

Review of Hamilton et al. (2026): Eddy covariance measurements of nitrogen dioxide exchange at a grazed savanna grassland in South Africa

Hamilton et al. (2026) present an analysis of NO₂ flux measurements from a South African shrubland spanning multiple years. The authors investigated flux patterns and deposition velocities in relation to environmental parameters and found significant differences between measured and modelled deposition velocities. These findings highlight the need for a better representation of semi-arid ecosystems in dry-deposition modelling frameworks. Consequently, additional measurement campaigns focusing on reactive compounds in African landscapes are needed. This long-term flux record is important for model development and is—at least to my knowledge—the first high-resolution dataset of NO₂ fluxes from an African savanna ecosystem. The study therefore represents a valuable contribution and is worthy of publication.

However, the manuscript needs substantial revision before it is suitable for publication. The authors omitted important details about the flux processing and quality assurance, making it difficult for the reader to assess the quality and reliability of the flux measurements. The results are presented in an overly detailed manner, with a large number of numerical values reported directly in the text. Combined with figures that are often difficult to interpret because of inconsistent scales and excessive detail, this makes the manuscript challenging to read. I encourage the authors to present the results more concisely, summarize key information in tables where appropriate, and move supplementary material to an appendix or supplementary section. Separating the Results and Discussion sections may also improve readability. I provide major, minor, and technical comments below.

Major comments:

- R1: Chapter 2.2: I miss essential information about the flux calculation. Did the authors use a commercial software or self-developed flux calculation program? If the latter is true: How comparable are the flux estimates to a commercial software like EddyPro? What were the steps used to obtain fluxes, e.g., despiking, detrending, rotation method to adjust the sonic anemometer, time lag compensation method? What was the time lag? Was the flow inside the tubes laminar or turbulent? This is very important for flux attenuation which can be significant at a 20m inlet line, especially for reactive gases like NO₂. No information is given on flux attenuation in the manuscript nor on the power or cospectra. Were the fluxes corrected for attenuation effects? Please incorporate this information into the analysis.
- R2: Almost all figures need revision. Thus, it is difficult to extract their main message and they are not really supportive to follow the detailed text. Please adjust the scale, make it uniform if subplots are used, increase label size, remove redundant labels, etc.
- R3: Given the length of this dataset, calculating cumulative estimates could be worthwhile using machine learning frameworks. This could be done for example for the

last year (August 19 - August 20) since this year has the largest data coverage and no prolonged data gaps.

Minor comments:

L. 32: Please use a uniform unit notation. Here, dots are used to separate units. At other places in the manuscript, for example L.139 ms^{-1} , is written without delimiter.

L. 77ff: I suggest to rewrite the introduction of the analysis. Now, the reader thinks your main focus of the analysis is the compensation point. I would recommend a step-by-step list of topics what the reader await in the analysis and discussion part.

L. 85ff. please write what are the supporting measurements or at least name the most important ones which were used in this study.

Fig. 1: Enlarge labels and dots. About the trajectory map: A lot of information is missing here: Which model was used? What was the resolution? What input data was used. The colour bar is not visible not clear what it is actually showing. The white star is not visible and its legend neither. Adding a Windrose to the map is would be helpful.

L. 102: I don't see the borders. May you put a map underneath showing the vegetation? Pictures of the site would be even more helpful.

L. 104: Are the (monthly/annual) footprints of this study comparable to Räsanen et al. (2017)? Was the annual/monthly variation significant?

L.131: What is the time response (e-folding time) of the QCL? This is an important information for suitability of sensors for eddy-covariance measurements.

L. 133: Please add the Reynolds number. According to calculations, the value is near the optimal value of 2300 (~2275). Thus, there can be a significant impact on the flux loss in the high-frequency range which is not discussed. (see comment R1)

L. 139: How was the ustar threshold chosen? Based on the ustar threshold algorithm or fixed to 0.2? Is there a significant difference to lower value of 0.1 m s^{-1} ?

Fig. 2: I would recommend to put this figure in the Appendix or remove it. Instead, add to the table above the data coverage after quality-checking per year. Due to the scale, is hard to get anything from the time series. A flux does not have a unit ppb. Is the flux zero, because there is no data in 2017? Then use nan there. To the lower plot: Are the unit ppm or ppb. In the caption, it says ppb. Also, please use the same scale on x and y-axis.

L.141 ff: I would move LL. 141-149 to Sec 2.3. It suits better to the flux modelling section.

L.172 ff: Was constant LAI and vegetation height used in flux modelling? Were measurements of the LAI and vegetation height done during the measurement period? Why not using a time series of these variables? An LAI from 0.4 to 2.3 is quite significant. Or an LAI depending on wind direction as input for the model?

