Authors’ response to Referee#2

First of all, we would like to thank the Referee for his/her positive evaluation of our manuscript and would like to thank him/her for the comments and suggestions, which have helped us to improve the manuscript. Below you will find our detailed, point-by-point responses to the comments and suggestions. Responses are given in blue.

Haszpra et al., present a valuable 10-year record of CO fluxes from a tall tower in rural Central Europe, a rare dataset given the scarcity of long-term CO flux monitoring. The authors present CO fluxes for quasi-natural land and populated sectors, attributing fluxes to these categories using a directional flux distribution based on footprint-weighted flux maps. They find that the vegetation-dominated sector acts as a weak net CO source during the growing season (April–September), with a midday peak and no statistically significant nighttime exchange. Solar radiation is identified as the primary flux driver. Populated areas emit CO at rates substantially higher than activity-based inventories suggest, with residential heating identified as the likely underestimated source. These findings are interesting and well presented, however, several methodological aspects require clarification or strengthening before the results can be accepted for publication. The main concerns are 1) general clarity in Methods section, 2) PBL height from ERA5 and its interaction with the nighttime filtering approach and 3) storage flux assumption. These points are elaborated below.

Major comments

1. **The Methods Section needs more clarity and better structure.** There are several things that need better explanation. For example, it is not clear if the meteorological sensors cited on L116 include a temperature profile, which actually could be use to get a rough idea of the nighttime PBL height. Then in Section 2.2 the reader learns that Air temperature is only measured at 10 m, so what are those meteorological measurements on several heights mentioned earlier? Another example is when the authors state (L186) that “lag time estimation was done interactively by visualizing the lagged covariance data for predefined time windows.” What does “interactively” mean, what was the day to day variability of the lag? One last thing that could improve readability, is that the authors could describe the tower site first and then separately, describe the instrumentation in another (sub)section.

Thank you for the comments. Carbon monoxide measurements are taken at a single level of the tower, at 82 m elevation. All other measurement levels are primarily dedicated to research related to carbon dioxide (Haszpra et al., 2015; Haszpra, 2024). Since these measurements are irrelevant for the present work, we followed your suggestion (minor comments) and that of the other Referee and have removed their presentation from the revised manuscript. Temperature profile measurements can indeed provide approximate information on the existence of a low-level inversion. However, carbon monoxide measurements are taken near the top of the tower, and the single thermometer located above the EC system no longer provides reliable information on whether the EC system is inside or outside the inversion layer.

We have modified the methodology section. As the other Referee also asked for a more detailed description of the eddy covariance data processing workflow, including the lag time estimation, we now provide ample information to make the workflow reproducible and transparent. Our original idea was to provide only a brief description, as the methodology is published in our previous works, but we understand that it is better to have everything in one place for improved readability. At the same time, we sought to maintain a balance between the length of the methodology chapter and the chapters presenting the results/discussions, since the novelty of the study primarily lies in the results and conclusions, not in the methodology.

In the revised manuscript, we now provide a description of the lag time calculation. The figures below, replotted from Haszpra et al. (2018) Supplementary material, demonstrate that the daily, interactive lag time calculation is the preferable method. In brief, the covariance maximization method would frequently lead to erroneous lag time values, indicating a highly variable lag time that is physically not reasonable. Assuming a constant lag time for each particular day is supported by the relatively constant performance of the pumps. It might be possible to estimate lag time in 2-3-day windows, or even longer windows, but we decided to follow the daily data handling routine that proved to work well for the tall tower eddy covariance system for decades (the same lag time estimation method is used for CO_2 flux calculation).

