Authors' response to Referee#1

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.

Comments to the Author

The manuscript by Haszpra et al. presents a 10-year time series of carbon monoxide (CO) fluxes measured using the eddy covariance (EC) from a tall tower in Hungary. The study presents CO fluxes separately from populated areas and vegetation-dominated quasi-natural regions. The results indicate that CO emissions from populated areas are substantially higher than emissions from quasi-natural areas. Authors suggest that also quasi-natural areas are a small but statistically significant CO source. The manuscript also suggests that the CO emissions measured from populated areas significantly exceed the current estimates reported in the EDGAR inventory.

The long-term dataset of CO fluxes is valuable, and the topic is relevant for improving our understanding of regional CO budgets in central Europe. However, I have several concerns regarding flux processing, data quality control, and the approach used to separate fluxes from populated and quasi-natural areas. Addressing these concerns is needed before the manuscript can be considered for publication in ACP.

Thank you for the positive evaluation. We have done our best to address your concerns and improve the presentation of the results of our study.

Major comments:

1. Flux processing (Section 2.3)

One of my concerns relates to the validity of the EC flux processing and several methodological choices made during data processing. The description of the processing steps is currently unclear. I encourage the authors to clearly describe the full flux calculation procedure, starting from the raw 5 Hz data to the final hourly fluxes, and revise Section 2.3 accordingly.

Thank you very much for your comments, which have helped make our work clearer and easier to follow, and have strengthened the credibility of the findings presented in the manuscript. Section 2.4 of the revised manuscript (the chapter numbering has changed in the revised manuscript!) now presents the eddy covariance data processing workflow in detail. We provide ample information to make the workflow reproducible and transparent. Originally, we planned to provide only a brief description since the methodology has been published in our previous works. However, we now 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 methodology chapter and the chapters presenting the results/discussions, since the novelty of the study primarily lies in the results and conclusions.

In particular, the authors should explicitly address the following points:

- From the text (Lines 185–186), it is not clear which method is used to determine the time lag between the sonic anemometer and the gas analyzer. Please clarify the method used (e.g. covariance maximization or something else) and specify at which stage of the processing the time lag is calculated (before or after coordinate rotation). Please also clarify what is meant by "interactive visualization of lagged covariance data".

The methodology applied for the calculation of the hourly turbulent fluxes of CO is identical to that used for N_2O flux calculation at this site and is described in detail in Haszpra et al. (2018). In the revised manuscript, we describe how the time lag is calculated. Rather than copying the description available in the Supplementary material of Haszpra et al. (2018), we have reformulated it slightly for the main text of the present paper. Figure R1a-c, which are replotted from the aforementioned paper, demonstrate that the daily, interactive lag time calculation is the supported method.

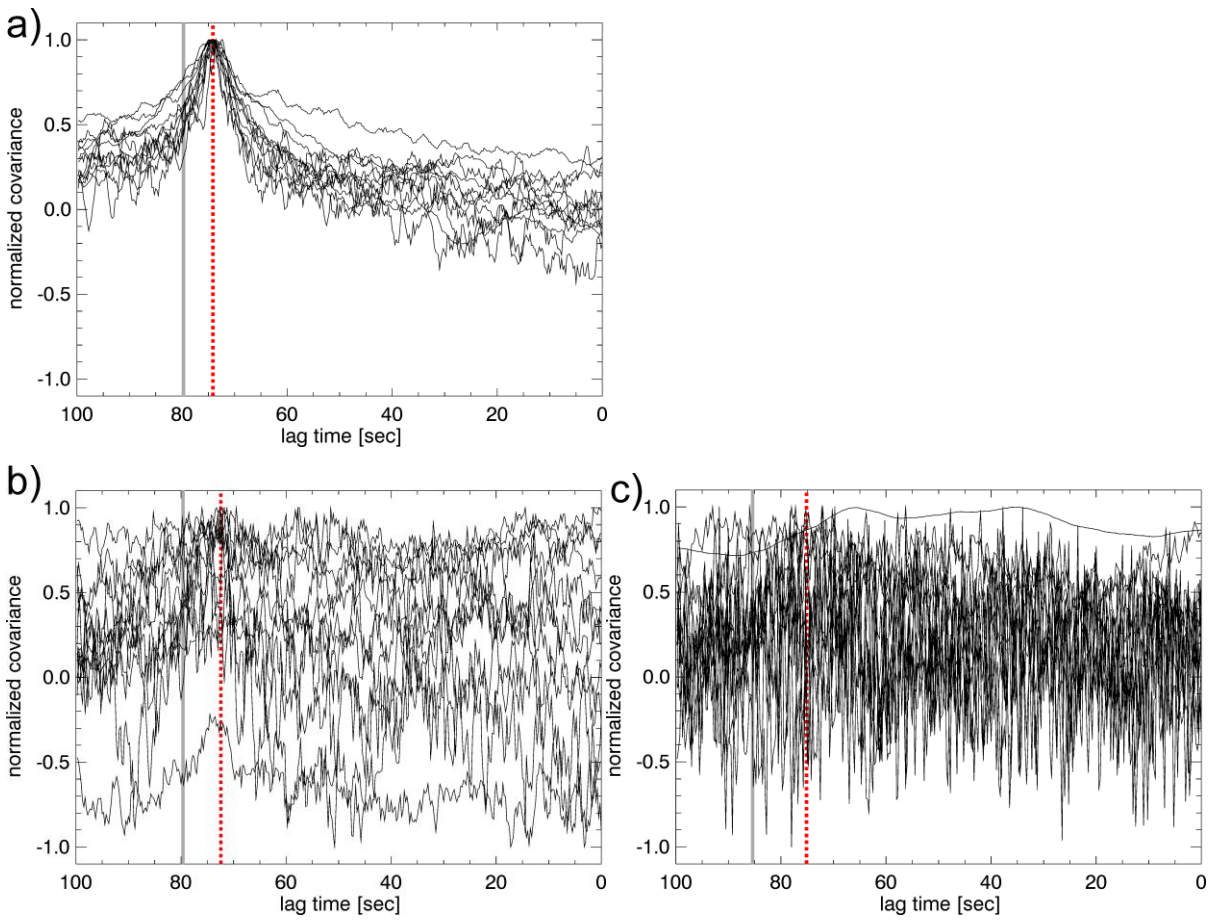

Figure R1 (Fig. S4 in Haszpra et al., 2018). Normalized lagged covariances for N_2O as functions of the time lag for three selected days. a) high flux case (15 July, 2016); b) low flux case (5 August, 2016); c) another low flux case (12 April, 2017). Vertical grey lines show the time lag for H_2O, while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

The modified text in the revised manuscript, lines 191- 199, reads as follows:

"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)."

- The manuscript suggests that the time lag is calculated on a daily basis rather than for each flux averaging period. This is potentially problematic, as the time lag is likely to vary throughout the day due to changes in temperature, pressure, and flow conditions. Could the authors justify this choice? Why not determine the time lag separately for each averaging period (e.g. hourly) by maximizing the covariance, which is common practice in EC flux processing?

See the previous answer.

- I also have concerns regarding the choice of coordinate rotation (Line 187). The use of 3-D wind rotation is not well justified, as the determination of the lateral component (v′w′) is often uncertain and can introduce additional noise into the flux estimates (e.g., Finnigan, 2004). Why not use 2-D rotation or planar fit?

As for the 3D wind rotation, we also extended the text to provide a rationale for the method. Figure R2 (replotted from Barcza (2001)) shows the essence of the method: the wind direction-dependent estimation of the wind vector deflection that was derived using the methodology provided by Lee (1998) and long-term measurement results.

The modified text in the revised manuscript (lines 200- 210) reads as follows:

"Angle-of-attack dependent calibration is applied to the raw sonic anemometer data (van der Molen et al., 2004). Since the EC system is located at a height of 82 m, the slight tilt of the sonic anemometer, the tower body structure, and the surrounding terrain can affect the mean airflow that is sampled by the anemometer. We hence chose the three-dimensional wind vector rotation to align the coordinate system with the prevailing streamlines (Lee, 1998). This method performed better for our site than the popular planar fit method and the simple 2D rotation (Aubinet et al., 2012). Using the technique detailed in Barcza (2001), rotation coefficients are calculated for each 3º wind direction sector using data from long time periods. The length of the time periods is determined by the timing of interventions when the tilt of the anemometer might have changed due to maintenance work. These coefficients are used to determine the rotation angle of the 3D wind for a given wind direction (see Fig. 2.12 in Barcza, 2001), which means that the rotated coordinate system matches the mean streamlines (but allows non-zero mean vertical wind speed – Lee, 1998). Linear detrending is applied to the wind and CO mole fraction time series."

