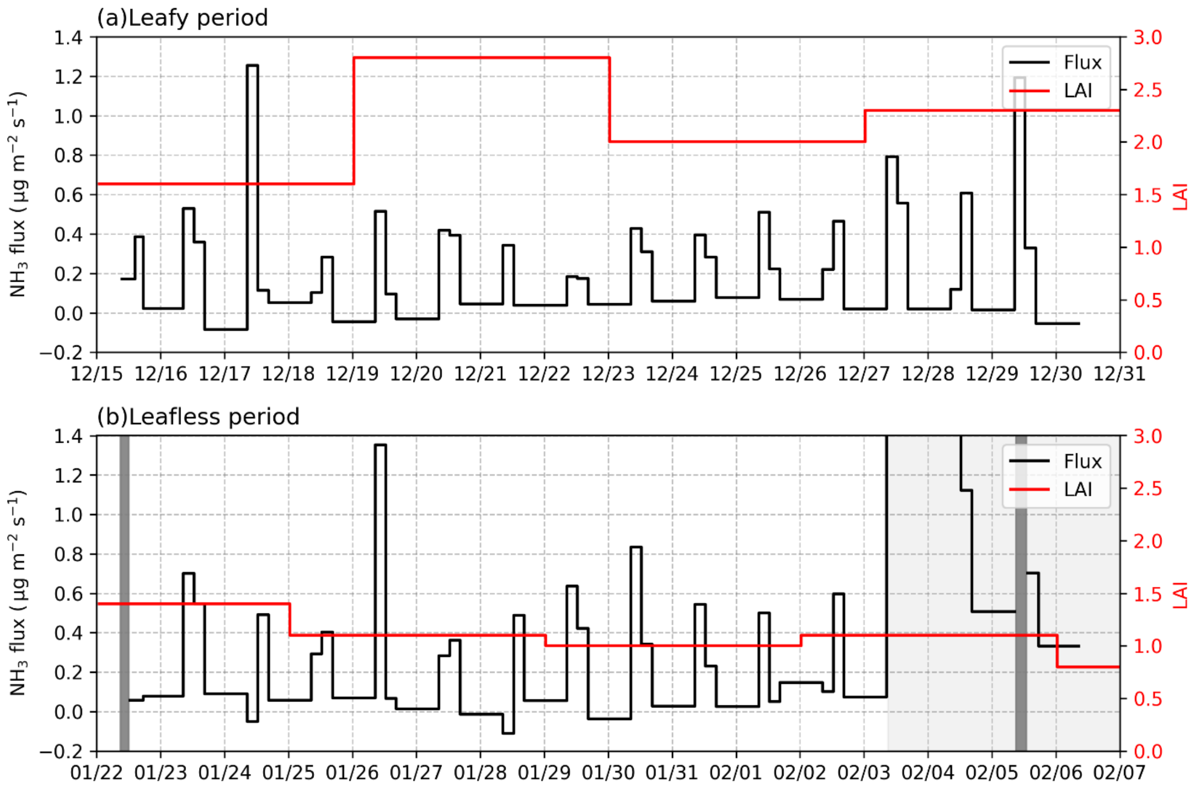

Fig. R7. Temporal variations in NH_3 flux and satellite-based LAI.

12. Sections 3.3 and 3.4: I would recommend restructuring these two sections, if possible, into one section. The first section focuses on the possible sources of the fluxes, whereas the second section focuses on the factors controlling the NH3 fluxes. In my view, these two subjects are too tightly interconnected to treat separately. I believe that discussing these two together will both aid in conveying your message more clearly and concisely, and in avoiding repetition.

    We agree that Sections 3.3 and 3.4 are closely connected in some parts. However, we prefer to retain them as separate sections because they play different roles in the manuscript. Section 3.3 discusses the characteristics of transfer velocity, concentration difference, and flux, and then proposes possible source based on the observed flux. Section 3.4 examines the meteorological factors that may influence the fluxes and provides additional support for the hypotheses proposed in Section 3.3. Moreover, Section 3.3 is already longer than other sections. Therefore, we believe that keeping the two sections separated improves the clarity and readability of the manuscript.

    If the Referee #2 strongly recommends restructuring the manuscript, we are open to dividing the Section 3.3 into two parts: one focusing on the flux characteristics and the other on the potential sources.

13. Lines 291 – 303: I would recommend putting the statistics (i.e., the weighted means and the standard deviations) in a table to improve readability.

Although we prefer to keep the current presentation to avoid redundancy, we have added a summary table in the revised manuscript (Table A2) to accommodate the Referee #2's request (Line 370-371, 609-611).

14. Line 346 – 349: Have you also considered that a decrease in LAI can also indirectly cause an increase in emissions, because (a) the forest floor becomes more exposed, and (b) the reduced LAI may diminish the canopy's capacity to act as a deposition sink through cuticular deposition?

    In the manuscript, the potential for increased emissions due to enhanced decomposition following a decrease in LAI has been mainly discussed with references to previous studies (e.g. Hansen et al., 2013; Xu et al., 2024). We also agree that decrease in LAI could reduce the canopy's capacity to act as a sink and suppress deposition. However, our current data does not provide evidence to evaluate this effect. Our future studies incorporating bi-directional exchange models could possibly simulate the impact of decrease in LAI on both emission and deposition processes.