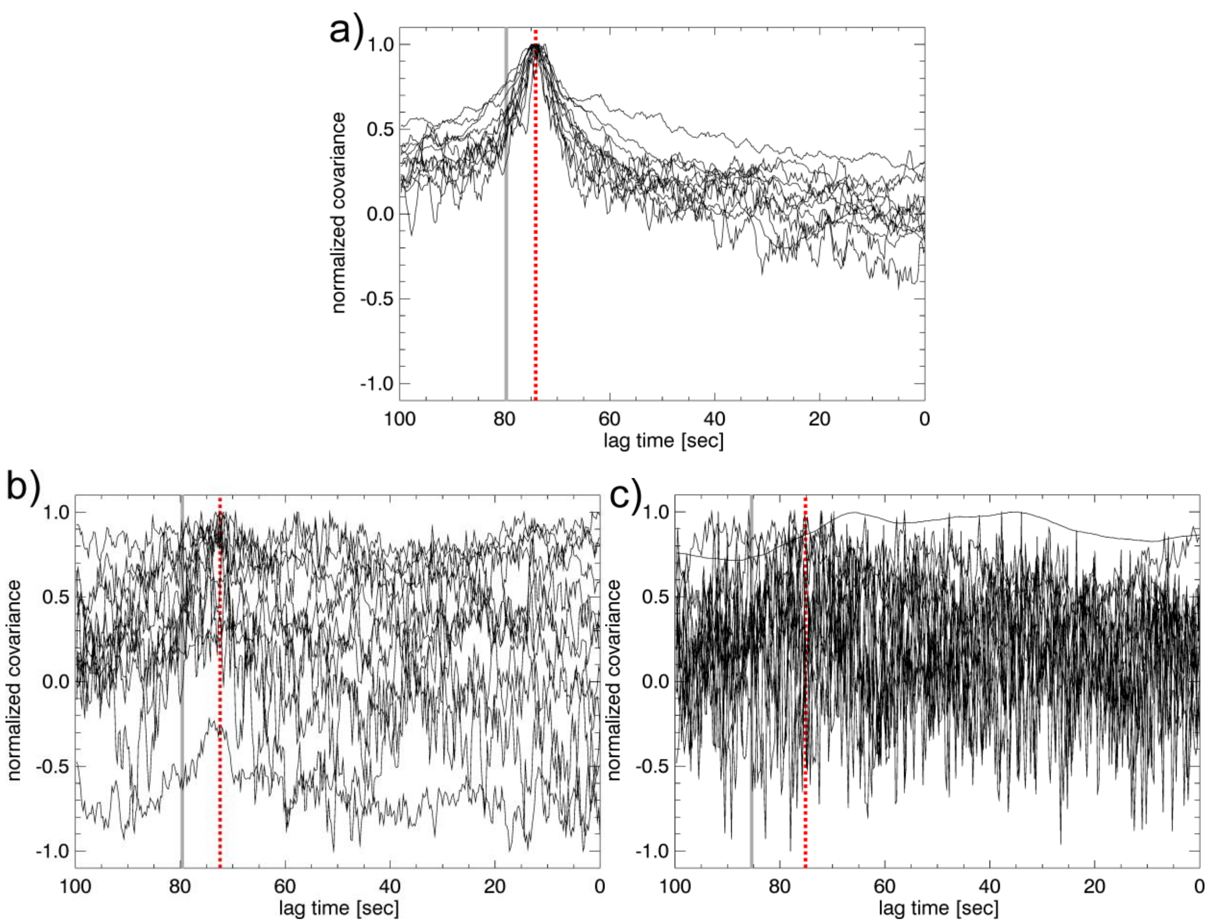

Figure R1 (Fig. S4 in Haszpra et al. (2018). Normalized lagged covariances for N_2O as functions of the lag time for three selected days. a) high flux case (15 July, 2016); b) low flux case (5 August, 2016); c) another low flux case (12 April, 2017). Vertical grey lines show the lag time for H_2O, while dotted red lines show the estimated lag time for N_2O.

Pressure changes and contamination of the filters cause variability in the lag time that is captured using the daily resolution. Fig. R2 shows the N_2O/CO lag time for the entire study period.

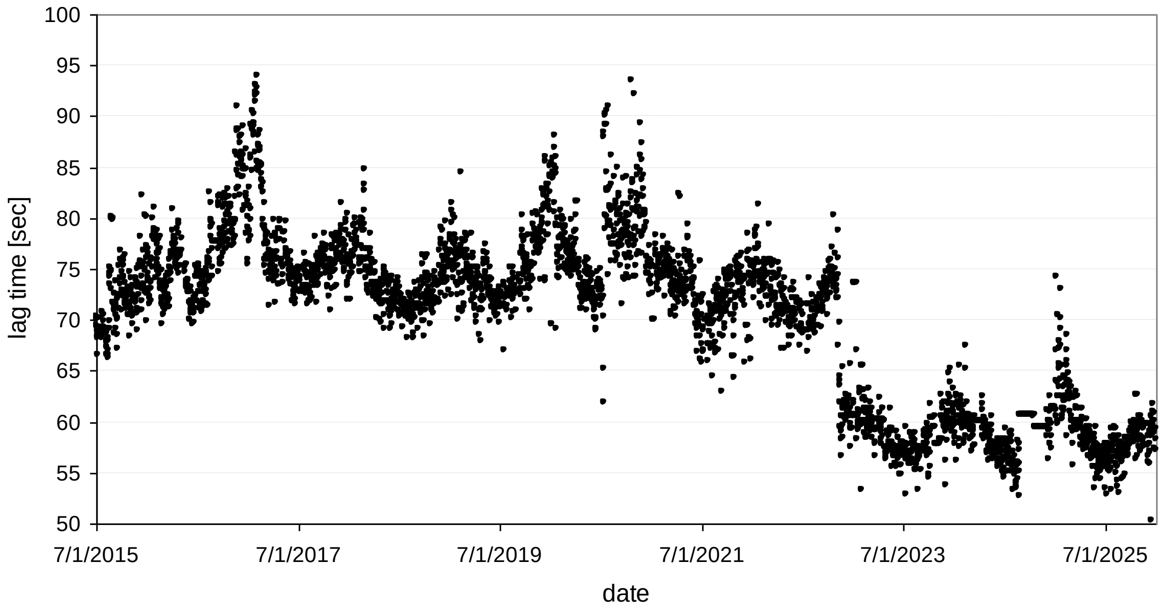

Figure R2. Daily lag time values (in seconds) during the entire study period.