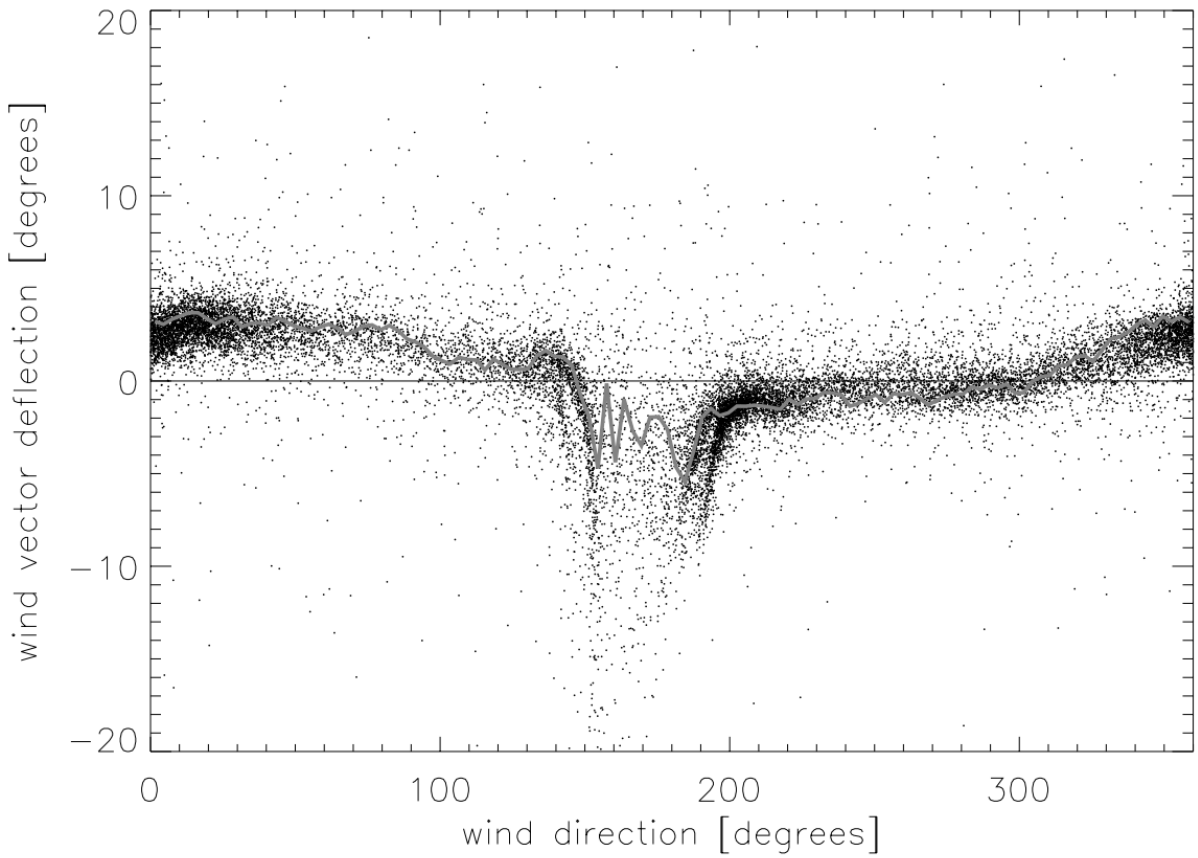

Figure R2. The figure demonstrates the wind vector rotation procedure (Fig. 2.12 in (Barcza, 2001)).

2. Data filtering and quality control (Section 2.5)

The description of data filtering and quality control procedures requires further clarification. While I understand the authors' choice not to remove outliers (Line 205), some form of quality control of turbulent conditions is essential in eddy covariance analysis. Please clarify what types of quality control procedures, if any, have been applied to the data. For example, were fluxes filtered based on stationarity or other turbulence criteria?

During the eddy covariance data processing workflow, we calculate several quality control (QC) flags. Different flags are calculated for CO_2, N_2O, and CO fluxes. Some flags are only calculated for certain components.
According to the authors' experience (note that CO_2 flux measurements have been performed since 1997 – Barcza et al. (2020)), the application of the QC flags in the data analysis is not always the best choice. We think that the flags do not provide enough information for interpretation and further application (e.g. aggregation, gap filling, flux partitioning). The flags are not indicative of the outliers either, as outliers are typically associated with non-informative quality flags. In contrast, most of the fluxes associated with questionable quality fit well with the other fluxes. Specifically, for our study, the issue with the flags might arise from the fact that the eddy covariance system is installed at 82 m height, where larger eddy sizes dominate. Other error sources are also present (i.e., flow distortion by the tower body, advection, convection, etc.).

Hence, we think that in this case, a visual quality control to filter ecologically unlikely fluxes for CO_2 outperforms standard QC flag filtering.
Nevertheless, we tested the applicability of the QC flags to see if there was any detectable relationship between the outliers of CO flux and the flags themselves.
Within quality control, uncertainty estimation was done according to Kroon et al. (2010), who proposed that the majority of the random flux error is caused by the one-point sampling of turbulence. We approximated the uncertainty of the hourly flux values using Eq. (16) in Kroon et al. (2010), which estimates one-point sampling uncertainty. We also used three quality control indicators focusing on the non-stationarity of the horizontal wind (Vickers and Mahrt, 1997). Namely, alongwind relative non-stationarity, crosswind relative non-stationarity, and vector wind relative non-stationarity were calculated based on the raw eddy covariance data. Those four quantities were plotted against the observed CO flux to detect any relationships that could be used to filter data (Fig. R3).

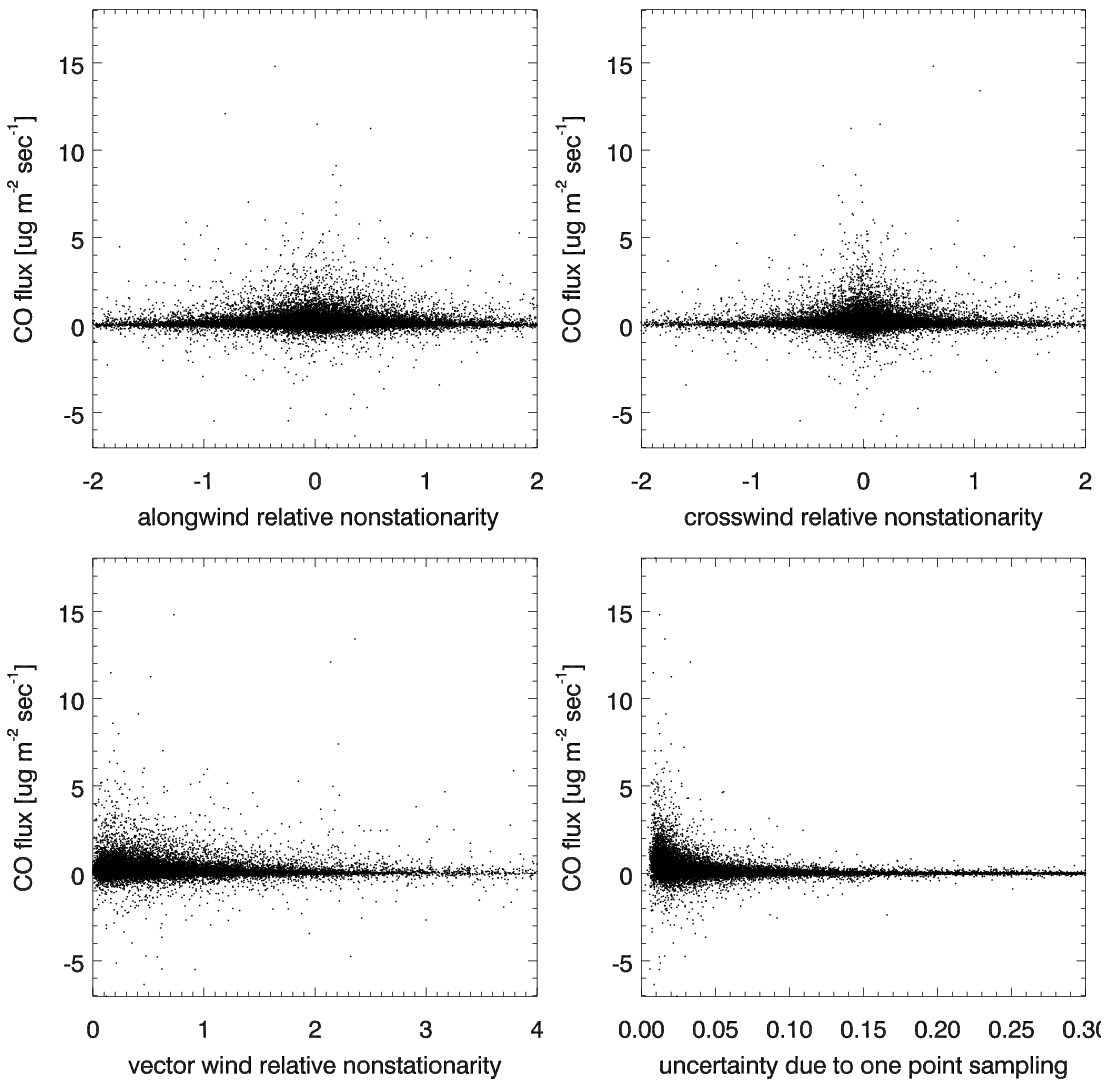

Figure R3. Relationship between the quality indicators and the CO flux for the entire observation period. As can be seen, there is no detectable relationship between the outliers and the larger values of the quality indicators (typically indicating questionable data).