    ・Hansen et al. (2013) https://doi.org/10.5194/bg-10-4577-2013

    ・Xu et al. (2024) https://doi.org/10.1007/s44273-024-00042-z

15. Line 393: "Observations and bi-directional exchange models have demonstrated that temperature is the most important factor controlling NH3 emissions (Flechard et al., 2013; Zhang et al., 2010)." Consider nuancing this statement, as indeed temperature is an important driver in the NH3-NH4 dissociation and Henry equilibrium – but temperature is also closely correlated with other key meteorological variables such as relative humidity and solar radiation.

    We agree that air temperature is often correlated with other meteorological variables such as relative humidity and solar radiation. However, this statement is intended to emphasize the direct effect of temperature on NH_3 emissions. As discussed in our response to Referee #2's Specific Comment (6), these variables influence NH_3 dynamics through different mechanisms, and we believe it is more appropriate to consider their individual roles separately rather than rely on the statistical correlations.

16. Line 403 – 405: Have you considered the potential influence of leaf phenology on the NH3 emission strength? Mattsson and Schjoerring (2003) found that the leaf senescence caused changes in the apoplastic NH4+ concentrations. Incorporating this aspect could provide additional insight into the observed flux patterns.

    In this study, we interpret that the source of NH_3 emissions likely shifted from stomata in the leafy period to leaf litter in the leafless period, according to leaf fall (LAI) and the changes in canopy structure (satellite imagery). As the Referee #2 suggested, we acknowledge that physiological processes, such as changes in apoplastic NH_4^+ concentrations during leaf senescence, could influence NH_3 exchange. However, our analysis is based on flux measurements over the canopy and discusses emissions from the entire forest. Therefore, currently we are not able to directly link fluxes to phenological changes in certain tree species. Future collaboration with plant ecologists may allow for a more detailed investigation of these relationships by measuring stomatal emission potential. Additionally, our future studies using a bi-

directional exchange modeling could help simulate how changes in stomatal emission potential associated with leaf senescence may influence fluxes.

17. Line 423 – 434: The median flux errors reported by Wolff et al. (2010), Ramsay et al. (2020), and Melman et al. (2025) are derived from high temporal resolution measurements, while this study relies on manual measurements. I am not fully convinced that a comparable measurement error of approximately 50% can be assumed without further explanation. Could the authors clarify whether they attempted to quantify the uncertainty of the flux, for example, through error propagation? In my opinion, the uncertainty of the fluxes needs a more critical evaluation to strengthen the credibility of the conclusion that the DDF acts as a net NH3 source during the observation period.

    We fully agree with this comment regarding measurement errors. Since we did not conduct high temporal resolutions and parallel measurements (Ramsay et al., 2020; Wolff et al., 2010), it was technically difficult statistically quantify the measurement error. Therefore, we estimated the random error of the measured flux in this study following the approach of Melman (2025). The error was calculated by propagating uncertainties in the u_*, ΔC, and the integrated stability correction function. As a result, a median random error of approximately 42% was estimated. Although this approach is more simplified than that of Wolff et al. (2010), we paid particular attention to the concentration difference, which most influences the error of flux. Multiple analyses using two ion chromatographs were performed to ensure quality control. The estimated uncertainty falls within the range reported in previous studies and therefore does not affect our conclusion that the DDF acted as a net source of NH_3 during the observation period. We added results and discussion in the revised manuscript (<u>Line 518-524</u>).

18. Figure 20: The caption of this figure contains repetitive information in the last two sentences, which have already been mentioned in the methodology section. Moreover, I would consider removing the sentence "(x" indicates values greater than x, and "x]" indicates values less than or equal to x, as this notation is standard and generally well understood. Moreover, increasing the font size of the axis labels and the tick marks would improve the readability. Finally, a technical correction is to change the numbering of this figure from 20 to 10.

    We revised Fig. 10 and the captions accordingly. For more clarity, we changed it to a boxplot following Fig. 9 (<u>Line 483-492</u>). The numbering is already corrected from 20 to 10 in the preprint version.

19. Line 434-436: You discuss the presence of outliers in the measured fluxes of D1, but it is unclear based on which criteria these are qualified as outliers. Moreover, are these outliers still taken into account in the calculation of the statistics mentioned in the paper?

    In the revised Fig. 9, the fluxes of D1 during the leafy period include two samples that exceed 1.5 times the interquartile range (IQR), and those during the leafless period also shows larger whiskers than D2 and N, suggesting the presence of potential outliers. However, these samples were not excluded from the analysis because the ΔC values exceeded the estimated random error, and no problems were identified during sampling or chemical analysis. Therefore, we did not have sufficient justification to treat these

samples as outliers. Instead, we discussed the possible influence of the dew as an explanation for these abnormally large fluxes based on previous studies in Section 3.5.