The modified text in lines 191-199 of the revised manuscript:

"As a first step, the lag time caused by the ~100 m long air intake tube is determined. The H_2O lag time is used as an indicator for the identification of the CO lag time. It is typically 5-10 seconds longer than the lag time for CO due to the adsorption/desorption of water vapor on the inlet tube wall. For each day, the lag time is manually determined by visualizing the lagged covariance data for all 24 hours. This method was chosen because the covariance maximization would frequently lead to erroneous lag time values for days with low turbulent flux (cf. Supplementary material S2 in Haszpra et al. 2018). Assuming a constant lag time for each particular day is supported by the relatively constant performance of the pump and the visual inspection of the dataset over the 10-year-long measurement period. For low flux days, the lag time can be determined from the lag time from previous days with larger fluxes, and with careful visual selection of some of the lagged covariance curves that correctly indicate the true value (e.g. associated with occasional higher fluxes)."

Finally, in the revised manuscript, we divided the description of the monitoring site and the presentation of the instrumentation into two separate subsections (2.1 Site location; 2.2 Instrumentation), following the Referee's suggestion.

2. T**he PBL height from ERA5, input to the footprint model and nighttime filtering.** I think the authors should make a more thorough assessment of the uncertainties associated with the use of nighttime PBL height from ERA5. For stable boundary layer conditions, ERA5 has been shown to overestimate the PBL height (Sinclair et al 2021) and has also difficulties in the timing of the stable period (Sinclair et al 2021). The study cited is for Finland, but I wonder how would this be for the presented site in Hungary, perhaps worth checking. These aspects could influence the footprint calculation during nighttime and the nighttime filtering at low PBL height, defined by the authors at 100 m based on ERA5. I wonder how is the seasonality in CO fluxes (Fig 6, 7) affected by this filtering? Do you select more/less data points in winter/summer nights? The exclusion of hours with PBL height < 100 m systematically removes the most stable nocturnal conditions when ERA5 has more difficulties in resolving the nocturnal PBL. The finding that nighttime fluxes are not significantly different from zero may partly reflect this

filtering rather than the absence of nighttime exchange? A more comprehensive assessment on these things should be presented. For example, nighttime radiosondes from airports could be used to try to bias-correct the values from ERA5 or at least assess the magnitude of the PBL bias.

The closest radiosonde (RAOBS) stations are located in Zagreb (Croatia) and Graz (Austria), ~150 km from the tall tower. Other stations in Vienna (Austria), Budapest and Szeged (Hungary) are located even farther away (Fig. R3). Although Hegyhátsál is located in a relatively flat area, its flow pattern is quite complex due to its proximity to the Alps. Geographically, the station is situated within the belt of westerly winds, but due to the flow-modifying effect of the Alps, north-northeasterly and south-southwesterly winds prevail locally. Hence, inferring the evolution of the boundary layer height at Hegyhátsál from data at relatively distant radiosonde stations would require complex modeling work. As the ECMWF reanalysis products assimilate the RAOBS observations and provide a physically consistent framework for the PBL height calculation, it can be considered as a realization of the above-mentioned complex modelling framework, including the ERA5 reanalysis. It is known — and the Referee also refers to such a study — that the ERA5 PBL height data may be subject to biases, but we have no other approach to derive PBL height from the available radiosonde measurements that is more reliable than the estimates provided by this reanalysis for the tall-tower site. It is important to keep in mind that our calculations were restricted to time periods with u*>0.2 m/s, i.e. situations with mechanic turbulence during nighttime and a thicker stable layer. This criterion mitigates the issues mentioned by the Referee. After filtering, the number of available nighttime data points is significantly lower than that of daytime data points, especially in the summer (Figure 8, bottom right panel); however, this primarily affects the uncertainty of the calculated values and the significance level of the correlations, rather than the calculated fluxes themselves.