As we did not find any evidence showing that the flux quality indicators could be used for deriving data filtering thresholds or criteria, we decided not to apply them. At our measurement height, random errors are clearly the most important factors affecting the flux. The revised manuscript has been supplemented by the following text (lines 237-245):

"Considering quality control and data flagging based on the calculated quality indicators (Vickers and Mahrt, 1997; Kroon et al., 2010), an attempt was made to check their applicability to filter outliers. However, as there was no detectable relationship between the magnitude of the CO fluxes and the indicators, this filtering was not applied. Another approach would have been to filter the data based on statistical measures (e.g. removing data outside the +/- 3 standard deviation threshold or using percentile-based filtering, etc.). We rejected this approach because we expect significantly different fluxes from the different land cover types (arable lands, forests, villages, etc.) in the heterogeneous landscape surrounding the tower. Hence, no outlier filtering was applied during the flux calculation. It is important to note that the prerequisites for the footprint calculations inherently filter out low wind speed conditions (i.e. strong inversion during nighttime), which represents an alternative quality control."

In addition, it is unclear whether any wind direction filtering has been applied. Are there wind sectors affected by tower structure or platform disturbances, and if so, were these sectors excluded from the analysis? This is not mentioned in the manuscript.

The ultrasonic anemometer is mounted at the end of a 4.4 m long instrument arm at 82 m above the ground, where the diameter of the steel-tube tower is 1.8 m. As Fig. R2 shows, the tower structure affects the flow around the tower, causing a shadow effect. Despite the wind flow modification, wind direction filtering is not performed. We developed a wind direction correction method based on a short-tower (3 m) eddy covariance system installed in the immediate surroundings of the tall tower (cf. Barcza et al., 2003). The wind direction measured at 82 m height is empirically adjusted based on the wind direction climatology at 3 m height, taking into account the changes in wind direction with height. This correction enables the correct localization of the footprint functions (see Barcza et al., 2009). We now mention this correction in the revised manuscript (lines 255-259):

"As wind direction is affected by the tower body (see Barcza, 2001), we developed a wind direction correction method based on the co-located short-tower (3 m) EC system documented in Barcza et al. (2003). The wind direction measured at the height of 82 m is empirically adjusted based on the wind direction climatology of the short tower (3 m), taking into account the changes in wind direction with height. This method was successfully used in earlier studies (Barcza et al., 2009; Gelybó et al., 2013) focusing on footprint analysis based on the tower data."

3. Derivation of populated and quasi-natural fluxes

One of my major concerns relates to the methodology used to separate fluxes from populated and quasi-natural areas, as several key results rely on this approach. The method appears to assume constant fluxes for all populated grid cells and, similarly, constant fluxes for all quasi-natural areas. This is a strong assumption that requires further justification. Could the authors provide additional support for this assumption? For example, if the flux from populated areas is assumed to be spatially uniform, one might expect a linear relationship between the parameter alpha and the measured flux f (as introduced in Equation 1). Demonstrating such a relationship would help evaluate the validity of the approach.

In our model, we assumed that emissions are spatially homogeneous both within the populated areas and in the vegetation-covered, quasi-natural areas. This seems a bold assumption, but despite the long time series, there was not sufficient data to account for spatial differences. Most

of the quasi-natural areas are small plots, where owners may change the crop type on a yearly basis, making it essentially impossible to break down emissions into further categories. In populated areas, residential heating is the major source of CO emissions. Natural gas is available in all settlements surrounding the tower, but there is no information available on whether there are significant local differences in the use of natural gas and biomass - or in the burning of waste, if any.

Nevertheless, we examined the relationship between the proportion of built-up areas and the calculated flux, regardless of direction, as suggested by the Referee. Indeed, if emissions from populated areas are spatially homogeneous, then there should be a linear relationship between the proportion of the area and the measured flux. Based on the data shown in Fig. R4, there is a statistically highly significant linear relationship ($p<0.001$) during both winter and summer. We hence conclude that emissions from populated areas may be approximately spatially homogeneous.

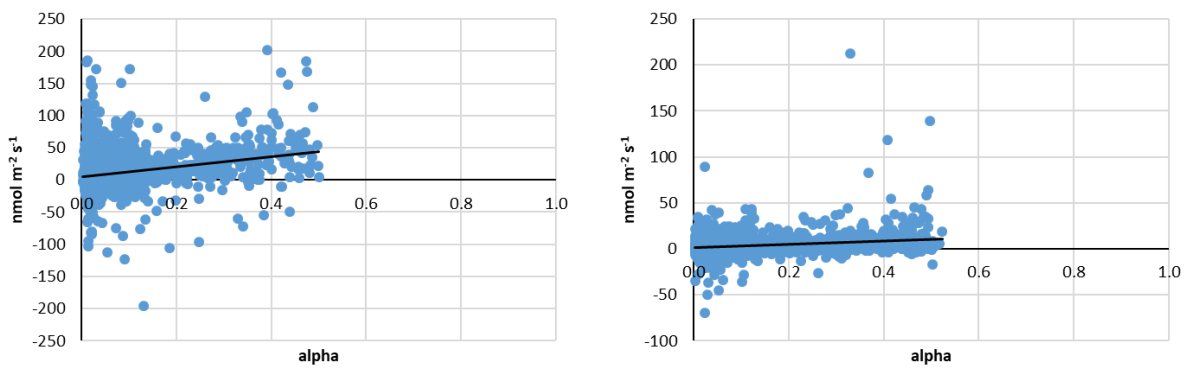

Figure R4. The measured CO flux as a function of the share of the populated area within the footprint in winter (Dec-Feb, N=4890, R^2=0.075, left panel) and summer (Jun-Aug, N=7492, R^2=0.067, right panel)

In theory, if we were to assume a spatially homogeneous flux either for populated areas or for vegetation-covered areas, we could infer spatial inhomogeneity within the other land-use category; however, we think that this approach would be too speculative for a scientific publication.

A second concern relates to the derivation of quasi-natural fluxes. It is not entirely clear how the fluxes shown in Figures 6 and 7 are calculated. Do these figures include all flux data, or only data filtered by a threshold (alpha <2.5%)? Please clarify this in the figure captions and in the main text. Furthermore, the terminology is somewhat inconsistent. In the Abstract, the authors refer to a "vegetation-dominated sector." It is unclear whether this corresponds directly to the quasi-natural grid cells used elsewhere in the manuscript. Please ensure consistent terminology and clearly define these terms throughout the manuscript.

Both Figures 6 and 7 in the manuscript show emissions from (quasi)natural grid cells based on the data obtained from the iteration procedure described in the text. We have modified the figure captions and the related text of the paper to make this clearer to the reader:

"Figure 6: Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data (α < 2.5 %). Whiskers indicate the lower and upper quartiles, while the symbols give the probability level of the deviation of the median from zero. Note the different y-axes."

"Figure 7: Seasonal variation of the monthly mean CO flux from the (quasi)natural grid cells (α < 2.5 %)."

For stylistic reasons, we used the terms “vegetation-dominated” and “(quasi)natural” interchangeably in the study. We reviewed the entire manuscript and made changes wherever it was unclear or misleading.

The authors themselves note that quasi-natural fluxes are likely affected by anthropogenic emissions, which complicates their interpretation. Given this limitation, further analysis would be helpful. I suggest including an analysis of quasi-natural fluxes as a function of wind direction. Such a plot would help assess whether fluxes are consistent across directions or whether certain sectors are more strongly influenced by anthropogenic sources. This is particularly important because the manuscript presents relatively detailed interpretations of quasi-natural fluxes and attributes them to specific drivers. In order to support such conclusions, it would be important to better isolate (or at least quantify) the influence of anthropogenic CO sources.

As shown in Figure 3 in the original/revised manuscript, anthropogenic influence is significant across a wide sector, while the amount of available data from areas predominantly covered by vegetation varies greatly due to the prevailing wind directions. Therefore, a wind sector attribution of the fluxes may result in misinterpretation of the fluxes. Instead, we chose a different method to assess the spatial homogeneity of emissions from (quasi-)natural grid cells.
Using Equation (2) and the monthly average emission data for populated areas, we estimated the flux proportion originating from (quasi)natural grid cells for each measured flux value. This is, of course, only an approximation, since we only have data on emissions from populated areas at a monthly temporal resolution, and we suppose uniform spatial distribution. From the resulting “virtual” (quasi)natural fluxes, we created footprint-weighted maps (Fig. R5) similar to Figure 3 in the manuscript, which thus should exclude anthropogenic fluxes. For this reason, the maps do not contain any data for populated cells (grey pixels = no data).