20. Line 464: "On the other hand, we hypothesize that NH3 concentrations are controlled by meteorological elements as well as by changes in canopy structure accompanied by defoliation. Specifically, the dominant NH3 emission source may shift dynamically from leaf stomata to leaf litter in response to changes in canopy, forest floor, and meteorological conditions.". These sentences refer to 'meteorological elements/conditions', which are a very general formulation. I would recommend rephrasing or combining them with the following sentences to improve clarity and avoid vague wording.
    We agree with this comment. We revised the sentence for more clarity (Line 557-561).

21. Section 4: I have a suggestion for future research: This study clearly demonstrates that the DDF ecosystem serves as an emission source during the dry season, likely due to leaf litter decomposition. However, this concerns a very seasonal process and may not be representative of NH3 flux dynamics on an annual basis. Have you considered analyzing whether this phenomenon of NH3 emission during the dry season is more broadly applicable to deciduous forests in tropical regions, with satellite observations (e.g., CrIS, IASI), could offer additional insights?
    We appreciate this suggestion regarding the use of satellite observations such as CrIS and IASI to explore NH_3 exchange at broader spatial and seasonal scales. However, these satellite sensors only provide column-integrated concentrations and currently have limitations in spatial resolution and retrieval accuracy (Someya et al., 2020). At this stage, we believe that continuous field-based flux measurements at SERS site should be given higher priority to capture seasonal variations, and we emphasized this in the manuscript. The SERS site is intended to serve as a long-term observation platform. Nonetheless, we agree that integration with satellite-based data could provide valuable insights into future studies.
    ・Someya aet al. (2020) https://doi.org/10.5194/amt-13-309-2020

22. Figure A3: I suggest combining this graph with Figure 4, as this is important information for the NH3 flux dynamics taking place at SERS.
    This is the same comment as the previous one and please see our responses to Referee #2's Specific Comment (7) and (11).

[Technical comments]

1. Line 79 – 81: The sentence "Leaf area index (LAI) [...] with a mean value of 3.1." would benefit from clearer structure and improved grammar for better readability. Second, do I understand correctly that the LI-COR has only measured the LAI at SERS once? And could you specify what the range "2.5 to 4.2" indicates here? Is this the uncertainty, or has the LAI been observed at multiple locations around the tower? Finally, you could also report that LAI values have been derived with the MODIS satellite, as mentioned at lines 231 – 233. This is relevant information, which should have been mentioned earlier in section 2.1.

Regarding the measurement of LAI, please see our responses to Referee #1's Specific Comment (2). Since Section 2.1 focuses on the site description, we added a new Section 2.4 to describe the satellite-derived LAI prior to the discussion in Section 3.2 (Line 150-156, 275-279).

2. Line 104: i.g. should be e.g.
   We corrected the sentences (Line 116).

3. Line 104: To maintain consistency in notation, consider including the symbol u* for friction velocity after introducing the full term, similar to how you denote displacement height (d).
   We denoted friction velocity as u_* in the entire manuscript.

4. Line 114: I found myself needing to return to this line when interpreting Figure 5 to recall what D1, D2, and N referred to. It may help the reader to place slightly more emphasis on the naming convention of the daytime and nighttime samples, as it is currently understated. E.g., rephrase it to: "We continuously collected two daytime samples, denoted as D1 and D2, corresponding to the periods 09:00–13:00 and 13:00–17:00, respectively.".
   We corrected the sentences (Line 128-130).

5. Line 171: I suggest replacing the phrase "[…] in a high category [...]" with more specific wording, as it is currently vague.
   This is the same comment as Referee #1 and please see our responses to Referee #1's Specific Comment (6).

6. Line 183: I would suggest replacing 'variation' with 'relationship'.
   We prefer to keep "variation" as it describes independent temporal changes. Using "relationship" may imply a causal or interactive link between temperature and wind speed.

7. Line 236: I suggest clarifying briefly that "[…] which is close to the observed mean value around the observation tower" is measured by the LI-COR LAI-2200 for clarity.
   We corrected the sentences (Line 280-281).

8. Line 242: "process" should be "processes".
   We corrected the sentences (Line 286-287).

9. Line 271: "[…] during the daytime, it remained above a certain level" – I would recommend making this sentence more specific about what the "certain level" refers to.
   We corrected the sentences (Line 322).

10. Line 355-356: "[…] which has an even lower pH and poorer nutrient"; this sentence seems unfinished.

We corrected the sentences (Line 424).

11. Line 273: Consider replacing ‘progress’ with ‘occur’ or ‘proceed’

    We corrected the sentences (Line 442).

12. Line 408-409: The sentence “In contrast, the change in median flux after the first increase was smaller in the second half” is vague and would benefit from clarification.

    We corrected the sentences (Line 478-479).

13. Line 423: The phrase “Although the case is limited, […]” is vague and requires more specific wording.

    We corrected the sentences (Line 511). Our intention was simply to note that the number of studies reporting high time resolution NH_3 fluxes using AGM over forests is limited.

14. Line 429: replace ‘term’ with ‘terms’

    We corrected the sentences (Line 517).

15. Line 457: replace ‘depend’ with ‘depends’

    We corrected the sentences (Line 551).