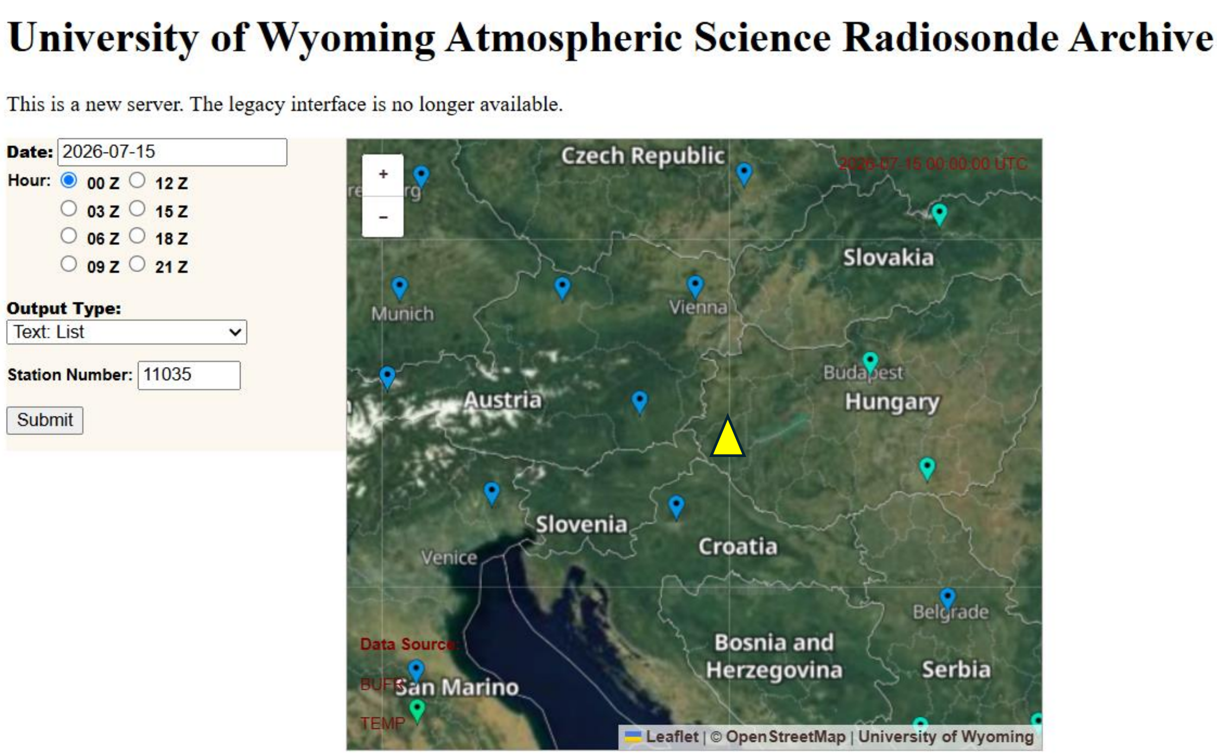

Figure R3: Radiosonde stations (map marker symbols) in the vicinity of the Hegyhátsál tall-tower monitoring station (yellow triangle). (https://weather.uwyo.edu/upperair/sounding.shtml)

3. **Storage flux omission.** The assumption that CO neither accumulates nor depletes in the surface layer during decoupling is physically reasonable and could be partly handled by the filtering of data points at PBLH<100m. However, the omission of the storage term deserves more attention, particularly given that at the same tower some of the co-authors explicitly included a CO2 storage term in the long-term CO_2 flux methodology (Barcza et al., 2020). The storage correction was deemed necessary for CO_2 at 82 m, yet it is omitted for CO without quantitative justification. An analysis of the potential magnitude of the storage term would substantially strengthen the paper. Two relatively straightforward approaches are available using existing data: 1) a single-point concentration-based storage estimate using the continuous CO concentration record at 82 m, following the approach discussed in Greco and Baldocchi (1996) and 2) an assessment of morning flush peaks in the hourly flux record, which would provide empirical evidence for or against nocturnal CO accumulation below the measurement level. If neither approach shows a significant storage term, that result itself would constitute a valuable justification for the omission.

We thank the Referee for this valuable suggestion. We have now quantified the below-sensor storage term of CO at 82 m using both suggested approaches. Neither reveals a storage flux large enough to bias the daily/seasonal CO budget, and we have now confirmed this statement by calculations rather than assuming it.

(1) We tested the single-point concentration-based storage flux calculation (Greco and Baldocchi, 1996). We first validated the single-point estimate $S = h \cdot c_{air} \cdot \partial\chi/\partial t$ against the site's operational CO_2 storage flux calculations (rate of change of storage; rcs; Haszpra et al., 2005; Barcza et al., 2020). The implemented method is identical in physical content to the single-level storage approximation of Greco and Baldocchi (1996), that is $S \approx h \cdot \partial\bar{c}/\partial t$; the only modification is the substitution $\bar{c} = \chi \cdot c_{air}$ (with $c_{air} = \rho/M_{air}$ the molar air density), because our 82 m analyzer records dry mixing ratio χ rather than a molar density - the two forms are algebraically equivalent. Using a centered time difference scheme, the single-point estimate reproduced the real CO_2 storage flux with values relatively well aligned with the 1:1 line ($R^2 = 0.51$, slope=1.03; Fig. R3) – i.e. the method is sound for CO_2. Clearly, applicability to CO_2 is not a direct proof for the applicability to CO, since surface CO_2 emission is continuous during the nighttime due to ecosystem respiration, which it is not the case for CO. Nevertheless, applied to CO, the single-point storage led to non-negligible storage fluxes for the hourly values (hourly median ≈ 0, inter-quartile range comparable to the turbulent flux itself), but its effect on the monthly mean diurnal variation curves is confined to the cold, high-emission associated, stably stratified months and is negligible in the growing season (Fig. R4). The daytime peak decreased in December (maximum change is from 15.8 to 11.9 nmol m^{-2} s^{-1}), February, and September–November, but remained unchanged from June to August.