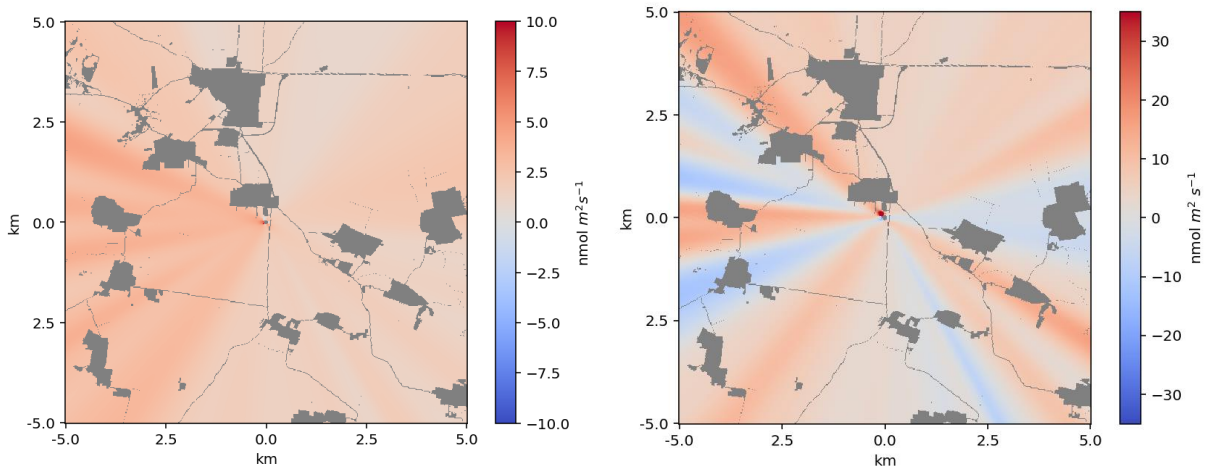

Figure R5. Footprint-weighted flux maps for the summer (Jun–Aug, 2015-2025, left panel) and winter months (Dec-Feb, 2015-2025, right panel) based exclusively on the emissions over the (quasi)natural grid cells. The maps are based on cases in which the footprint contribution over a radius of 5 km from the tower is at least 80 % to the measured fluxes. The tower is at the origin (0, 0). Note the different scales of the panels.

During summer, the spatial distribution of natural flux is fairly homogeneous. Slightly higher fluxes are only detected from the southwest-westerly sector. This relatively slight excess is not linked to the location of settlements (e.g., no such effect is observed in the northern direction) or anthropogenic emissions. It may be due to a higher proportion of forests in that direction, but this cannot be substantiated with the available data. As discussed in the manuscript, the natural

emissions calculated for the winter months are distorted by anthropogenic emissions that are two orders of magnitude greater; it is not possible to completely filter out the latter using such a simple model. Accordingly, the winter footprint-weighted map also shows a more heterogeneous picture than that of the summer. Since no systematic directional pattern was evident, the observed heterogeneity can likely be attributed to uncertainty in the estimated anthropogenic emissions. Even a small relative uncertainty in a large anthropogenic flux can propagate into a substantially larger relative uncertainty in the inferred natural emissions, as these are considerably smaller in magnitude. With the caveats mentioned in the manuscript regarding the winter period, we consider the approximate spatial homogeneity of natural emissions to be an acceptable assumption in our simple model. Since we do not observe any signs of the influence of anthropogenic emissions during the summer months, we can assume that our data on natural emissions during the summer correspond to the actual values.

Another important issue relates to the treatment of diurnal variability in anthropogenic emissions. As stated in Line 335, the derivation of quasi-natural fluxes does not account for the diurnal variation of anthropogenic emissions. The manuscript argues that this limitation does not significantly affect the diurnal shape of the derived fluxes. However, this assumption is not convincing. If anthropogenic emissions are lower at night than during the day, applying a constant correction would likely lead to an overcorrection during nighttime, resulting in an underestimation of the quasi-natural flux. This raises the question of whether the diurnal variation shown in Figure 6 could partly reflect biases introduced by this assumption rather than real ecosystem processes. Could the authors evaluate the potential magnitude of this bias?

This point is entirely valid. The diurnal variation in anthropogenic emissions can influence the calculated daily pattern of natural emissions. Unfortunately, there are no data on the diurnal variation in anthropogenic emissions; at best, we can only make assumptions. During the summer (non-heating) months, on which the manuscript primarily focuses, the effect may be relatively small, since, on the one hand, the anthropogenic influence on the calculated natural fluxes is low to begin with, and, on the other hand, the difference between daytime and nighttime values for the relatively low emissions from transportation is not significant. The situation is different during the winter months, when residential heating is the dominant source of CO. Here, it is not so much the day-night difference but rather the intraday variations that may be of interest (e.g., the evening heating of residential buildings). Unfortunately, no data are available on this either. Our paper primarily focuses on wintertime anthropogenic and summertime natural CO emissions. In our assessment, our summer data approximately correspond to the actual situation, while the influence of anthropogenic emissions on the calculated fluxes over the (quasi)natural grid cells is mentioned in the manuscript.

Lines 112-113:

"We focused primarily on analyzing anthropogenic surface-atmosphere CO fluxes during winter and natural CO fluxes during summer."

Lines 481-488:

"The area influencing our measurements performed at 82 m elevation can be about two orders of magnitude larger, and may cover different distant anthropogenic sources. This is true even when the "clean air" sector is carefully selected and constrained to footprints for which the contribution of the distant sources (outside of the 5 km radius of the tower) to the flux measured at the tower is less than 20 %. The wintertime emission in the populated grid cells can be two to three orders of magnitude larger than in the (quasi)natural grid cells. Even if the weighted footprint contribution from a distant grid cell is minor, the potentially huge emission from that cell may substantially influence the measured fluxes. This effect cannot be completely eliminated with our rather simple correction algorithm."

Finally, regarding the interpretation of soil water content (SWC) effects (Lines 366–369): It is not clear that the current approach is well suited to investigate SWC effects. Given that the analysis relies on tall-tower measurements and assumes spatially uniform fluxes across different land-use types, it is difficult to attribute observed signals to SWC with confidence. Previous studies have shown that soil CO uptake can vary between land-use types (e.g. agricultural vs forest ecosystems), which complicates such interpretations.

The spatial resolution of the ERA5 database is 0.25x0.25 degrees. For this study, we used data from the two grid points closest to the tower. According to the literature, natural flux may depend on soil moisture; however, the variability in soil moisture and soil structure, as well as the vegetation type within a single ERA5 pixel (approximately 28×20 km², larger than the whole domain studied in this work), can be so great that it cannot be analyzed using our method. Therefore, in our study, we could only conclude that it is not possible to derive a SWC effect based on the data used. We emphasized this fact in the revised manuscript to avoid any misinterpretation. The dependence of the flux on soil moisture content can only be investigated using shorter EC towers with a smaller footprint and local in situ soil moisture measurements within the footprint area. Lines 429-434:

"However, in our study, the correlation between CO flux and local soil water content (SWC) in the top 0-7 cm layer is not statistically significant (Fig. 8). This suggests that SWC may not be an important driver, at least with the given soil/vegetation characteristics. It should be emphasized, however, that we used data from the ERA5 database with a resolution of 0.25×0.25 degrees for the calculation, while there may be very significant variations in soil moisture content, soil types, and structure within an area of this size. Therefore, the fact that we do not observe a correlation between flux and soil moisture content in the present study does not mean that such a correlation cannot exist."

4. Manuscript structure

I suggest that the authors revise the manuscript structure to improve clarity and better focus on the most relevant aspects of the study. At present, the Introduction lacks a clear statement of the research objectives. The Methods section includes information (e.g. CO_2 flux and concentration measurements) that is not directly used in the analysis and does not appear essential for the reader.

Thank you for the comments, which help improve the manuscript and make it easier to follow. In the revised manuscript, we specify the research objective in the Introduction. The reference to the CO_2 measurements not relevant to this study has been removed from the revised manuscript.

In addition, some methodological details are presented in the Results section and would be more appropriately placed in the Methods, while certain results are described in the Discussion. There is also some repetition between the Introduction and Discussion; these sections could be more carefully structured to reduce redundancy and improve the overall flow of the manuscript.

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 Referee's suggestion, we have restructured the manuscript to follow the more conventional "methods, results, discussion" format. The major change was moving the model descriptions from Results (Section 3.2 and 3.3) to Measurements and methods (Section 2.6). The numerical results of our measurements have been entirely transferred from the Discussion into the Results section. In our opinion, the comparison with the

EDGAR database is more appropriate for the “Discussion” section than for the “Results” section; therefore, we have left the results of this analysis in their original location (Discussion).

Furthermore, the Results section would benefit from a more quantitative presentation of the findings. In several cases, including numerical values would improve clarity and strengthen the conclusions.

Thank you for your suggestion. We now present numerical values of all main findings in addition to the previously presented figures.

Finally, while the Abstract emphasizes quasi-natural fluxes, I recommend placing greater emphasis on emissions from populated areas, which appear to be one of the key findings of the study.