L. 174. The abbreviation MODIS is not explained. How was the MODIS data obtained?

Fig. 3: This plots should be put in the appendix. Again, it is difficult to examine patterns. I would plot here only a specific time period as there is no need to plot the gaps here. Plot a period from

August 18 till March 19 (until data collection stops). Also adjust the limits of the y axis, especially for the NO₂ flux. The trend is important to see, not all the spikes.

Sec. 3.1: Here a table would be suitable for summarizing all the numbers. Then, the description of the plot can be shorter. Focus in the text on the main points. What is most interesting for the reader? The rain fall events causing the NO₂ pulses. Also remove redundant sentences like 191ff beginning with “this aligned vertical structure ...”. (See also the general comment)

Fig. 4: Avoid mixing different date formats. Instead of Day of Year, I would recommend to use month labels. Maybe then only the specific period if Figure 3 goes to the Appendix.

Fig. 5: add the number of valid data to each box. Therefore I would use a table

L. 241: For my understanding: Fluxes are multiplied with N or the molar mass of NO₂?

L. 250: It should be clear that EC flux measurements provide only net fluxes. I think is not needed to repeat it if it already stated in in 2.2 and in 3.2.

L. 251: Maybe put Figure 7 and at different place. Now, it is on page 17. Put a reference to figure 5 in the follow-up sentence.

L. 258-260: If the number of observations is that low, I would not integrate the data in plot. Then, the flux data are not representative for the month.

Sec 3.2.1 i) :Same as for Sec. 3.1. Less details and be more concise and focus on the important observations of the flux patterns of Figure 5.

L. 282: Again, if the data coverage is that low, I would recommend to discard these data and don't draw conclusions on long-term means and medians if they are based on peak events.

L. 296ff: This can be shorter as well. Specific dates and the flux values of these extreme events can't be seen in Figure 5 and maybe not needed to be described since it is already indicated by the large IQRs.

L. 306: Are median or mean diurnal trends plotted? In the caption of Figure 6, it is stated “mean”.

L. 315: Does this belong to Figure 5 or 6? Figure 6 has standard errors as shaded areas, no IQRs

Fig. 6: Please use the same format of y-values and also the same scale. Then monthly differences are visible. Remove redundant y- and x-tick labels in columns and rows, respectively.

L 331: Brümmer et al measured total reactive nitrogen, not NO₂ fluxes.

Fig. 7: I would add the month in brackets after the seasons. Again, use the same scale for the y-axis. If the diurnal pattern is not complete like for autumn 2017 due to a low data coverage, please remove them. Please check the other seasonal plots as well.

L. 365: Here, I read local time for the first time. Please indicate in the plots or the method section which time is used, local time or UTC.

L. 405: Please introduce the term NEE.

Figure 8: Use the same y-scale, and remove redundant y-tick labels. Then the months can be compared. Trends are here more important. Due to the scale, nothing can be seen for June, July, August, April, and February.

L. 413: be careful with the wording: You separate your fluxes, you do not partition them. Your deposition flux is still a net flux, if you just separate them by using the vertical wind direction. CO₂ fluxes are partitioned in respiration and gross primary production.

Sec. 3.2.2: This section appears like a repetition of Sec. 3.2.1. Again, this section is difficult to read due to all the details. My suggestion is remove or shorten Sec. 3.2.2 substantially and go directly over to the deposition velocity section.

L. 465-468: If the number of valid observations is that low, do not conduct an analysis for the respective month.

Sec 3.3: Also here, a table may help to summarize all the deposition velocity results.

L. 505: Please write out abbreviations if they occur for the first time.

L. 511-520: Very difficult to read. Too detailed description of Fig 10. Focus on the main messages.

Fig. 10: Why is 2015-2016 mentioned in the title although the first row shows annuals from 2015 until 2020? For consistency reasons, change the numbers 1 to 12 to month names. Are the monthly v_d patterns for the entire campaign or 2015-2016 only?

L. 611: I would not make comparisons of the compensation point of trees here. Also these measurements were done in a different climate zone.

L647: As written above: separation is not the same as partitioning

Reference section: Some references do not have a DOI. Please add a DOI or a link where it is possible.

Technical comments:

L. 20: comma after here

L. 107: italic "et al". Please also check for other occurrences in the manuscript, for example L. 149, L. 151, L. 492, etc.

L. 152: set the 2 in H₂O as subscript.

L.186: change reactive nitrogen to NO₂

L. 327: Add a reference to the Birch effect.

L. 497, L. 578: Large V in v_d

L. 646: set 2 in NO₂ as subscript.