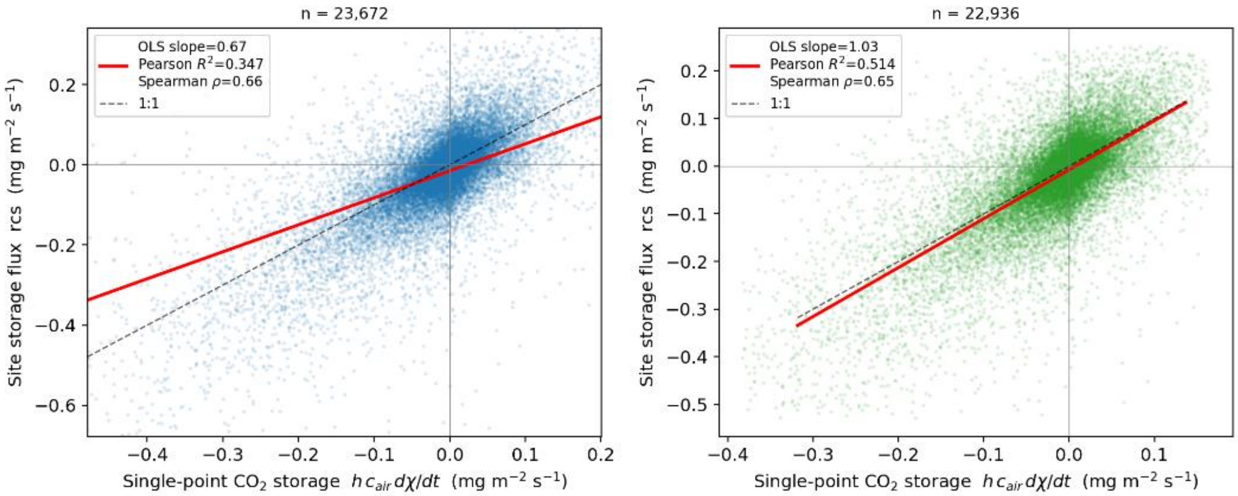

Figure R3. Single-point CO_2 storage vs. the operational storage flux calculated from the real CO_2 profile changes. Left: physical threshold + centered difference. Right: additional restrictions, +1–99 % truncation of data (R^2=0.51, slope 1.03 ≈ 1:1). OLS means ordinary least squares fit. n indicated the number of data pais.

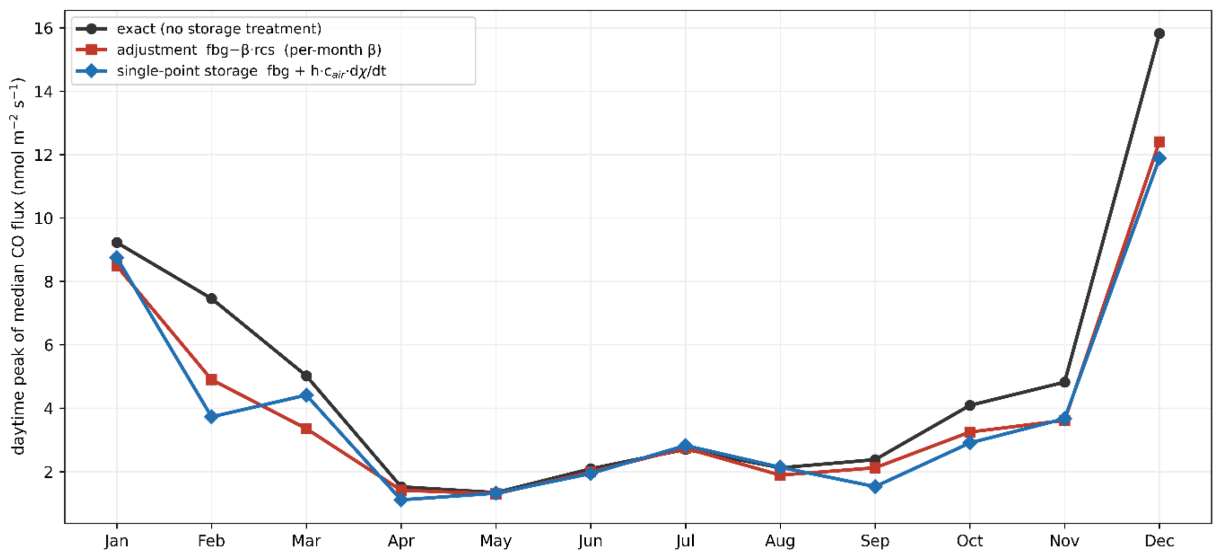

Figure. R4. Monthly variation of the CO peak flux under each storage treatment. The single-point storage (blue) tracks the β·rcs regression adjustment (red), cutting the peak in cold/high-emission months and leaving summer unchanged. 'exact' means the data in the original manuscript.

We also tested the applicability of another storage estimation method that we call β·rcs regression adjustment. For that purpose we use the co-located, simultaneously measured CO_2 storage flux (rcs) as a proxy for the below-sensor CO storage flux: for each month we regressed the background CO-flux anomaly (fbg minus its hourly median that was calculated as the mean diurnal CO flux estimation; Fig. 6 in the original manuscript) against the CO_2 storage flux with a robust Theil–Sen fitting method to account for outliers, leading to β as the slope, and removed that storage-linked component as fbg − β·rcs (hours with missing storage flux values are left unadjusted, so no data were discarded). β can be interpreted as the CO storage sensitivity in nmol per mg m^{-2} s^{-1} to CO_2 storage. Results show that it is near zero in summer and strongly negative in the cold, high-emission months, mirroring exactly the findings from the single-point CO storage flux estimations:

Month	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
β	−52	−46	−26	−4.1	−0.9	−0.4	−1.1	−1.5	−3.6	−12	−20	−48

The correction terms in the single-point estimation agree in sign and magnitude with an independent $\beta \cdot rcs$ regression adjustment described below, giving two mutually consistent estimates.