The paper focuses on both anthropogenic and (quasi)natural CO emissions. The previously unrecognized underestimation of anthropogenic emissions — due to the role of carbon monoxide in the atmosphere — is indeed a more significant finding than the presentation of data on (quasi)natural emissions, which do not differ significantly from the results of other studies. Therefore, we have moved the data on populated areas to the forefront of the Abstract, highlighting some numerical data, and placed greater emphasis on them than in the original version of the manuscript. However, we had to take into account that the length of the Abstract cannot exceed 250 words.

“**Abstract.** The atmospheric trace gas carbon monoxide (CO) significantly influences the atmosphere's oxidative capacity and indirectly affects climate. However, the net land-atmosphere exchange of CO remains highly uncertain due to the scarcity of long-term monitoring. This study presents and analyses a 10-year-long CO flux data series from a tall-tower eddy covariance (EC) system operated in a rural Central European region. The measured fluxes were evaluated separately for vegetation-covered, (quasi)natural areas and for populated areas. The estimated monthly mean emissions from the populated areas varied between 12 and 110 nmol m^2 s^1, with the lowest value observed in July and the highest in January, respectively. The measured wintertime emissions from the populated areas significantly exceed the activity-based estimations, presumably due to underestimated emissions factors for residential heating. The vegetation-dominated sector (agricultural fields and forests) acted as a weak net CO source from April to September with an average emission of 0.58 nmol m^2 s^1 (monthly range 0.31–0.91 nmol m^2 s^1), with the highest value occurring in July. The diurnal peaks of the median hourly emissions can be observed around noon, and they range from 1.59 to 2.66 nmol m^2 s^1. The nighttime (2004 h LST) hourly median values range from 1.01 to +1.17 nmol m^2 s^1. Their deviations from zero are not statistically significant at a $p<0.05$ probability level. Solar radiation was identified as the primary driver of CO exchange. This study also demonstrates the advantages and challenges of tall-tower EC flux measurements.”

Minor comments

Line 30: Please use the abbreviation CO, as it has already been introduced earlier in the text. Please ensure consistent usage throughout the manuscript.

In most places, we have replaced the term “carbon monoxide” with “CO.” We have retained the full term only in the Section titles and, to some extent, in the Introduction, where it seemed necessary for stylistic reasons.

Line 40: Please use the abbreviation CO_2, as it has already been introduced earlier in the text. Please ensure consistent usage throughout the manuscript.

Corrected.

Line 57-58: The sentence appears incomplete. Please revise and provide appropriate references to support the statement.

The sentence has been reformulated for clarity. Reference is provided. Lines 57-60:

"The CO-consuming microbial activity may depend on the organic fraction of the soil, porosity, temperature, water content, etc., while the production rate of carbon monoxide via thermal- or photodegradation of the living or dead organic matter may depend on plant species (Bruhn et al., 2013)."

Line 60: Bruhl -> Bruhn

Thank you, corrected.

Line 60–61: If referring to Bruhn et al. (2013), it should be clarified that their results indicate that grassland acts as a net CO sink primarily under dark conditions.

We have added this additional clarification in the revised text. Lines 60-62:

"Bruhn et al. (2013) conducted a 2-month survey over a grassland in Denmark using UV-transparent Plexiglas chambers and found that the grassland was a net sink in darkness and a net source in natural sunshine."

Line 63–64: The statement "Cowan et al. (2018) over a grazed grassland in Scotland indicated net emission from the soil-vegetation system, not supporting the net soil sink hypothesis" may be misleading. Even if the ecosystem shows net CO emission, the soil itself can still act as a net sink, with emissions originating from vegetation or litter.

We have reworded and expanded the sentence to better reflect the statement from the referenced article. Lines 63-66:

"However, the 20-month-long survey of Cowan et al. (2018) over a grazed grassland in Scotland indicated net emission from the soil-vegetation system. The nighttime fluxes were around zero. The measurements did not reflect a net soil sink, but the EC system used did not allow the separation of the fluxes from the vegetation and from the soil."

Line 64 and 67: Lassonen –> Laasonen

Corrected.

Line 105-107: I suggest reconsidering the wording here. Emphasizing the limitations of the measurement setup at the beginning of the manuscript weakens the overall presentation. I recommend focusing first on the study objectives and discussing these limitations later in the Methods or Discussion.

We agree with the Referee, and now mention the limitations of the study, the lack of certain measurements in the Auxiliary data section.

Line 130: Please include a legend in Figure 1 to describe the different land cover types, as this would improve the readability and interpretation of the figure.

The NÖSZTÉP land cover map distinguishes between 56 different types of land cover. Adding a detailed legend to the figure seems hardly feasible. As a compromise, we list the typical colors used for the land cover type mentioned in the paper. For interested readers, we have provided a reference to the publication where a detailed legend for the map can be found.

Lines 171–172: This statement would fit better in the Introduction than in the Methods section.

The paragraph has been reformulated to better fit into the auxiliary data section. Lines 175-178:

"UV radiation and soil water content (SWC) measurements are not available at the tower. These variables are mentioned in the literature as potential drivers of the CO exchange between the surface (vegetation+soil) and the atmosphere (Bruhn et al., 2013; Murphy et al., 2023; Laasonen et al., 2025); therefore, we turned to the ERA5 reanalysis dataset of the European Centre for Medium-Range Weather Forecasts (Copernicus Climate Change Service, 2022)."

Line 220: Please clarify how and why these criteria were selected.

These criteria are recommended for the applicability of the footprint model by the developers, and we applied them without revision (cf. Kljun et al. 2015). The sentence is reformulated to make it clearer (lines 264-266):

"In this study, following the recommendations for the application of the FFP model (Kljun et al., 2015), the model was used with the following restrictions: $20z_0 < z_m < he$, $-15.5 \leq z_m/L$, where he is the height of the entrainment layer. h_e is typically around 0.8 h (Holtslag and Nieuwstadt, 1986). We also applied $u_* \geq 0.2$ m s^{-1} to ensure turbulent forcing."

Line 234: Please clarify what is meant by "technical reasons."

"Technical reasons" cover instrument malfunction, data acquisition gaps, maintenance, etc. We now explain the term in the revised manuscript. Lines 278-280:

"Due to the potential decoupling of the EC system from the surface, we rejected these data points for surface flux estimation. In the case of an additional 16,499 hours, fluxes could not be calculated for technical reasons (power outage, instrument malfunction, data acquisition gaps, maintenance, etc.)."

Line 245: Please use the abbreviation EC for eddy covariance, as it has already been defined. Ensure consistent usage throughout the manuscript.

Corrected and consistently used in the text.

Line 260: Please quantify how much higher the fluxes are from the village direction compared to agricultural/forest areas.

Because each pixel of the footprint-weighted flux map has a different value, a quantitative comparison of the village and the vegetation-covered sectors is not possible. Generally, in winter the fluxes from a grid cell (pixel) in the village sector are an order of magnitude higher than those in the vegetation-dominated sector. In summer, there are regions where the footprint-weighted flux is a small negative value reflecting the overall low fluxes and their uncertainty.

Lines 278–287: Please move this content to the Methods section

This paragraph has been moved to the Measurements and methods section (Section 2.7 Flux map, and separation of populated and (quasi)natural area).

Line 328: Please clarify that this sentence refers to fluxes from quasi-natural areas

The revised manuscript clearly states that Fig. 6 shows the emissions over the (quasi)natural grid cells. Lines 385-386:

"Figure 6 shows the mean diurnal variation of net CO exchange over the (quasi)natural grid cells for each month based on the converged data."

Line 348: Please add some numbers to make results more quantitative.

Fluxes from the (quasi)natural grid cells are 1–8 % of those from the populated grid cells. The numerical values have been added to the text of the revised manuscript. Lines 397-398:

"Fluxes from the (quasi)natural grid cells are only a few percent (1-8 %) of those from the populated grid cells (Fig. 5)."

Line 360: The correlation between CO flux and solar radiation varies seasonally (stronger in July–December than in April–May). Could the authors provide an explanation for this pattern?

This is correct, but at the moment, we have no explanation for this phenomenon.

Line 380: You could compare your results with CO fluxes reported from other tall-tower studies. Are the observed emissions relatively higher or lower?

To the best of our knowledge, our measurements are unique. No one else has yet conducted similar measurements with which we could compare our results. The tall-tower carbon monoxide measurements cited in the Introduction of our paper were conducted specifically in urban environments (London, Paris, Zurich, Munich), and the emission origin differs for these environments. The emissions are hence not directly comparable.

Lines 393–394: "Our measurement recorded 15.2 nmol m^{-2} s^{-1} CO emissions for May-September on average, of which 13.7 nmol m^{-2} s^{-1} may be attributed to the transport sector." It is not clear how these values are derived from the measurements. Please clarify the methodology and assumptions used to obtain these numbers.