(2) Morning-flush assessment of the hourly flux record was also done. As it was clear from Fig. 6 in the original manuscript, the diurnal CO flux over the (quasi)natural area typically rises smoothly with the growing convective boundary layer and shows no anomalous isolated post-sunrise spike that would indicate the release of a large nocturnal CO accumulation below the sensor. The single-point storage term confirms this: the dominant feature is a modest evening accumulation ($S > 0$, 16–19 h, ≤ 1.4 nmol m^{-2} s^{-1}) as the nocturnal stable layer forms, flushing out until midday – a redistribution, not a sharp morning flush – and it stays below the magnitude of the turbulent flux in every hour. Two applied criteria (that is the precondition for the footprint calculation) further suppress any hidden storage: the $u^* > 0.2$ m s^{-1} friction-velocity threshold rejects the calm, stably stratified nights during which a below-sensor accumulation would form in the first place (violating the footprint calculation conditions), so the retained flux record is drawn from turbulent, well-mixed conditions; and the PBLH < 100 m rejection removes the remaining, most strongly stratified, decoupled situations.

Both approaches show the CO storage term at 82 m is real but modest, acting as an intraday redistribution. Figs. R5 and R6 show the mean diurnal variation of net CO exchange in the (quasi)natural grid cells for 2015-2025, using the same monthly aggregation as it was in the original manuscript (Fig. 6), but also showing the storage-corrected fluxes. Fig. R5 shows the single-point storage-corrected fluxes, while Fig. R6 presents the $\beta \cdot rcs$ regression-based adjustment. The figures demonstrate that the change in the hourly fluxes is small, and corroborate the above findings. Note that in the case of the single-point-based storage calculation peaks appeared in some months (e.g. June, July, and to some extent October) that do not seem to be physical but rather represent additional uncertainty due to the single-point storage calculation that has implicit uncertainty. Also, at those peaks the sample size is too low to get consistent fluxes.

It is also important to mention that in the study we calculate monthly means for the CO fluxes, where the intraday flux redistribution has no relevance; thus, the storage term can be omitted. Additionally, for the populated areas we do not present mean diurnal variations for the different months since the dataset is restricted and inevitably noisy. The storage flux omission affects only the mean diurnal curves from the quasi-natural areas, but the effect is small and restricted to the winter months with intensive domestic heating. Given the uncertainty, we think that it is best to omit the CO storage flux inclusion.

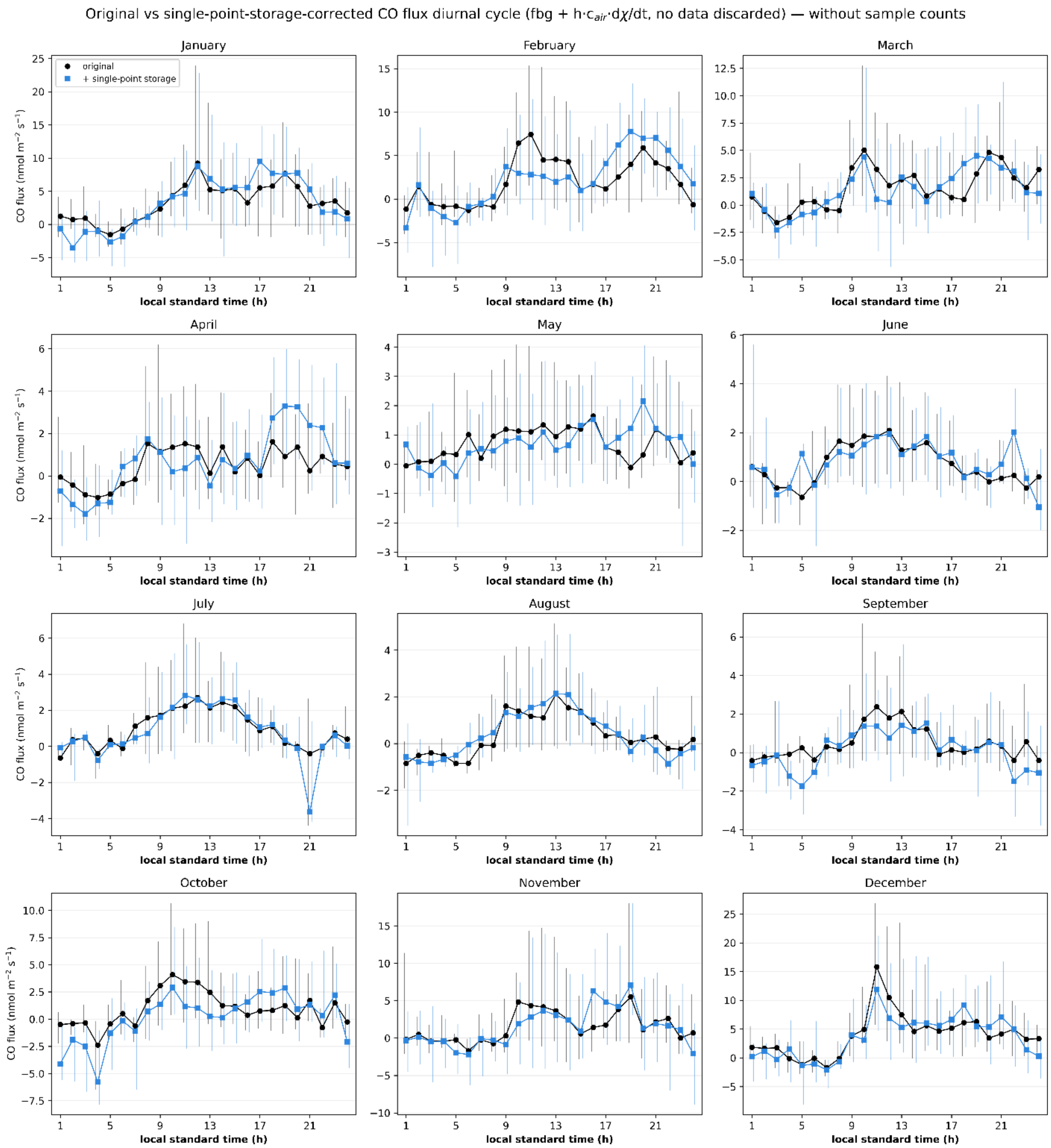

Figure R5. Same as Fig. 6 in the original manuscript (Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data. Whiskers indicate the lower and upper quartiles.) but the single-point storage-corrected CO fluxes are also indicated.

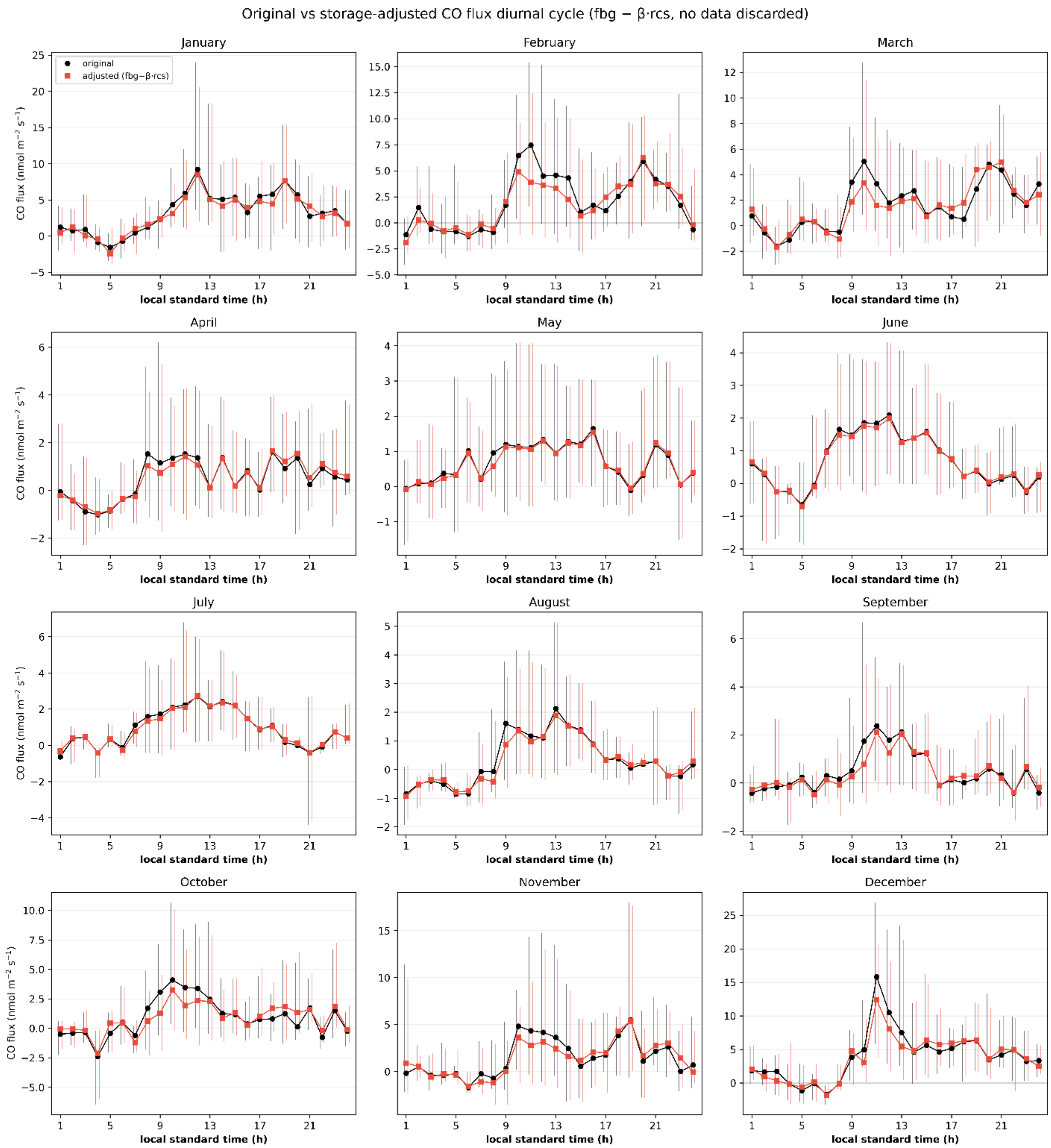

Figure R6. Same as Fig. 6 in the original manuscript (Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data. Whiskers indicate the lower and upper quartiles.) but the single point storage corrected CO fluxes are also indicated.