According to our measurements, the average emission over the populated grid cells in the period of May-September is 15.2 nmol m^{-2} s^{-1} (Fig. 5). According to the EDGAR database, the residential emissions in summer are only about 10 %, i.e., ~1.5 nmol m^{-2} s^{-1}. Deducting the estimated residential emissions from the measured emissions, we obtained a value (13.7 nmol m^{-2} s^{-1}) for the transport sector, assuming that in summer, these two sectors account for the total emissions. Forest fires and open biomass burning in agricultural areas did not occur during the measurement period in the footprint area according to satellite observations and personal field experience. Lines 457-466:

"Our measurement recorded 15.2 nmol m^{-2} s^{-1} CO emissions for May-September (non-heating season) on average, of which 13.7 nmol m^{-2} s^{-1} may be attributed to the transport sector (90 % of the total). The populated grid cells for which this value was calculated cover 12.8 % of the EDGAR grid cell (84.3 km^2) including the tower. Considering the summer value to be valid for the entire year, the total CO emissions of transport origin are 131 Mg yr^{-1} from the EDGAR grid cell. This value is twice as high as the EDGAR v8.1 average for the years 2015–2022 (60 Mg yr^{-1}).

The annual average flux over a populated grid cell is 48.1 nmol m^{-2} s^{-1} (see Fig. 5). Taking into account the total area of the populated grid cells within the EDGAR grid cell (84.3 km^2 × 0.128 = 10.8 km^2), and summing up the emissions for a year, we obtain that the total measured anthropogenic emissions expressed in mass of CO are around 460 Mg yr^{-1}, from which 330 Mg yr^{-1} can be attributed to residential activity, primarily heating."

Lines 398: Could you clarify how the estimate of total anthropogenic emissions (approximately 460 Mg yr^{-1}) was derived? Does "Mg yr^{-1}" refer to CO mass or to carbon (C)?

The average annual flux is 48.1 nmol m^{-2} s^{-1} over the populated grid cells (data in Fig. 5 of the manuscript). These cells cover 12.8 % of the EDGAR grid cell of 84.3 km^2, i. e., their area within the EDGAR grid cell is 10.8 km^2. Of this, the total anthropogenic emissions falling within this EDGAR grid cell are 48.1*10.8=519.5 mmol s^{-1}. This equates to $16.4 \cdot 10^6$ mol yr^{-1}. As EDGAR provides the emissions in mass of CO over the grid cell, $16.4 \cdot 10^6$ mol yr^{-1} corresponds to $459 \cdot 10^6$ g yr^{-1}. See the corrected text (lines 457-466) above.

Reference:

Finnigan, J.J. A Re-Evaluation of Long-Term Flux Measurement Techniques Part II: Coordinate Systems. Boundary-Layer Meteorology 113, 1–41 (2004). https://doi.org/10.1023/B:BOUN.0000037348.64252.45

References in the response to Reviewer

Barcza, Z.: Long term atmosphere/biosphere exchange of CO_2 in Hungary, Ph.D. Thesis, Eötvös Loránd University, Department of Meteorology, Budapest. (Supervisor: T. Weidinger, scientific adviser: L. Haszpra), http://nimbus.elte.hu/~bzoli/thesis/, 2001.

Barcza, Z., Kern, A., Davis, K. J., and Haszpra, L.: Analysis of the 21-years long carbon dioxide flux dataset from a Central European tall tower site, Agricultural and Forest Meteorology, 290, 108027, https://doi.org/10.1016/j.agrformet.2020.108027, 2020.

Barcza, Z., Kern, A., Haszpra, L., and Kljun, N.: Spatial representativeness of tall tower eddy covariance measurements using remote sensing and footprint analysis, Agricultural and Forest Meteorology, 149, 795-807, https://doi.org/10.1016/j.agrformet.2008.10.021, 2009.

Barcza, Z., Haszpra, L., Kondo, H., Saigusa, N., Yamamoto, S., and Bartholy, J.: Carbon exchange of grass in Hungary, Tellus, 55B, 187-196, 2003.

Haszpra, L., Hidy, D., Taligás, T., and Barcza, Z.: First results of tall tower based nitrous oxide flux monitoring over an agricultural region in Central Europe, Atmospheric Environment, 176, 240-251, https://doi.org/10.1016/j.atmosenv.2017.12.035, 2018.

Kroon, P. S., Hensen, A., Jonker, H. J. J., Ouwersloot, H. G., Vermeulen, A. T., and Bosveld, F. C.: Uncertainties in eddy covariance flux measurements assessed from CH_4 and N_2O observations, Agricultural and Forest Meteorology, 150, 806-816, https://doi.org/http://dx.doi.org/10.1016/j.agrformet.2009.08.008, 2010.

Lee, X.: On micrometeorological observations of surface-air exchange over tall vegetation, Agricultural and Forest Meteorology, 91, 39-49, https://doi.org/10.1016/S0168-1923(98)00071-9, 1998.

Vickers, D. and Mahrt, L.: Quality Control and Flux Sampling Problems for Tower and Aircraft Data, Journal of Atmospheric and Oceanic Technology, 14, 512-526, https://doi.org/10.1175/1520-0426(1997)014<0512:qcafsp>2.0.co;2, 1997.

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).

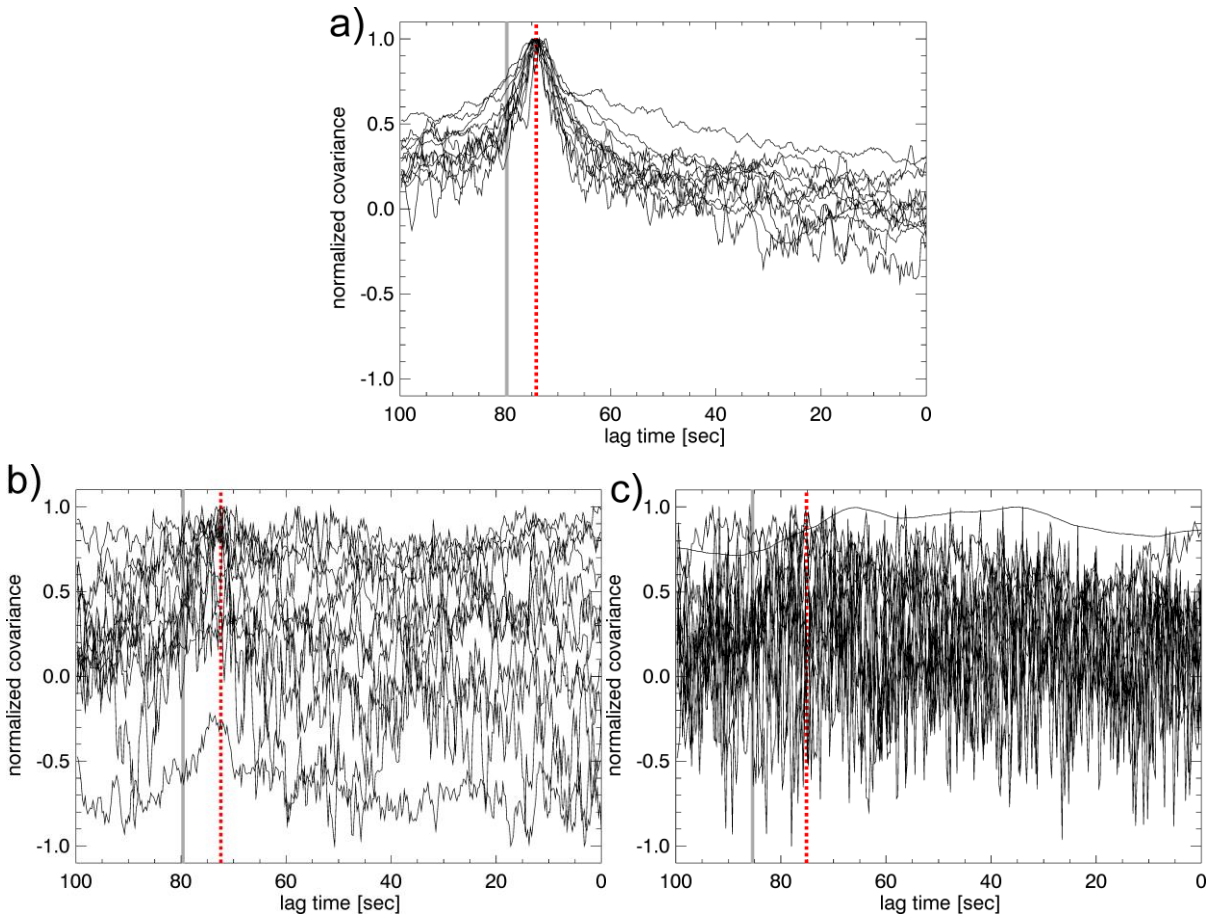

Figure R1 (Fig. S4 in Haszpra et al. (2018). Normalized lagged covariances for N_2O as functions of the lag time for three selected days. a) high flux case (15 July, 2016); b) low flux case (5 August, 2016); c) another low flux case (12 April, 2017). Vertical grey lines show the lag time for H_2O, while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

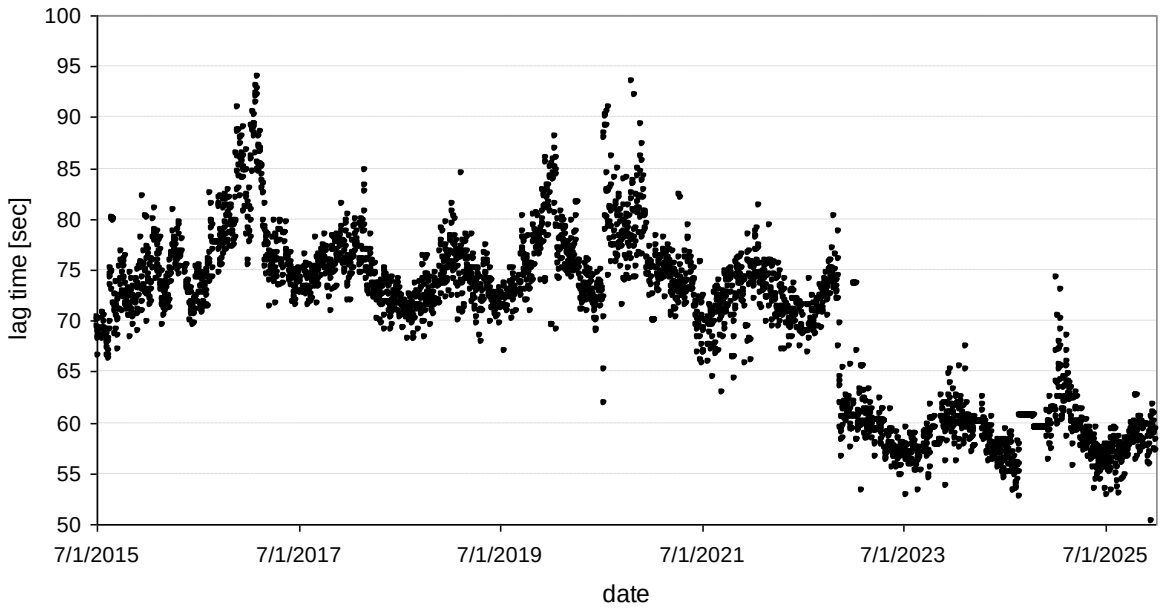

Figure R2. Daily lag time values (in seconds) during the entire study period.

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.

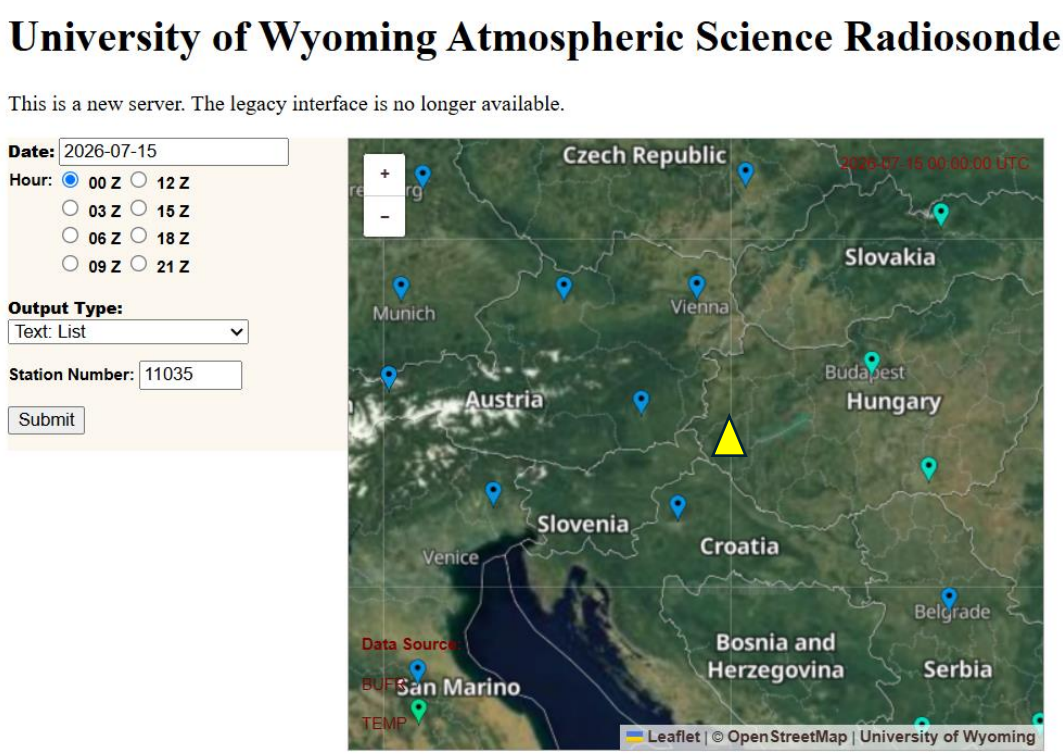

Figure R3: Radiosonde stations (map marker symbols) in the vicinity of the Hegyhátsál tall-tower monitoring station (yellow triangle). (https://weather.uwyo.edu/upperair/sounding.shtml)

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.

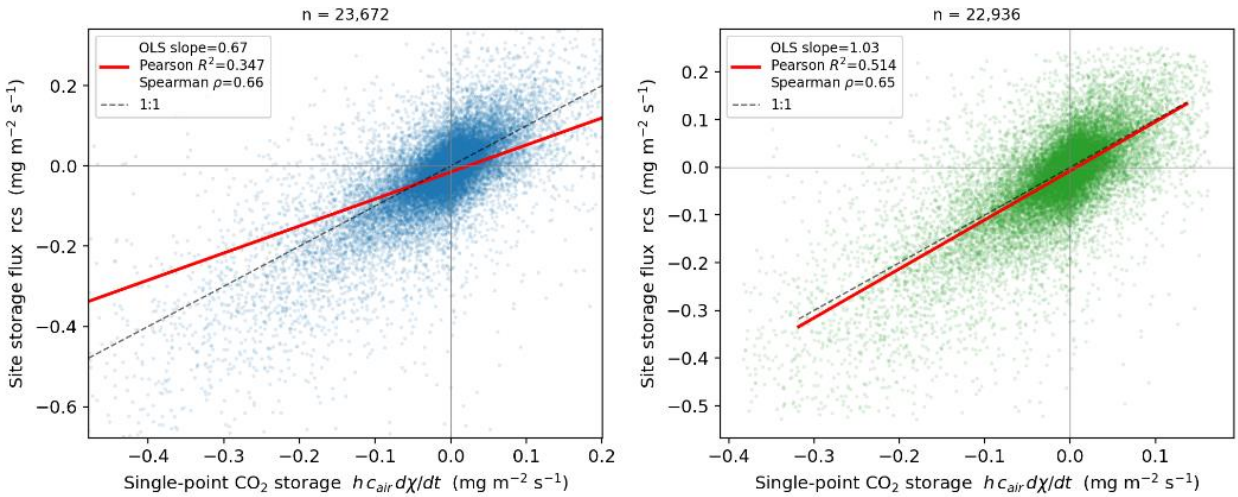

Figure R3. Single-point CO_2 storage vs. the operational storage flux calculated from the real CO_2 profile changes. Left: physical threshold + centered difference. Right: additional restrictions, +1–99 % truncation of data (R^2=0.51, slope 1.03 ≈ 1:1). OLS means ordinary least squares fit. n indicated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

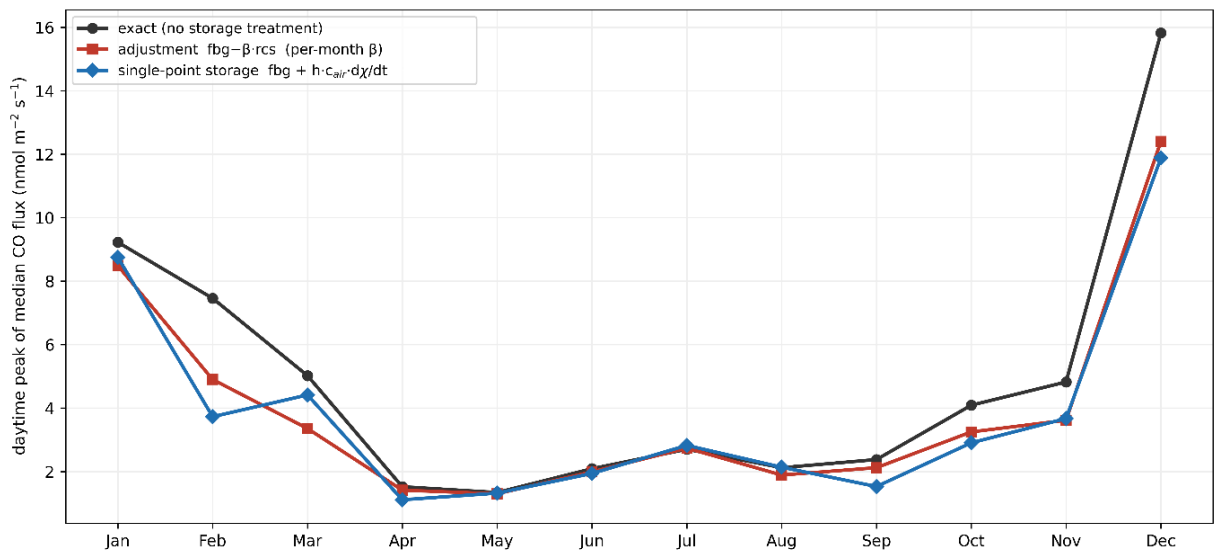

Figure. R4. Monthly variation of the CO peak flux under each storage treatment. The single-point storage (blue) tracks the β·rcs regression adjustment (red), cutting the peak in cold/high-emission months and leaving summer unchanged. 'exact' means the data in the original manuscript.