Considering the the actual focus of the study, we could only include a summary of our investigations in lines 217-236 of the manuscript:

"An EC system measures the mass transport through a horizontal plane at its elevation above the ground, which is 82 m in our case. In situations of limited vertical mixing, the EC system may become decoupled from the surface, leading to measurements that are no longer representative of the actual surface processes. To avoid such values, we

rejected fluxes when the boundary layer height was below 100 m. The layer below the EC system may become enriched (surface source) or depleted (surface sink) in the trace gas studied without recording. After the decoupling (i.e., during the breakup of the nighttime inversion after sunrise), the accumulated excess or deficiency in the surface layer appears in the measured flux (storage flux). Although the temporary decoupling may distort the measured diurnal variation, the daily total flux properly reflects the daily surface-atmosphere exchange (as there is no long-term accumulation or depletion below the measurement level; cf. Haszpra et al., 2005). Unlike in the case of CO_2, we hypothesize that emitted CO is neither taken up nor reemitted during the time of decoupling, i.e., there should not be any distortion of the net CO exchange estimations.
To test this hypothesis, we calculated the single-point concentration-based CO storage flux (assuming a constant vertical CO mole fraction profile up to the measurement level – Greco and Baldocchi, 1996). The result showed that the CO storage term at 82 meters has relevance for the CO fluxes only in the winter season, redistributing the hourly fluxes slightly, but not affecting the total flux. It is important to mention that the variability of the fluxes is high (see Results), and as the single- point-based storage flux calculation is only a simple approximation of the true storage flux (where the latter is supposed to take into account the non-linear shape change of the CO profile as well), consideration of the storage flux increases the uncertainty. As the analysis is restricted to turbulent conditions ($u^* \geq 0.2$ m s^{-1}, PBLH ≥ 100 m – see Section 2.5), to ensure that the surface fluxes are identical with the measured CO flux measured at 82 m elevation, the storage-prone hours are largely excluded beforehand. Thus, it seems reasonable to accept the aforementioned hypothesis on the negligible effect of CO storage flux on the results."

Specific Comments:

L68: I suggest to rephrase:

"was overcompensated by the mainly radiation-driven emission"

To

"was offset by emissions mainly driven by radiation"

Accepted and corrected.

L71-72: What is meant by: "seasonally variable net flux direction"? Do you mean a seasonally variable net sink or source? Perhaps it is worth rephrasing this.

Corrected.

L116-117: It is not clear if these air intake tubes air for CO. I realize that they aren't afterwards because the authors are not calculating storage fluxes. It is not clear for what are these air intakes then, CO2 profiling system? If so, why adding this here? I think is rather confusing.

Carbon monoxide measurements are taken at a single level of the tower, at 82 m. The other measurement levels and the instruments located there are primarily used for research related to carbon dioxide (Haszpra et al., 2015; Haszpra, 2024). Since these measurements are irrelevant to the present work, we have now removed them from the revised manuscript. This is also in line with suggestions from the other Referee.

L278-284: This reads more like Methods. The authors should consider adding this to the Methods section.

This part of the paper has been moved to the '2. Measurement and methods' section, specifically to the '2.7 Flux map, and separation of populated and (quasi)natural areas' subsection.

L311-322: Also reads like methods. I also recommend to add a scheme of this back-and-forth and possibly circular methodology.

This part of the paper has also been moved to the '2. Measurement and methods' section, specifically to the '2.7 Flux map, and separation of populated and (quasi)natural areas' subsection. The original intention was to write the study in a narrative style, in which the methods and results follow one another in chronological order as they build upon each other, without being strictly separated from one another. Following the Referees' suggestion, we have restructured the manuscript to follow the more conventional "methods, results, discussion" format.

L402-404: Given this possibly reason (biomass burning), did the authors check fire counts? A quick check on the VIIRS fire counts could strengthen this argument.

Western Hungary is not a fire-prone area; forest fires are rare, and open burning is prohibited in agricultural areas. We analyzed remote sensing-based, fire-related observations to assess whether biomass burning could have contributed to the measured CO fluxes. Specifically, we examined two independent MODIS products, as they are the longest available satellite-based archive (longer than the suggested VIIRS products, which are available only from 2012). We analyzed the MCD64A1 Burned Area official NASA product (500 m spatial resolution), which identifies areas affected by fire after the event, and the MOD14 Active Fire product (1 km spatial resolution), which detects active thermal anomalies at the time of the satellite overpass. Since carbon monoxide measurements began in 2015, the closest fire event revealed by satellites was more than 20 km from the monitoring station, and even that was small in scale. The last fire detected within the monitoring station's footprint area occurred in 2003. We inserted a sentence in the manuscript stating that forest fires and open burning in agricultural areas should not be considered as sources of carbon monoxide due to regional characteristics and legal prohibitions (lines 341-344):

"Western Hungary, where the monitoring site is located, is not a fire-prone area; forest fires are rare, and open burning is prohibited in agricultural areas. Since CO measurements began in 2015, the closest fire event revealed by satellites (MODIS MCD64A1 Burned Area and MOD14 Active Fire products) was more than 20 km from the monitoring station, and even that was small in scale."

L431: With "The longer-term EC measurements recorded diurnal CO flux variations similar to our measurements" did you mean previous studies? Rephrasing needed to better introduce this paragraph.

We mean the previous, longer-term EC measurements over vegetated areas, which are discussed in the paragraph. Now (line 492):

"Earlier longer-term EC measurements recorded diurnal CO flux variations similar to our measurements."

References

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