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 β·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 β·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.

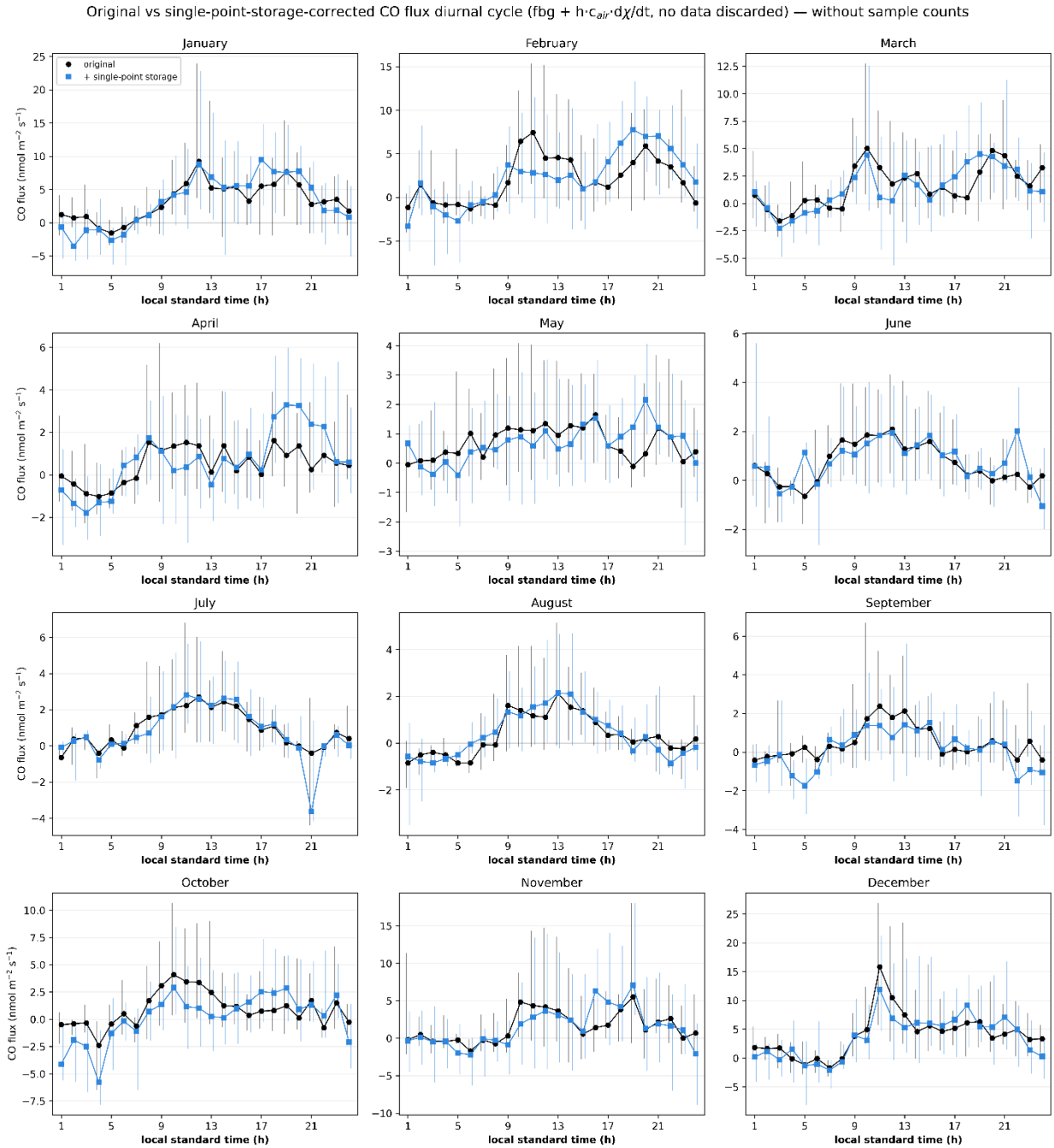

Figure R5. Same as Fig. 6 in the original manuscript (Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data. Whiskers indicate the lower and upper quartiles.) but the single-point storage-corrected CO fluxes are also indicated.

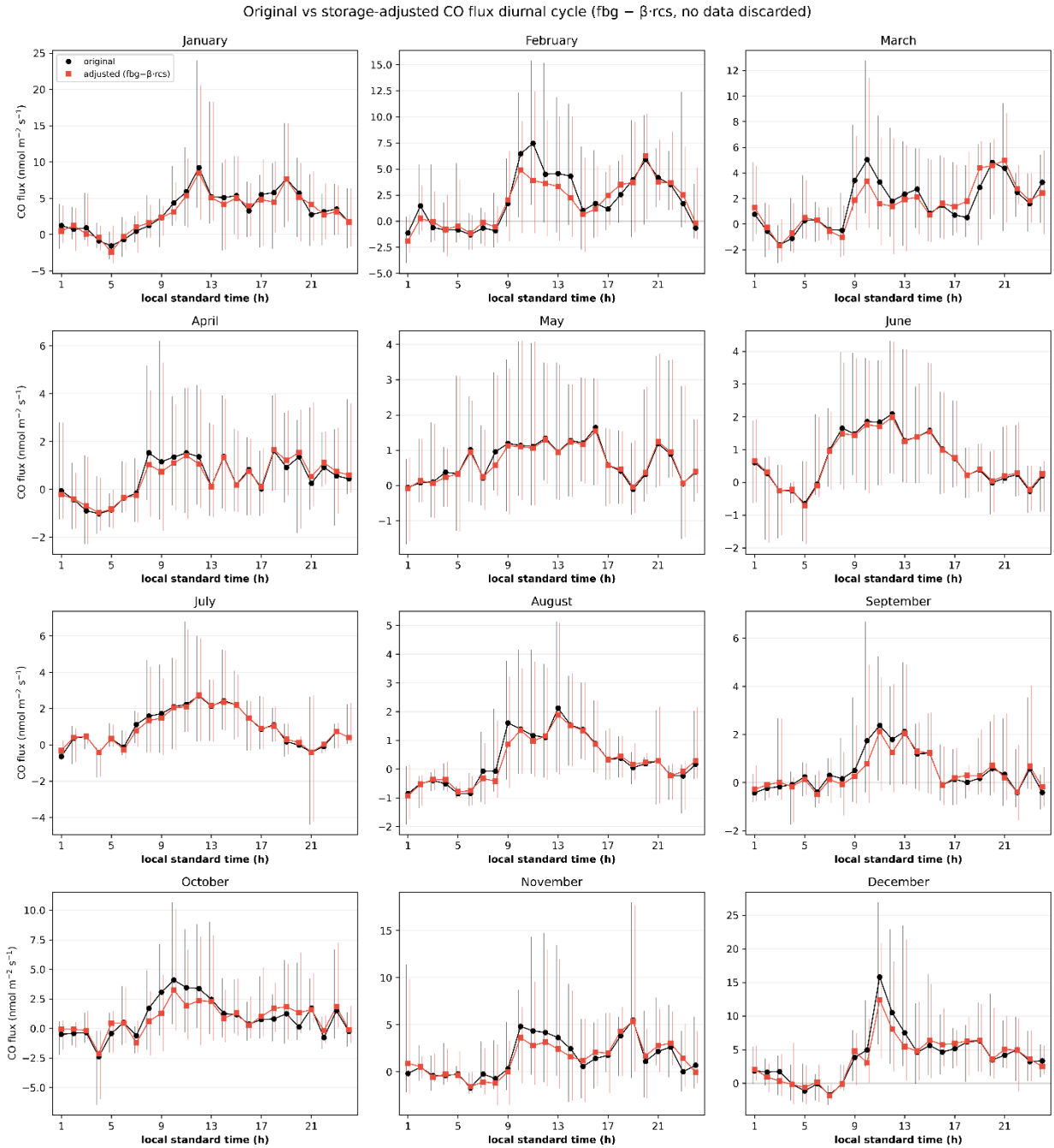

Figure R6. Same as Fig. 6 in the original manuscript (Mean diurnal variation of net CO exchange in the (quasi)natural grid cells (2015-2025) based on the converged data. Whiskers indicate the lower and upper quartiles.) but the single point storage corrected CO fluxes are also indicated.

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.”

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